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**DISAGGREGATING CIVIL CONFLICT:  
THEORETICAL, METHODOLOGICAL AND  
EMPIRICAL CONTRIBUTIONS**

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# Abstract

Civil conflict has been the predominate form of large-scale violence since the second half of the 20<sup>th</sup> century. The escalation of the conflict in Syria or the resurgence of the insurgency in Iraq, however, took most practitioners and academics by surprise. This underscores the lack of a deep understanding of the roots and mechanisms of civil conflict—a critical prerequisite for active and effective conflict prevention. This dissertation aims to close this gap by focusing on endogenous drivers of conflict, a focus that sets it apart from country-level analysis which typically rely on structural factors. The key conceptual motivation is that while structural determinants may set the stage for conflict, endogenous (feedback) mechanisms often determine how it plays out. Analyzing data from conflicts in Jerusalem and Iraq, this dissertation specifically addresses three central questions: First, why does intergroup contact in some circumstances exacerbate but in others mitigate violence? Second, what is the role of civilians in conflict dynamics? Are they merely bystanders or actually help shape the conflict dynamics we observe? And third, how does the scale of violence affect subsequent conflict dynamics? Addressing these questions, this dissertation builds on and contributes to a growing literature on disaggregate dynamics of civil conflict. It also harnesses the increasing availability of disaggregate conflict event data, making it possible to study conflict dynamics in more detail. This conceptual and empirical focus on smaller units of analysis, in turn, requires the development of new methodology particularly suited to analyze these data. Besides the theoretical focus on endogenous conflict processes, this dissertation, thus, also has a methodological focus. It develops new or refines existing techniques for the analysis of disaggregate conflict data. It further draws attention to data biases. These methodological contributions are, in fact, a critical prerequisite for clean inference in disaggregate settings. The studies of the conflict in Jerusalem and Iraq then empirically underscore the importance of endogenous conflict mechanisms showing that contact, civilian agency and the scale of attacks affect the trajectory of civil conflicts. Moreover, they clarify the conditions under which they deter or incite future violence and reveal the strength of these effects. This dissertation carefully places the studies in the more general context of the conceptual and theoretical framework of disaggregate research on civil conflict and highlights attendant policy implications.



# Zusammenfassung

Seit der zweiten Hälfte des 20. Jahrhunderts stellen gewaltsame innerstaatliche Konflikte die vorherrschende Form von extremer Gewaltanwendung dar. Die Eskalation des Konflikts in Syrien oder das Wiedererstarken des bewaffneten Aufstands im Irak kam jedoch für viele Fachleute und Wissenschaftler überraschend. Dies deutet auf ein mangelhaftes tiefergehendes Verständnis der Ursachen und Mechanismen innerstaatlicher Konflikte hin—eine entscheidende Voraussetzung für aktive und effektive Konfliktprävention. Diese Doktorarbeit zieht darauf ab diese Forschungslücke zu schliessen, indem sie sich auf endogene Konfliktprozesse konzentriert. Mit diesem Schwerpunkt hebt sich die Arbeit klar von Studien auf Länderebene ab, die typischerweise strukturelle Faktoren untersuchen. Die entscheidende konzeptuelle Motivation für den hier gewählten Schwerpunkt ist, dass der tatsächliche Ausgang von Konflikten häufig von endogenen (Verstärkungs-) Prozessen bestimmt wird, während strukturelle Faktoren die Voraussetzungen für Konflikte schaffen. Unter Verwendung von Daten zu Konflikten in Jerusalem und Irak konzentriert sich diese Arbeit dabei auf drei zentrale Fragen: Erstens, warum führt Kontakt zwischen Gruppen unter manchen Umständen zu mehr unter anderen zu weniger Gewalt? Zweitens, was ist die Rolle von Zivilisten in innerstaatlichen Konflikten? Sind sie lediglich unbeteiligte Zuschauer oder beeinflussen sie die Konfliktodynamik, die wir beobachten? Und drittens, wie beeinflusst das Ausmass von Gewalt die nachfolgende Konfliktodynamik? In der Beantwortung dieser Fragen baut diese Dissertation auf einer wachsenden Literatur zur detaillierten Dynamik innerstaatlicher Konflikte auf. Die Arbeit macht sich zudem die zunehmende Verfügbarkeit von hoch aufgelösten Daten zu Konfliktereignissen zu Nutze, Daten die die detaillierte Untersuchung solcher Konfliktprozesse erst ermöglichen. Dieser konzeptuelle und empirische Schwerpunkt auf kleineren Beobachtungseinheiten wiederum erfordert die Entwicklung von neuen, für die Analyse dieser Daten besonders geeigneten Methoden. Diese Dissertation hat daher neben dem theoretischen Schwerpunkt auf endogenen Konfliktprozessen auch einen methodologischen Schwerpunkt, in dem sie neue Techniken für die Analyse von hoch aufgelösten Konfliktdaten entwickelt bzw. bestehende Techniken verfeinert. Weiterhin wird die Bedeutung von verzerrten Datenerhebungen herausgearbeitet. Diese methodologischen Beiträge sind in der Tat eine kritische Voraussetzung um klare Rückschlüsse aus hochaufgelösten Daten ziehen zu können. Die Analysen der Konflikte in Jerusalem und im Irak heben empirisch die Bedeutung von endogenen Konfliktmechanismen hervor in dem sie zeigen, dass Kontakt zwischen Gruppen, die Handlungen von Zivilisten und das Ausmass von Gewalt den Ausgang von Konflikten massgeblich beeinflussen. Die Studien klären zudem unter welchen Bedingungen diese Mechanisms zu mehr bzw. weniger Gewalt führen und geben Auskunft darüber wie stark diese Effekte sind. Die Arbeit ordnet diese Studien

## **Abstract**

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in den breiteren konzeptuellen und theoretischen Kontext bestehender detaillierter Studien zu innerstaatlichen Konflikten ein und hebt sicherheitspolitische Konsequenzen hervor.

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# 1 Introduction

In the second half of the 20<sup>th</sup> century, civil conflict replaced international war as the most dominant form of large-scale violence (Fearon & Laitin, 2003). The consequences of civil violence are extreme with massive destruction to the economy, severe damage to society and the accompanying loss of civilian life. In the past decade, civil conflict has been highly visible in Iraq and Afghanistan, due in no small measure to the notable involvement of Western nations. At the same time, seemingly intractable conflicts such as the one in Israel and Palestine show no sign of abatement. Recent developments in the Middle East and North Africa—for example, the war in Libya, the ongoing conflict in Syria, and the more recent resurgence of violence in Iraq—underscore the prevalence of civil conflict at the start of the 21<sup>st</sup> century.

From the perspective of both policy makers and practitioners, a more detailed understanding of the mechanisms and processes that characterize civil conflict is of critical importance. For it is only if we understand civil conflict, if we are able to quantitatively replicate its characteristics, events and outcomes, that we can ultimately make informed policy decisions. In other words, a limited understanding of past and ongoing conflicts severely constrains our ability to predict future developments. This, in turn, makes active and effective conflict prevention very difficult. The humanitarian tragedies in Syria, the plight of its refugees in neighboring countries, the sectarian violence in Sudan, the resurgence of the insurgency in Iraq—all took most practitioners and academics by surprise. Thus, while civil conflict clearly remains the predominant form of large-scale violence in our time, we appear to lack a deep systematic understanding of its roots and mechanisms.

This dissertation aims to close this gap in the understanding of the roots and mechanisms of civil conflict by focusing on endogenous conflict drivers, a focus that sets it apart from country-level analyses which typically rely on structural factors. The key conceptual motivation is that while structural determinants may set the stage for conflict, it is endogenous (feedback) mechanisms that often determine how it plays out. In other words, a too narrow focus on the proximate conditions under which civil conflict emerges leads us to underestimate the ways in which prior events shape current conflict trajectories. For example, in the context of the Israeli-Palestinian

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conflict or the war in Iraq, “tit-for-tat” dynamics have been shown to be an important endogenous driver of conflict dynamics (Haushofer et al., 2010; Linke et al., 2012). To predict conflicts and their outcomes, it is crucial to know the conditions under which such feedback mechanisms are active.

The studies in this dissertation address three central questions that pertain to endogenous conflict dynamics: First, why does intergroup contact in some circumstances exacerbate but in others mitigate violence? Second, what is the role of civilians in conflict dynamics? Are they merely bystanders or actually help shape the conflict dynamics we observe? And third, how does the scale of violence affect subsequent conflict dynamics?

These mechanisms feature prominently in the literature on civil conflict. Yet empirical evidence is often either contradictory—in particular regarding the role of contact for violence—or insufficient. Especially country-level research designs often suffer from an apparent mismatch between the mechanisms tested and the data used to test them (Kalyvas, 2008, 398). The increasing availability of detailed, geocoded datasets in recent years,<sup>1</sup> however, has made it possible to study the complex interactions between actors in space and time while at the same time accounting for actor group dynamics. In fact, a growing quantitative literature on disaggregate dynamics of civil conflict has already taken important steps towards a more detailed quantitative understanding of conflict processes (Blattman & Miguel, 2010; Cederman & Gleditsch, 2009; Donnay et al., 2014)

Beside the theoretical focus on endogenous conflict processes, this dissertation also has a methodological focus. It develops new or refines existing techniques for the analysis of disaggregate conflict data. It also analyzes the effect of data bias on inferences drawn from these data. These methodological contributions are a critical prerequisite for “clean” inference in the disaggregate settings considered and thus essential to the substantive analyses.

It is important to emphasize that theoretical questions, methodological approaches and empirical applications as they pertain to disaggregate conflict data are intrinsically related. The specific choice of unit of analysis, for example, is usually determined by a particular research question. This choice is often complicated by the fact that coding or identification of actors may vary over time and among regions. Without proper theoretical reasoning, attention to potential issues of data collection and quality, and careful attention to the choice of appropriate methodology, disaggregate analyses are thus just as vulnerable to methodological issues as country-level research designs.

This dissertation tackles all three issues head on. In the following sections I first give an overview of the substantive contributions. The next section then places them into the more general context of the conceptual and theoretical framework of disaggregate research on civil conflict. In the last two sections I focus on the methodological contributions, both with regard to methods of analysis and bias in event data.

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<sup>1</sup>See, for example, ACLED (Raleigh & Hegre, 2009) or UCDP GED (Sundberg et al., 2010)

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The dissertation is a cumulative collection of five separate studies, one published as a book chapter and four corresponding to journal articles. Chapter 2 is an edited version of: Karsten Donnay, Elena Gadjanova and Ravi Bhavnani. (2014). “Disaggregating Conflict by Actors, Time, and Location.” in David A. Backer, Paul K. Huth, and Jonathan Wilkenfeld (eds.) *Peace and Conflict 2014* (Paradigm Publishers). I substantively contributed to the writing of this book chapter and generated the empirical illustration. The study in Chapter 3 was written in collaboration with researchers from the Graduate Institute in Geneva and the Hebrew University in Jerusalem. It appears as: Ravi Bhavnani, Karsten Donnay, Dan Miodownik, Maayan Mor and Dirk Helbing. (2014). “Group Segregation and Urban Violence.” *American Journal of Political Science* 58(1): 226–245. I jointly developed the model with Ravi Bhavnani and Dan Miodownik, implemented the computational framework for the evidence-driven modeling of violence, generated all results and figures and substantively contributed to all aspects of the writing of the article.

The third project presented in Chapter 4 is the result of a collaboration with Sebastian Schutte (formerly ETH Zürich). It was recently published as: Sebastian Schutte and Karsten Donnay. (2014). “Matched wake analysis: Finding causal relationships in spatiotemporal event data.” *Political Geography* 41: 1–10. The development of the methodology, the programming of the corresponding *R* package, the empirical analysis and writing of the article were all done in close collaboration with Sebastian and I have substantively contributed to every aspect of the work. The study in Chapter 5 was undertaken in collaboration with Vladimir Filimonov (also ETH Zürich) and will appear later this year in *EPJ Data Science*. Together with Vladimir I conceived and designed the study and wrote the manuscript. I also prepared and coded the data that Vladimir subsequently analyzed. The fifth and last study presented in Chapter 6 is a single-authored article. I designed the study, performed the analysis and wrote the manuscript. The study is submission-ready and will be sent out for review to a leading political science journal this summer. The four chapters in the appendix correspond to the supplementary or supporting information of the articles in Chapters 3 to 6 respectively. The references for all works cited are integrated into one summary bibliography at the end of this dissertation.

## **Substantive contributions**

*What is the relationship between intergroup contact and violence? How is this relationship affected by group relations?* This discussion has featured prominently in the literature on intergroup conflict with studies lending support to two competing perspectives. The first argues that intermixed group settlement patterns reduce violence, as more frequent interactions enable rivals to overcome their prejudices towards each other and thus become more tolerant, while the second argues that group segregation more effectively reduces violence, given less frequent contact and fewer possibilities for violent encounters.<sup>2</sup> The study in Chapter 3 shows that, in fact, both perspectives can be reconciled, if one acknowledges that intergroup tensions effectively mitigate the effect of spatial proximity, i.e., only in situations where tensions between groups are high does contact lead to violence. The detailed quantitative analysis of violence in Jerusalem

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<sup>2</sup>Please refer to Section 3.1 for a detailed theoretical and empirical overview of both perspectives.

## Chapter 1. Introduction

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lends empirical support to this theoretical argument: the degrees of intergroup tensions with which the model best replicates the violence patterns in Jerusalem very accurately reflect the intergroup relations in the city. In the context of Jerusalem, the framework can be used to directly evaluate a number of concrete future scenarios. The finding that intergroup relations effectively mitigate the effect of contact on violence, however, has broader implications beyond this specific case and helps to clarify the relationship of intergroup contact and violence.

*What is the role of civilians in conflict dynamics? Are they merely bystanders or actually help shape the conflict dynamics we observe?* The empirical analysis of the conflict dynamics in Iraq in Chapter 4 suggests that civilians do indeed actively take sides: collaboration with security forces significantly increases if civilians are targeted by insurgent attacks. The disaggregate analysis, however, also shows that this effect is only noticeable in the direct vicinity of attacks and with a delay of about 1-2 weeks. Note that in contrast to the prior work of Condra & Shapiro (2012) on Iraq, the study more directly tests the impact of insurgent violence on civilian agency: first, it explicitly uses instances of collaboration with US forces as the dependent variable and, second, the causal inference design provides a much more direct and robust measure of the causal relationship. Researchers and practitioners emphasize the importance of population centric warfare in countering insurgencies, i.e., the necessity to consider the impact of military measures—especially indiscriminate violence—on the civilian population (DoS, 2009; Kalyvas, 2006; Lyall, 2009). The finding that civilians also actively and strategically respond to insurgent attacks suggests that insurgents operate under similar constraints, with attendant consequences for both insurgent and counterinsurgent tactics.

*How does the scale of violence affect subsequent conflict dynamics? Does large-scale violence tend to incite or deter subsequent attacks? And how is it related to small-scale violence?* This research is motivated by the intuition that simply knowing where and when violent incidents occur is not always sufficient to gauge their effect. Instead, it is often just as relevant how strategic and how severe they are—studies analyzing sub-national conflict have, in particular, analyzed the effect of indiscriminate as compared to selective violence (Bhavnani et al., 2011; Kalyvas, 2006; Lyall, 2009). The analysis of large- and small-scale violence in Iraq in Chapter 6 suggests that strategic, large-scale violence tends to significantly increase subsequent levels of violence. This effect both tends to be stronger and has a larger range than the effect of small-scale attacks. The analysis, however, also reveals that levels of small- and large-scale violence in Iraq are intricately related. Disaggregating by conflict periods and provinces of Iraq further shows that the spatiotemporal dynamics of small- and large-scale violence vary strongly across the conflict. From a theoretical point of view, the study highlights the need to explicitly consider event severity in both theoretical frameworks and empirical analyses. The statistical regularities in the timing and location of violence—in particular of large-scale attacks—identified here can further inform practitioners and help guide policy decisions.

Together the three studies underscore the importance of endogenous conflict mechanisms. Contact, civilian agency and the scale of violent attacks are all shown to affect the trajectory of civil conflicts. Moreover, the studies clarify the *conditions* under which they deter or incite future

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violence and reveal the *strength* of these effects: contact tends to lead to violence if intergroup tensions are high; civilian collaboration with security forces, which is detrimental for insurgent activities, increases when civilians themselves become targets of violence; and large-scale attacks more strongly incite subsequent violence than small-scale attacks.

### **Disaggregating the dynamics of civil conflict**

A key criticism leveled at aggregate, country-level analysis of civil conflict is that, in many cases, theories of civil war are not tested at the level at which they are theorized to operate (Eck, 2012; Kalyvas, 2008). While cross national studies find no conclusive evidence that ethnic fragmentation of states is related to civil war onset (Fearon & Laitin, 2003), disaggregated analyses show that exclusion from power in particular constellations of ethnic groups and governments increases the probability of conflict (Cederman et al., 2009). In fact, recent research emphasizes that there is a multitude of relevant factors influencing the dynamics of civil conflict that are only revealed when considering sub-national units of analysis.<sup>3</sup> As Cederman & Gleditsch (2009) put it, “These distinctions and variations are disregarded in studies that lump together all forms of civil war and focus on country-level characteristics” (Cederman & Gleditsch, 2009, 490).

Chapter 2 reviews the current state of disaggregate research into civil conflict with an emphasis on the three most prominent distinctions made in the literature: disaggregation by actors, time and location. It also outlines substantive implications of this emerging research agenda for policy makers and practitioners, placing a particular focus on a number of key issues addressed in disaggregate analyses: participation of individuals in civil conflict, victimization of civilians, conflict-driven migration and segregation, the effect of state policies on violence, and implications for post-conflict reconstruction (see Section 2.4).

The first section of Chapter 2 focuses on studies disaggregating civil conflict dynamics by relevant actors. This line of research specifically highlights the importance of actors’ heterogeneous characteristics, beliefs, or interests for the onset and dynamics of civil conflict. This has led to novel insights into why and under which conditions civilians may become targets of violence (Section 2.1.1), and how rebel group dynamics affect conflict dynamics (Section 2.1.2). It has also helped to clarify the relationship between inequality and violence (Section 2.1.3). The latter research, in particular, challenges greed and opportunity as prevalent explanations for civil conflict, in turn, highlighting the importance of grievances such as income inequalities based on ethnicity.

We fully recognize the critical importance of actors’ characteristics, beliefs and interests, for how civil conflict unfolds in our own research. The research on the conflict in Jerusalem in Chapter 3 disaggregates by relevant actor groups—in this case Palestinian, Ultra-Orthodox Jews, Secular Jews and (Israeli) security forces. Taking a group-level perspective here is of critical importance.

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<sup>3</sup>Beardsley & McQuinn (2009); Buhaug et al. (2009); Cunningham et al. (2009); Hegre et al. (2009); Weidmann (2009).

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In fact, our study shows that the basic tenet of contact theory—the question of whether contact between members of different groups leads to violence or not—effectively depends on intergroup relations. We similarly disaggregate by actor group (insurgents, security forces, and civilians) in Chapter 4 and demonstrate that civilians are more than a neutral third party in insurgent conflict.

The second section of Chapter 2 highlights the contributions of studies that disaggregate conflict dynamics in time, i.e., studies that depart from country-year research designs and explicitly consider monthly, weekly, daily or even hourly time series of events. Considering events at the temporal resolution at which conflict dynamics unfold, is a critical step in closing the gap between concepts and data (Kalyvas, 2008): while country-year research designs often invoke conflict mechanisms that implicitly rely on an event based, day-to-day logic, they can usually not be empirically tested in aggregate data. In fact, much of the dynamics of civil conflict has significant variations on time-scales much shorter than years. This includes violence mechanisms such as reactive or “tit-for-tat” dynamics found across a range of conflicts (Haushofer et al., 2010; Linke et al., 2012), for which the temporal ordering of events critically matters. The chapter particularly highlights two contributions that have shed light on civilian agency in the conflict in Iraq (Section 2.2.1) and used crowd-sourced data for a temporally very highly resolved analysis of reactive dynamics in the Israeli-Palestinian conflict (Section 2.2.2).

In our own research we subscribe to the notion that temporally disaggregate data is a key requirement for the empirical analysis of civil conflict. The studies throughout this dissertation therefore rely on temporally highly resolved data with at least a resolution of days—data on Iraq even reports timestamps with a resolution of minutes. This temporal disaggregation reveals that the causal effect of insurgent violence on civilian loyalties in Iraq (Chapter 4) and the relationship of small- and large-scale violence (Chapter 6) is explicitly time dependent. In both studies, high temporal resolution is of critical importance because it allows to accurately determine temporal ordering of events—a critical prerequisite to infer causal relationships (Chapter 4) and systematic co-occurrence (Chapter 6) of subsequent incidents.

The third section of Chapter 2 reviews research that disaggregates conflict dynamics in space. The key motivation to consider dynamics at sub-national spatial units of analysis—villages and cities, administrative units such as districts or provinces, but also exact geo-locations of incidents—is that, empirically, conflict dynamics typically vary significantly for different regions within a state. Kalyvas (2006), for example, prominently argues that the degree of territorial control critically matters for whether conflict parties rely on indiscriminate or on selective violence in a given region. The section reviews, in particular, two focal areas of research in more detail: reactive violence dynamics (Section 2.3.1) and the relationship between (spatial) segregation and violence (Section 2.3.2).

These two substantive areas of interest also prominently feature in this dissertation. In Chapters 4 and 6 we show that the endogenous violence mechanisms in Iraq explicitly depend on geographic distance. In Chapter 3, spatial disaggregation is central to the empirical study of the relationship between spatial segregation and intergroup violence in Jerusalem. Specifically, we rely on detailed

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empirical data on the spatial distribution of the population groups in the city to endogenize the degree of contact between them.

In addition to disaggregating by actors, time and location, the research in this dissertation also explicitly disaggregates conflict dynamics by the severity of violence. The analysis in Chapter 6 is motivated by and specifically builds on prior theoretical and empirical research that emphasizes the importance of the “scale” of violent events for the understanding of civil conflict dynamics. Typically, scale is defined by the type of attack—an air strike compared to a shooting attack, for example. In empirical data, however, this kind of information is often absent or incomplete. To complement categorization by type for such cases, the study introduces a theoretically-grounded statistical method that directly uses event severity, i.e., casualty counts, to robustly classify events into two broad event categories: small- and large-scale violence.

### **Statistical and computational techniques for disaggregate data**

Standard statistical techniques are based on the assumption that empirical observations are independent. In other words, it is assumed that the observed cases do not influence each other. Especially in the disaggregate settings we consider this assumption is obviously often violated. In fact, the substantive analyses in this dissertation explicitly focus on endogenous conflict processes. The conceptual and empirical focus on smaller units of analysis thus requires the development of new methodology particularly suited to robustly analyze detailed, disaggregate data on civil conflicts. Methods include a broad range of statistical and econometric techniques applied to disaggregate data (Linke et al., 2012), causal inference designs (Lyal, 2009) and field experiments (Humphreys & Weinstein, 2009), but also entail a novel emphasis on quantitative data-driven computational models of conflict (Bhavnani et al., 2014; Weidmann & Salehyan, 2013). Note that much of this research has significantly profited from a novel emphasis on Geographic Information Systems (GIS) to precisely code the locations of violence (Gleditsch & Weidmann, 2012).

In general, when moving to smaller temporal units of analysis—typically days instead of years—many statistical or econometric techniques may still be applicable. This is also usually the case when moving to smaller spatial units of analysis as long as the empirically relevant conflict dynamics vary at natural units of analysis (such as villages, cities, regions or provinces). In these cases, regressions, for example, can simply be run for district-day or district-week instead of country-year series (Condra & Shapiro, 2012). Similarly, causal inference designs relying on statistical matching have been successfully used for village-level conflict data (Lyal, 2009). In many cases, however, such natural spatial units of analysis are missing. This applies, in particular, to dynamics such as conflict diffusion (Schutte & Weidmann, 2011) or insurgent violence (see Chapter 4), which are not bound to specific spatial units.

A number of recent studies relies on artificial units of analysis—grid-cell months or days—to overcome this problem. These artificially binned data are then again suitable for econometric

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analysis (Buhaug, 2010; Linke et al., 2012). Analyzing conflict dynamics using arbitrary spatial bins, however, leads to two serious methodological problems widely described in the literature. Taking grid cells as spatial units of analysis artificially inflates the number of observations and may thus make even the smallest empirical effects statistically significant. In many cases it also generates many units with no observations, i.e., it artificially inflates “null” observations, and may thus systematically bias inferences.<sup>4</sup> In addition, already the very selection of artificial cell sizes drives spatial inference—a well-known problem in the geographic literature known as the “modifiable areal unit problem” (MAUP) (Openshaw, 1984).

The empirical studies in this dissertation strictly avoid the problems associated with artificial spatial units of analysis. In our statistical analyses we instead rely on methods that employ sliding spatial and temporal windows when comparing the location and timing of events. This methodological approach was first introduced for the spatial analysis of epidemics (Kulldorff, 1997), but has also been successfully employed to analyze diffusion of violence (Schutte & Weidmann, 2011). These methods generally rely on the use of geographic information systems (GIS) that allow to precisely code the coordinates of events. Note that GIS further greatly simplifies combining data on conflict with existing contextual data from other sources, including covariates like GDP, elevation, and population (Cederman & Gleditsch, 2009; Gleditsch & Weidmann, 2012).

Chapter 4 introduces a new method for causal inference in spatiotemporal event data, which has also recently been released as an *R* package.<sup>5</sup> In addition to sliding spatial and temporal windows, the method—called Matched Wake Analysis (MWA)—relies on statistical matching on contextual variables to avoid selection bias and thus guarantee clean causal inference (Iacus et al., 2012; Rubin, 1973). MWA allows to explicitly test the effect of one type of *treatment* intervention hypothesized to have a significant effect on the level of *dependent* events as compared to a second type of *control* intervention.

It is important to note that a technique such as MWA is most suited for the analysis of not only specific but also comparably rare interventions. In fact, we analyze in detail in Chapter 4 how estimates are systematically affected if observations overlap, i.e., if interventions cluster.<sup>6</sup> For the case of highly clustered, i.e., spatially and temporally dense data other methods may therefore be more suitable.<sup>7</sup> In our analysis of large- and small-scale violence in Iraq Chapter 6 we rely on the Knox clustering test, an elegant, non-parametric test that detects significant co-occurrence of events in space and time. Note that there is an explicit tradeoff in the ability to analyze dense event data and the statistical power of the analysis: in comparison to MWA we here “only” detect systematic spatiotemporal correlations and can thus—strictly speaking—only invoke Granger

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<sup>4</sup>Units with non-zero event counts effectively become “rare” events, which has to be explicitly account for (King & Zeng, 2001).

<sup>5</sup><http://cran.r-project.org/package=mwa>

<sup>6</sup>This is a direct consequence of the stable unit value assumption (SUTVA) inherent to statistical matching that requires units to be uniquely affected by either control or treatment interventions.

<sup>7</sup>This is the case in Chapter 6 where the density and frequency of small-scale events, especially in the central regions of Iraq, is very high.

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causality (see also Linke et al. (2012)).

From a methodological point of view, disaggregate analyses of civil conflict also favor the use of detailed computational models, in particular agent-based approaches. Disaggregation by actors allows models to explicitly explore the link between the (micro-level) interactions of actors and the (macro-level) violence patterns we observe. Similarly, computational (agent-based) models can at the same time also be specified to explicitly incorporate spatial dimensions and generate simulated conflict trajectories. While simulation models are often only considered to be excellent testbeds to explore abstract causal mechanisms,<sup>8</sup> the availability of disaggregate conflict and contextual data allows us to bring them closer to concrete empirical cases.

There is a small but growing number of quantitative studies relying on (agent-based) simulation models to develop a more detailed understanding of the dynamics underlying civil violence. These studies analyze the interactions between civilians, rebels and state authorities (Epstein, 2002) and the likelihood of successfully suppressing insurgencies (Bennett, 2008). The study of Cioffi-Revilla & Rouleau (2010) investigates the trade-off and consequences of civilian support for insurgents compared to support for the government, while others include group dynamics in addition to individual level dynamics in their analysis of guerilla warfare (Doran, 2005) and analyze the influence of ethnic salience on the relationship between ethnic polarization as a proxy for ethnic mixing and violence (Bhavnani & Miodownik, 2009).

The specific mechanisms tested in such models are usually expected to be firmly theoretically grounded in the literature and empirically motivated. These models are then generally accepted as a complement to other formal modeling techniques (Cederman, 2001). Recent computational approaches, however, go significantly beyond this in establishing much closer connections to concrete empirical cases.<sup>9</sup> The idea is to not only validate the outcome of such models using empirical data but to actively calibrate them (Cederman, 2010). The studies by Schutte (2010) and Weidmann & Salehyan (2013) introduce two alternative systematic approaches to model calibration for agent-based models, one using machine learning techniques, the other employing a more heuristic optimization routine. In both approaches the model mechanisms that lead to the best representation of the empirical data are characterized by an optimized set of parameters, which are *post hoc* analyzed as to whether they are empirically meaningful—this guarantees a degree of model validity that goes beyond “generative sufficiency” (Epstein, 2006).

In the study on Jerusalem in Chapter 3 this concept of *evidence-driven* computational (agent-based) models is developed further. The new methodology, in particular, systematizes the use of empirical data in setting up realistic simulation models and further strengthens techniques for validation and calibration of models with empirical data. For the case of Jerusalem the model is seeded with detailed information about the topography of the city, the location of the relevant actor groups but also contextual information such as empirical in- and out-migration. It is then calibrated to best represent the empirical neighborhood-by-neighborhood violence patterns

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<sup>8</sup>For example, the famous models of Schelling (1971) or Axelrod (1984).

<sup>9</sup>For a detailed discussion see Cederman & Girardin (2007b).

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observed. The model's correspondence to empirical data is not only maximized for the spatial dynamics but also explicitly for the fraction of incidents perpetrated by each relevant actor group, guaranteeing maximal consistency between simulated and empirical violence patterns. The set of parameters that specify the model with the best agreement to empirical data are then shown to be consistent with the empirical situation in Jerusalem. This formal validation scheme is complemented by an extensive sensitivity analysis, in-sample prediction tests and the comparison to a statistical baseline model.

Detailed computational models necessarily sacrifice some external for internal validity. However, the clear advantage of a higher degree of internal and empirical validity is the ability to precisely test hypothesized mechanisms. These models can then also be used to generate simulated counterfactuals designed to answer important and policy-relevant *what if* questions (see Chapter 3). Of course, insights derived from these models may be not as readily transferable to other empirical cases where the detailed empirical situation differs. However, general mechanisms—such as the contact hypothesis we test in Chapter 3—can, in principle be tested across a range of cases using the exact same model, but seeding it with case-specific data.

Researchers using detailed computational models, however, must be very wary not to just produce models with high fit to empirical data and no empirical relevance. In Lim et al. (2007), for example, the link between segregation and violence is analyzed in former Yugoslavia and India using an agent-based simulation model seeded with census data. Their analysis suggests that rather than specific individual-level mechanisms, demographic characteristics determine locations of violence. In a detailed critique, Weidmann & Toft (2010) conclude that “all we learn from the results of the model is that it is possible to tune the model to achieve a high agreement, whereas in fact we would be interested in whether the identified patterns are of some generality”(Weidmann & Toft, 2010, 174). The central criticism leveled at Lim et al. (2007) concerns their lack of model validation: an out-of-sample analysis performed by Weidmann & Toft (2010) reveals that the mechanism of identifying locations of violence by demographic characteristics has no predictive power.

In our research on Jerusalem we carefully guard against such issues. First, our analysis builds on the well-established theoretical framework of contact theory, which is here generalized to explicitly consider how intergroup relations shape the relationship of contact and violence. Second, our evidence-driven model uses empirical data to both set up and calibrate the model. We further extensively validate our model results and test the model's predictive power. Taken together this ensures a high degree of both empirical validity and empirical relevance of our substantial analysis.

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## Addressing bias in detailed conflict event data

The disaggregate study of civil conflict would not be possible without a corresponding emphasis on the collection and coding of detailed conflict data. These data typically collect information on violent incidents including their location and timing but also their severity (typically as casualty counts), their type, perpetrator and victims, or any other relevant context information. Data collection for a single case or a select number of cases is typically done by individual scholars or small teams. In many cases, they conduct intensive field research (Ibáñez & Velasquez., 2009; Staniland, 2012), but also rely on existing data from surveys, data collected by NGOs, official statistics and newspaper reports (Bhavnani et al., 2011; Lyall, 2010). In comparison, large-scale institutional initiatives, such as the Uppsala Conflict Data Program's geo-referenced event dataset (UCDP GED) (Sundberg et al., 2010) and the Armed Conflict Location and Event Dataset (ACLED) (Raleigh & Hegre, 2009) typically focus on entire regions or continents. These initiatives rely on a much wider pool of coders but also use automatic coding from news-media reports to achieve maximal coverage. Within such programs coding procedures are generally standardized to maximize coding precision and minimize (human) error.

These conflict event data, however, have been found to be prone to bias (Eck, 2012). Biases identified in the literature can generally be grouped by whether they affect *if* incidents are reported or *how* they are reported. The location of incidents, for example, tends to affect the chance of an event being reported. If coding relies on news media sources or their local partners (NGOs, development agencies, etc.), whether an incident enters a dataset critically depends on their coverage and location. Remote locations tend to be systematically covered less than the capital or urban centers, thus resulting in a corresponding center-periphery bias (Raleigh, 2012). The same usually applies for data collected by the state, as permanent government presence (offices, police and military installations etc.) tends to be less developed in the periphery. This is particularly true for volatile states where the government's reach of power usually does not extend far beyond the capital or major population centers. Studies have also found that media-based data is prone to selective reporting of certain types of events (Earl et al., 2004; Oliver & Maney, 2000) as well as of larger compared to smaller events (McCarthy et al., 1996). With respect to *how* events are reported, previous research highlights, in particular, the role of ideological biases in reporting—this may be true for news-media coverage (Raleigh, 2012) but also for casualty reporting by the military (Rogers, 2010b). More generally, Davenport & Ball (2002) highlight how the interests of observers tend to directly affect how incidents are reported.

If disaggregate conflict event data are biased, this may seriously affect any inferences with regard to conflict dynamics and mechanisms, even for otherwise unbiased and flawless research designs. Unfortunately, the kind of systematic issues related to data collection—even leaving aside human error and coding accuracy—are very hard to identify and difficult to eliminate in the process of data collection. This is also true for the kind of institutionalized large-scale collection efforts mentioned before. *Post hoc* identification of potential biases in existing datasets is also extremely difficult since usually not more than one independently generated dataset exists, essentially making it impossible to infer any biases.

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In some cases, however, more than one independently collected dataset exists and biases can—at least comparatively—be analyzed. Focusing on the case of Iraq, we show in Chapter 5 that media-based and military reporting in this case do indeed substantially differ. We show that these differences are in line with the different nature of the data collection underlying the two different types of data. While there is not one strategy to mitigate data biases, our study suggests that researchers can at least actively address them. In particular, statistical tests can help—given the choice—to identify datasets that are more suitable than others for the analysis at hand. The awareness for issues of bias in disaggregate conflict data, however, to date is still very low: most studies neither analyze potential biases nor systematically test the robustness of their findings. With the growing availability of large and detailed conflict event datasets issues of data quality clearly have to be taken more seriously.

The studies in the following chapters do take issues of data quality seriously. In fact, the substantive analysis on Iraq in Chapters 4 and 6 exclusively rely on the dataset identified to be most suited for the kind of timing analyses performed there. And the study on Jerusalem in Chapter 3 relies on a wide range of data sources to consistently code the violence dynamics in Jerusalem.

The remainder of this dissertation is organized as follows. Chapter 2 reviews the current state of disaggregate research into civil conflict before we turn to the substantive studies in Chapters 3 to 6. It concludes with a brief discussion of key results and implications for future work.

## 2 Disaggregating Conflict by Actors, Time, and Location<sup>†</sup>

Disaggregated studies of conflict, which are increasingly common, provide fine-grained renderings of the relevant actors, timing, and location of events. These studies look beyond the country-year as the unit of analysis, in lieu of research designs that focus on individuals, households, or groups, the heterogeneous characteristics, beliefs, and interests of these actors, and resulting variation in attitudes, decision making, and behavior. The shift toward the micro level also permits a more nuanced analysis of conflicts, with explanations that account for changes over time and across spatial units—spanning the range from villages, neighborhoods, cities, subnational administrative units, states, and regions—in the incidence, intensity, and duration of events. The ability to specify and test causal mechanisms, and thereby address a characteristic limitation of more highly aggregated large-N studies, constitutes a noteworthy advancement in conflict research.

Yet disaggregated approaches are not without limitations. One involves the trade-off in sacrificing greater external validity for internal validity—when variation is explored at the subnational level, within a single country or even several countries, as opposed to cross-national studies that yield broadly applicable findings. Also, there are uncertainties about design, measurement, and analysis: What is the appropriate level of disaggregation? What should be measured? What is observable in practice? How can studies that select different units of analysis be compared, given the known problems with changing the number of units under study and the shape and size of those units? In what ways can different datasets on conflict be linked to each other and to data on other factors? How can challenges associated with analyzing disaggregated data be addressed? In particular, what are the strengths and weaknesses of statistical inference from disaggregated analysis?

This chapter takes stock of the emerging research track by providing an overview of notable recent work that disaggregates conflict by its constitutive actors and the timing and location of

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<sup>†</sup>This chapter is an edited version of the following book chapter: Karsten Donnay, Elena Gadjanova and Ravi Bhavnani. (2014). “Disaggregating Conflict by Actors, Time, and Location.” in David A. Backer, Paul K. Huth, and Jonathan Wilkenfeld (eds.) *Peace and Conflict 2014* (Paradigm Publishers). <http://www.paradigmpublishers.com/books/BookDetail.aspx?productID=367827>

events. We discuss select insights from these examples, why they challenge results from prior research or the conventional wisdom, and the associated implications for policy. Our concise review reveals a surge of rich context-specific research, which represents welcome progress, despite the rather limited communication across studies, the absence of data pooling, and the plethora of mixed findings.

### 2.1 Disaggregation by Actors

Micro-level research on conflict draws attention to actors who feature less prominently, if at all, in state-centered analyses that rely on country-year research designs (see Buhaug & Rød, 2006; Humphreys & Weinstein, 2006; Salehyan et al., 2011). By disaggregating agency, these studies explicitly take individuals (Annan et al., 2011; Bhavnani & Backer, 2000; Bosi & Della Porta, 2012; Florez-Morris, 2010), households (Bozzoli & Brück, 2009; Justino, 2009; Justino et al., 2013), and groups (Buhaug et al., 2009; Cederman et al., 2011b; Gubler & Selway, 2012; McCauley, 2013; Staniland, 2012) as units of analysis.

Representative studies account for actors' heterogeneous characteristics, beliefs, and interests, underscoring variation in their propensity to engage in violence (Bhavnani & Backer, 2000; Humphreys & Weinstein, 2008; Verwimp, 2006), join paramilitary groups (Bosi & Della Porta, 2012; Muldoon et al., 2008), and stay put or flee (Czaika & Kis-Katos, 2009; Steele, 2009). An added benefit is the impetus to identify mechanisms and emergent structures that shape the attitudes, decision making, and behavior of actors. Influences include: the link between ethnicity and conflict during counter-insurgency operations, due to the identity of soldiers conducting sweeps and their prior experience as insurgents (Lyal, 2010); individual decisions to migrate as a function of security considerations, police presence, and intimidation by rebels (Czaika & Kis-Katos, 2009); flight patterns determined by community characteristics and the salience of ascriptive cleavages during a war (Steele, 2009); and levels of violence against adversaries and civilians as determined by rewards and punishments used to foster intragroup cohesion (Bhavnani, 2006; Humphreys & Weinstein, 2006; Staniland, 2012). The remainder of this section provides three detailed examples of sets of research on conflict that disaggregate by actors.

#### 2.1.1 Targeting of Civilians

One focus of disaggregated analyses has been variation in the extent to which civilians are targeted, most notably within the same civil war. This topic has been examined with respect to violence committed by both state and non-state actors, differentiated into factions and even assessed at an individual level.

Kalyvas (2006) emphasizes the distinction between selective and indiscriminate violence during civil war as a function of territorial control. Selective violence against civilians is predicted to be highest where control is hegemonic but incomplete, whereas the use of indiscriminate violence is

greatest in zones completely under rival control. Building on this distinction, Herreros & Criado (2009) use the case of the Spanish Civil War to advance two separate logics to account for civilian victimization during civil war. One is strategic violence targeting potential political entrepreneurs. The other is indiscriminate violence as a consequence of the breakdown of the state. While the link between state collapse and the onset of civil war is well established in the literature, Herreros & Criado (2009) demonstrate that temporal variation in the recovery of public services better accounts for patterns of abuse against civilians.

Humphreys & Weinstein (2006) focus instead on warring factions, hypothesizing that internal structures and oversight of members are critical factors in determining whether civilians are abused during civil wars. The authors use data from a novel survey of ex-combatants to show that the absence of in-group policing within rebel groups leads to indiscriminate violence against civilians. Similarly, Balcells (2010) finds, based on the analysis of municipal-level data on violent events during the Spanish Civil War, that pre-war political competition between rival political factions is a factor in the degree of violence committed against civilians. Subsequently, Balcells (2011) shows that varying levels of violence against civilians within the same conflict are affected by prewar political support for enemy groups and wartime political parity within a locality.

All of this research highlights the advantages of not analyzing conflict as an aggregate, generic event. Taking the specific nature of violence seriously—and seeking to explain variation in type and severity, in this case with respect to the targeting of civilians—has prompted scholars to look more carefully at different actors. The findings demonstrate that the characteristics of those actors and how they are constituted and operate matters greatly for inferences regarding conflict. In addition, there is strong evidence to suggest that treating groups as monolithic and unified tends to be a poor assumption, since myriad interests, cleavages, and disputes are evident in conflicts. It is clear that the historical context and current environment exert influence, but also that both of these effects are not constants, as they depend on the specific situations of actors.

### 2.1.2 Rebel Group Dynamics

Another line of research seeks to understand the capabilities and actions of rebel groups, which affect the duration and severity of civil wars. Studies have yielded crucial awareness of conflict as typically comprised of complex interactions among a number of separate groups, rather than merely a dyadic interaction between a government and challenger.

Of particular importance is the finding that rebel in-fighting and side-switching may result in the proliferation of numerous local disputes, prolong the tenure of weak governments, and complicate settlement in the face of conflicting allegiances and grievances. Staniland (2012) demonstrates that lethal competition among insurgent factions can result in ethnic defection, with some groups joining the government. This mechanism is used to explain the rise of pro-state paramilitaries in Kashmir and Sri Lanka. Bakke et al. (2012) draw attention to rebel group fragmentation as a function of the degree of internal institutionalization, the number of organizations within a

## Chapter 2. Disaggregating Conflict by Actors, Time, and Location

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movement, and the internal power structures. These factors determine the cohesion of rebel movements and affect the duration and intensity of fighting.

Recent studies also delve into rebel motivations. A noteworthy example is Lyall (2013), which employs a novel geocoded dataset of 23,000 air strikes and shows of force in Afghanistan between 2006 and 2011. The analysis demonstrates that shows of force are associated with more insurgent violence, insofar as they create incentives for insurgents to establish and maintain their reputations with the local population. Lyall's finding provides unusual insight into the relative effectiveness of different counter-insurgency tactics, with a degree of rigor and precision that is facilitated specifically by the disaggregated nature of the data and the ability to examine events and their consequences in proximity.

Clearly, overlooking the full extent of what happens among and within rebel groups risks a mischaracterization of conflict, by oversimplifying what are actually complex dynamics. The latest research tackles those dynamics head on, capitalizing on new data, and provides a more nuanced understanding about how conflict unfolds.

### 2.1.3 Inequality and Violence

A further theoretical advance made possible by disaggregating agency in conflict pertains to the relationship between inequality and civil war. A decade ago, Fearon & Laitin (2003, 85) remarked that: "The poor quality of the inequality data, available for only 108 countries, does not allow us to go beyond the claim that there appears to be no powerful cross-national relationship between inequality and onset." The constraint they identified has, to a large extent, been relaxed with the availability of detailed subnational data on inequality across countries.

Buhaug et al. (2011) find that civil wars are more likely to break out in areas with low absolute income or high deviations from the national average, regardless of a country's aggregate level of economic development. Further research in this vein indicates that one of the principal drivers of violence is grievances arising from the unequal distribution of resources and resulting in resentment along group lines. Cederman et al. (2011b) use geocoded data on ethnic group settlement patterns and income to show that relatively richer or poorer groups fight more compared to those with incomes near the country mean (see also Chapter 9 of Backer et al., 2014). McCauley (2013) suggests that when economic inequalities overlap with ethnic identities and few provisions are made to include or compensate marginalized groups, the likelihood of violence increases. Sekulic et al. (2006) examine the causal link between ethnic intolerance and conflict, drawing attention to the importance of elite political mobilization. Pappas (2008) shows that elite exclusion from government creates incentives to capitalize on resentment and increases the salience of identity categories. Gubler & Selway (2012) find, however, that the likelihood of civil war decreases when ethnic cleavages crosscut class, regional, and religious ones.

Taken together, studies examining the ethnic bases of income inequality in a disaggregated fashion effectively challenge the notion that greed or opportunity override grievances as explanations for

civil war, as has been claimed (Azam, 2002; Collier & Hoeffler, 1998; Fearon & Laitin, 2003). The micro-level research underscores the intricate interactions between the behavior of key actors and broader social structures that can either enable or restrict such behavior.

## 2.2 Disaggregation by Time

The increasing availability of detailed information on the timing of conflict events enables analysis at monthly, weekly, daily, and even hourly time scales. From a theoretical standpoint, temporal disaggregation permits researchers to study mechanisms at more natural or appropriate time scales, thereby closing the gap between concepts and data (Kalyvas, 2008). Consider, for instance, cycles of escalation and de-escalation in the Israeli-Palestinian conflict, which typically last for days or weeks and are obscured by data reporting violence for calendar years (Bhavnani & Donnay, 2012; Haushofer et al., 2010; Jaeager & Paserman, 2006, 2008). With the same logic, Strauss (2007) uses temporally disaggregated data to study the relationship between the broadcast of “hate radio” messages and the onset of genocidal violence in Rwanda during 1994. Because the violence was concentrated in a period of about 100 days, and its onset around the country varied by weeks, calendar-year data is again inadequate. Taking proper account of this compressed timing, with respect to the sequence of events, Straus’ analysis rejects the popular narrative that hate radio was the primary driver of the genocide.

Temporal disaggregation also lends itself to addressing the endogeneity of conflict: the notion that previous conflict shapes factors such as actors’ preferences, which then influence the potential for ongoing and future conflict (Kalyvas, 2006; Voors et al., 2012). Bhavnani & Backer (2000), in a study of ethnic conflict and genocide in Rwanda and Burundi, show that temporal variation in the scale of violence is best explained by a combination of individual-level factors such as the propensity to engage in violence, form independent beliefs about others, and react to public messages about current levels of ethnic aggression, and genocidal norms enforced by group leaders. In their model, these factors both influence and are influenced by ensuing violence. Justino (2009) also highlights the self-reinforcing nature of endogenous dynamics. She suggests that poorer households in conflict areas support armed groups for protection and are in turn preyed upon, increasing the duration of conflicts independent of other explanatory factors.

Two examples that follow illustrate the utility of temporal disaggregation in studying civilian agency in conflict and the value of social media as a novel data source.

### 2.2.1 Civilian Agency

During the ongoing conflicts in Iraq and Afghanistan, civilians are often caught in the line of fire. Recent research by Condra & Shapiro (2012) sheds light on how violence against civilians—perpetrated both by insurgent and coalition forces—shapes the dynamics of conflict. Using weekly time-series, district-level data from 2004–2009, the study finds that civilian casualties

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caused by coalition forces led to an increase in the level of insurgent attacks, whereas civilian casualties caused by insurgent attacks dampened insurgent violence. As the authors explain, support for coalition troops among civilians increases when the latter are targeted by insurgents, and declines when targeted by coalition forces. Greater levels of civilian support for the coalition, in turn, tend to reduce insurgent violence. The study illustrates the value added of temporal disaggregation, given that Condra and Shapiro's analysis requires a precise tracing of what transpires following incidents resulting in civilian casualties—something that is impossible with more aggregated data. The research bolsters a literature that highlights the role of individual civilian agency in civil war (see also Kalyvas, 2006; Lyall & Wilson, 2009). It also sheds light on how the interactions between civilians and military actors shape violence.

### 2.2.2 Using Crowdsourced Data

Understanding the dynamics of short-duration military conflicts, in which events unfold over a matter of days or even hours, has traditionally been a challenge because of a lack of data with sufficiently high temporal resolution. This constraint has recently been overcome, thanks in part to the advent of social media and its exploitation as an information resource, greatly improving the prospects for relevant analysis.

One of the earliest examples of such research focuses on the conflict in Gaza from late 2008 to early 2009, the most deadly escalation between Israelis and Palestinians following the second Intifada, which was both rapid and intense. Zeitzoff (2011) generated hourly, dyadic conflict-intensity scores from Twitter and a number of other social media sources. He then analyzed these detailed time series to find an endogenous relation between current and future levels of violence. The results revealed a tendency for violence to escalate immediately after attacks by the rival side, as well as responses sensitive to international reactions. Zeitzoff's work demonstrates how social media sources can be used creatively, with great depth and a relatively fast turnaround, to study political violence in ways that were normally infeasible in the past.

Other "crowdsourced" data collection efforts have attracted broad attention. One that stands out is the deployment of the online platform Ushahidi, which was first developed to track the violence that broke out after the disputed 2007 election in Kenya. This particular platform has since been used in numerous other conflict settings, as well as in response to natural disasters, such as with the coordination of humanitarian relief after the devastating 2010 earthquake in Haiti. Similar crowdsourcing initiatives related to conflicts and disasters have been implemented elsewhere (see also Chapter 11 of Backer et al., 2014).

Through the work of organizations such as ICT4Peace, crowdsourced "big data" tools have been readily embraced by various UN agencies. Their general utility for conflict research, however, remains to be established. In this respect, a key issue is data quality, which inevitably affects the confidence in the results that can be obtained. The underlying idea is simple: thousands of discrete, small pieces of information supplied by local witnesses more accurately reflect a

situation on the ground than any expert observer possibly could. Nonetheless, there are valid concerns that these data have limitations (e.g., selective availability of geolocations) and even biases (e.g., those with the means to access technology and an inclination to report what they see are disproportionately represented). Of course, conventional datasets on conflict are hardly immune to analogous issues, especially given their reliance on mainstream media as data sources.

### 2.3 Disaggregation by Location

The burgeoning micro-level approaches to the study of conflict have directed far greater attention to the location of violence. The use of geographic information systems (GIS) permits researchers to combine spatial and statistical data to examine existing problems in novel ways (Cederman & Gleditsch, 2009). In particular, GIS simplifies integration of data from other existing sources, including covariates like GDP, elevation, and population.

Some conflict studies that use locations are based on data collection by individual scholars or small teams. These typically focus on a single case or a select number of cases. They may employ a combination of intensive field research (e.g., Ibáñez & Velasquez., 2009; Staniland, 2012), existing surveys, data collected by NGOs, official statistics and/ or newspaper reports (e.g., Bhavnani et al., 2011; Lyall, 2010). Other studies draw upon large-scale institutional initiatives, such as the Uppsala Conflict Data Program's Georeferenced Event Dataset (UCDP GED) (Sundberg et al., 2010) and the Armed Conflict Location and Event Dataset (ACLED) (Raleigh et al., 2010).<sup>1</sup> Such initiatives generally involve more expansive data collection spanning many countries, with standardized coding procedures to maximize precision and minimize error.

Among the research at the subnational level, some focuses on centers of population such as villages and cities and administrative units such as districts and regions (Balcells, 2011; Czaika & Kis-Katos, 2009; Kalyvas, 2006; Østby et al., 2009; Steele, 2009), while others employ grids composed of cells of an equal predefined area (Hegre et al., 2009). The specific research question typically determines the choice of spatial unit for the analysis. Studies then seek to explain variation across or within units, controlling for variations in unit characteristics (Buhaug et al., 2009; Do & Iyer, 2010; Lujala, 2010). Examples include analyses of variation in civilian abuse (Humphreys & Weinstein, 2006), the incidence of indiscriminate versus selective violence as a function of territorial control (Kalyvas, 2006), the number and relative capacities of rivals in shaping the use of selective violence (Bhavnani et al., 2011), the role of in-group policing and segregation in reducing violence in civil wars (Weidmann & Salehyan, 2013), and local wealth differentials as determinants of conflict onset (Buhaug et al., 2011).

We discuss two examples of research using spatially disaggregated units of analysis to study reactive dynamics and segregation. These examples further highlight the breadth of methodological approaches used in micro-level studies on conflict.

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<sup>1</sup>For more details on ACLED, see Chapter 7 in Backer et al. (2014).

### 2.3.1 Reactive Violence Dynamics

The increased availability of disaggregated event data has renewed interest in the relationship between conflict events. Studies examine what is broadly referred to as “reactive” dynamics—the circumstances under which violence perpetrated by one group elicits a reaction from the targeted group, resulting in the escalation (or de-escalation) of the conflict. Locations and their characteristics are logically important factors when examining the relationship between events. For instance, an attack in one ethnic enclave might be expected to generate a retaliatory attack on the rival group’s stronghold. Such topics can be studied properly only if the necessary details—such as where groups are based and commit acts of violence—are available. The latest research has made that leap, using data disaggregated by actor-group, as well as temporally and spatially. For instance, Linke et al. (2012) investigate the “tit-for-tat” dynamics between insurgent and coalition forces in Iraq. Applying autoregressive techniques, after aggregating event counts to small spatial grid cells, they find evidence for a “reactive” dimension to violence.

Of note, there are specific methodological challenges associated with disaggregating data spatially, in particular when conflict events are not confined to natural units of analysis, such as cities or villages. As in the study discussed earlier, researchers frequently aggregate data to arbitrary cells in order to apply standard econometric techniques to the resulting discrete spatio-temporal series (see also Buhaug et al., 2011; Raleigh & Hegre, 2009). The resulting inferences may be biased, however, given the selection of artificial grid sizes. In the geography literature, this issue is referred to as the “modifiable areal unit problem” or “MAUP” (Openshaw & Taylor, 1979). A number of disaggregated studies address this problem. Schutte & Weidmann (2011) introduce an innovative technique for the study of conflict diffusion processes in civil wars that overcomes the MAUP. Braithwaite & Johnson (2012), who examine the relationship between insurgent attacks and coalition counterinsurgency operations in Iraq, provide another example in which the inferences about spatial and temporal patterns are unaffected by the MAUP.

To achieve robust causal inferences, others prefer field experiments. For example, Lyall (2009) uses a natural quasi-experimental design to study reactive violence in Chechnya. Using shelled and unshelled villages as units of analysis and a statistical matching design for pseudo-random assignment, he demonstrates that indiscriminate violence produces a significant decrease in subsequent insurgent attacks. In this study and Linke et al. (2012), disaggregated data is essential for the detection of reactive dynamics, which are entirely obscured by data at higher levels of aggregation.

### 2.3.2 Segregation and Violence

The new data resources have also sparked interest in “bottom-up” agent-based modeling (ABM) techniques. This approach is well suited to studying dynamic interactions among agents on natural (i.e., realistic geographic) and artificial landscapes and to relating hypothesized micro-level processes to observed macro-level outcomes. Seeded with geographic and population data,

ABM affords a high degree of empirical validity.

Recent studies demonstrate the utility of empirically grounded ABMs for analyzing the relationship between individual-level interaction and violence. Weidmann & Salehyan (2013) analyze ethnic violence in Baghdad following the US troop surge in Iraq. Their ABM is seeded with detailed empirical data on the topology and ethnic geography of the city, as well as the location of violence. In a similar vein, Bhavnani et al. (2014) examine the case of Jerusalem between 2001 and 2009 using a realistic representation of the city based on the population structure and location of dwellings within each neighborhood. The study aims to reconcile competing perspectives on the effect of intergroup contact on violence. The first assumes that intermixed group settlement patterns reduce violence, with more frequent interactions enabling rivals to overcome their prejudices towards each other and become more tolerant. The second suggests just the opposite: that group segregation more effectively reduces violence given less frequent contact and fewer possibilities for violent encounters to occur.

Both studies make significant methodological advances and contribute to the long-standing debate on the relationship between residential settlement patterns and violence. The combination of formal models with rich, spatially disaggregated data enables the systematic study of alternative scenarios, with possible implications for policy makers and practitioners.

## 2.4 Contributions to Policy and Practice

By employing a disaggregated, micro-level approach to study the roots of conflict, the research surveyed in this chapter has illuminated, with considerable rigor and precision, the assortment of factors that contribute to violence. In the past, many of these drivers were consigned to a black box, or rendered as rough assumptions or post-hoc explanations for conflict phenomena. In contrast, the latest empirical analyses tackle these factors head on as hypotheses and are better able to detect the presence and absence of correlations and even causal relationships down to the level of groups (and segments thereof), communities, and individuals, accounting for spatial and temporal variation in dynamics. Next, we briefly consider the applications of this research to policy and practice, paying particular attention to several key issues: participation, victimization, migration, segregation, governance, and reconstruction.

### 2.4.1 Participation

The question of who participates in violence is complicated, given that violence lacks a single root cause and is driven by a mixture of grievance and opportunity. Individual studies point to a wide assortment of factors: the salience of religious and political identification, communal responsibility, patriotism, social status, reputation, and peer pressure (Muldoon et al. 2008); interactions between individual motivations, group networks, and state repression (Bosi & Della Porta, 2012); poverty, a lack of access to education, and political alienation (Humphreys &

## **Chapter 2. Disaggregating Conflict by Actors, Time, and Location**

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Weinstein, 2008); personal dependence on an organization, shared values with other recruits, the appeal of a clandestine lifestyle, and self-valuation (Florez-Morris, 2010); and age, gender, the size of rented land, and household income and investment (Verwimp, 2005).

What emerges from these studies is that the motives for participation in violence are heterogeneous, as are the characteristics of those who volunteer or are recruited for military and other armed groups. This insight may not be surprising, let alone revolutionary. The real contribution of the disaggregated approach has been to provide a growing accumulation of compelling evidence of the influences that matter, with ample room for exploring and understanding nuances, conditions and contradictions. This work underscores the need for evidence-based policy measures that are tailored to address the local topography of conflict—i.e., the relevant actors and their motivations and behavior.

### **2.4.2 Victimization**

A conventional approach to studying vulnerability to conflict at the macro level is to focus on structural variables, some of which exhibit strong correlations to outcomes. A natural question is whether parallel relationships are observed at the micro level—e.g., do individual manifestations of structural conditions, like poverty, have the same relationship to vulnerability?

From various studies, different conclusions emerge. Local conflict is positively correlated with unemployment, inequality, natural disasters, changes in sources of incomes, and clustering of ethnic groups within villages (Barron et al., 2004); with inequality and group polarization (Nepal et al., 2011); with poverty (Do & Iyer, 2010); larger group shares and more densely populated locales (Dabalén et al., 2012); and wealth (Hegre et al., 2009). Such detailed awareness is crucial to devising and deploying targeted, effective measures of conflict prevention that identify those at maximal risk.

### **2.4.3 Migration**

A major consequence of violence is the movement of individuals, households, and groups to locations—including segregated enclaves—that ostensibly offer a greater degree of safety. What drives individual flight, and who is most likely to flee? Compared to a cruder analysis of country-year data, such as on populations of refugees and IDPs, disaggregated research offers more revealing insights. Studies highlight the salience of conflict clashes, socio-economic factors, and local ethnic composition (Czaika & Kis-Katos, 2009); violent events as drivers of increased migration, with political events displaying the opposite effect (Williams et al., 2012); and the type of community (urban or rural) and the characteristics of the conflict (the existence of some ascriptive cleavage) (Steele, 2009). Improving the ability to anticipate flight—especially in large numbers—and understand the composition, motivations, and concerns of populations that flee are important for the planning and logistics of humanitarian relief efforts, the implementation of which can also have repercussions for the course of conflicts.

### 2.4.4 Segregation

A high-stakes consideration for policy makers is whether members of nominally rival social groups ought to be kept apart, more closely integrated, or at least encouraged to interact in various formal and informal settings. Conflicting arguments and evidence exist about which of these strategies achieves the best results in avoiding and mitigating conflict, as well as contributing to post-conflict peacebuilding. On the one hand, studies using disaggregated data suggest that ethnic avoidance and the establishment of relatively homogenous enclaves result in declining violence by reducing contact (Blair et al., 2012; Field et al., 2008; Weidmann & Salehyan, 2013). On the other hand, the opposite conclusion—pointing to a correlation between violence and ethnically segregated residential patterns—emerges in different contexts (Kasara, 2013; Kingoriah, 1980; K’Akumu & Olima, 2007). While findings are mixed, the latest research also suggests that the effects of segregation and mixing on conflict are critically dependent on the nature of intergroup relations, as gauged by indicators such as social distance (Bhavnani et al., 2014). The implication is that more fine-grained empirical research can help to inform what approach ought to be favored, and when.

### 2.4.5 Governance

What effect do specific state policies have on violence? Of note, disaggregated studies have focused on spending priorities, land tenure, and access to state power. One set of findings indicates that increased government spending on education, health, and social security mitigates civil conflict, albeit with little or no effect attributed to non-targeted public spending and military expenditures (Taydas & Peksen, 2012) or to the absolute level of state wealth (Bohlken & Sergenti, 2010). Another study shows that absolute poverty and inequality increase conflict risk (Buhaug et al., 2011). Additional research reveals that secure property rights feature among the most significant drivers of long-term income (Voors & Bulte, 2008) and by association, given the relationship between income and conflict, conflict mitigation (Butler & Gates, 2012). These results suggest a number of policy goals that governments could emphasize, including to reduce violence in locations with certain contributing characteristics.

### 2.4.6 Reconstruction

Policy makers and practitioners often strive to successfully navigate the aftermath of conflict and maximize the potential for sustained peace. A portion of the recent literature has evaluated the effects of targeted reform efforts in post-conflict societies. Studies examine the impact of promoting the adoption of specific crops on household welfare per capita (Bozzoli & Brück, 2009); the relation between subjective perceptions of violence, consumption expenditure, land use intensity, and the adoption of more risk-taking crop mixes (Badiuzzaman et al., 2011); individual exposure to violence, altruistic behavior, risk seeking, and high discount rates (Voors et al., 2012); the relationship between gender, reintegration and resilience (Annan et al., 2011); and the link

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between pre-conflict wealth and post-conflict economic growth at the provincial level (Justino & Verwimp, 2013).

In contrast to more aggregate studies of outcomes like conflict recurrence and their relationship to structural political, economic, and social characteristics of countries, the fine-grained results of disaggregated and especially micro-level research provide detailed assessments of policy successes and failures from the perspective of individuals, households, and groups. These findings offer concrete guidance to development agencies and organizations that are seeking to allocate programs and resources in a more targeted, calibrated, and efficient manner.

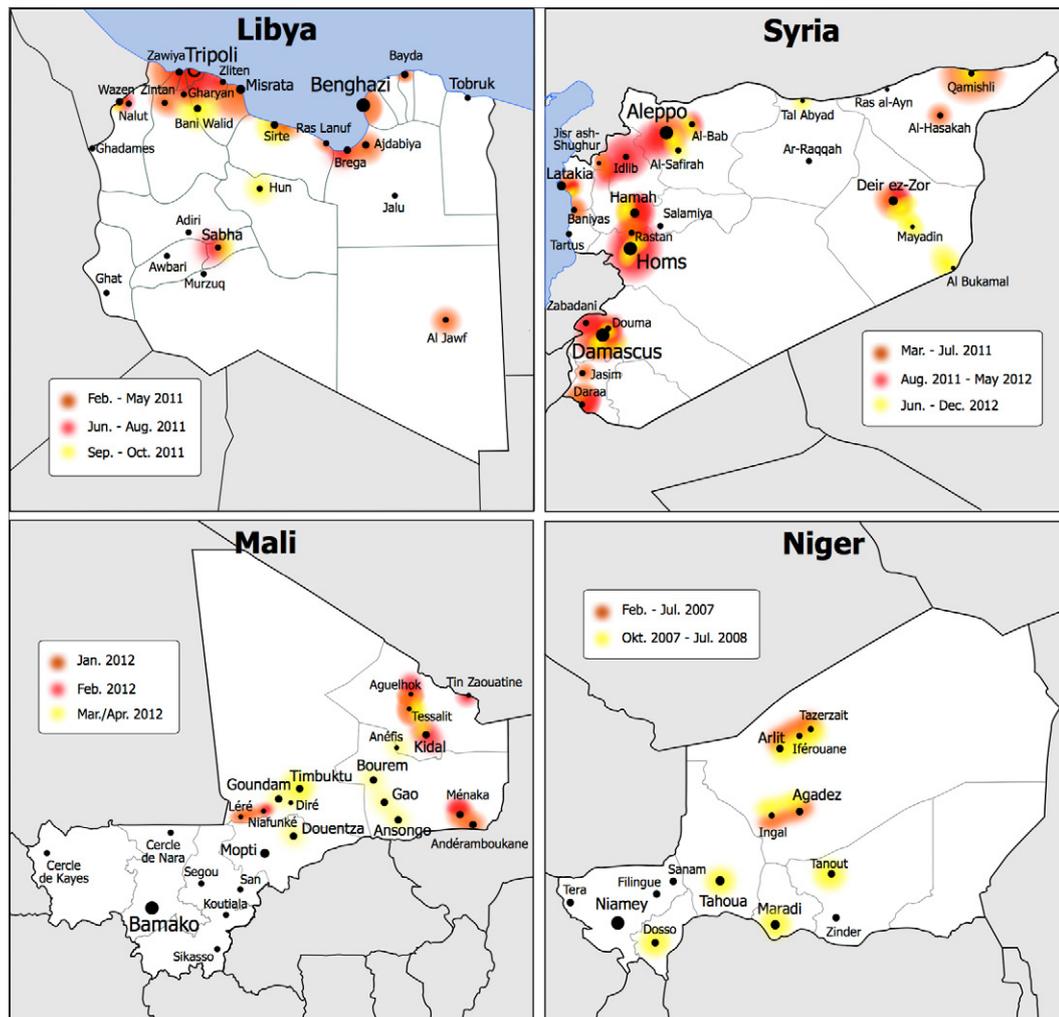
### 2.5 Conclusion

The various theoretical, methodological, and policy contributions reviewed in this chapter follow a common logic: new, more rigorous, accurate, and subtle insights are generated and overall understanding is improved by studying conflict and violence at the level at which the hypothesized mechanisms actually operate. This means gathering the necessary data on (1) actors, including individuals, households, and groups; (2) the timing of events of different kinds; and (3) their location, including neighborhoods, cities, municipalities, and provinces, as well as exact geographic coordinates. Disaggregation has shed light on previously unexplained issues, clarified or rectified findings from previous analyses, and in the process, uncovered new considerations and questions.

Moving to data with greater geographic and/or temporal resolution typically increases sample size, with obvious benefits for statistical inference. Shortcomings may arise, however, from inadequately disaggregated variables:

These practices lead to the reproduction of problems encountered in the macro-literature such as the absence of clear microfoundations, the distance gap between theoretical constructs and proxies, and the inability to adjudicate between observationally equivalent causal mechanisms. (Kalyvas, 2008, 398–399).

In particular, Shellman et al. (2010) show that inadequate actor disaggregation may affect inferences and lead researchers to commit both Type I (i.e., false positive) and Type II (i.e., false negative) errors. The problem of finding the “right” unit of actor aggregation is often complicated by the fact that the coding or identification of actor groups varies over time and across regions—a long-standing challenge recognized in the literature on cross-national studies (see, for example, Hug, 2003). Nonetheless, the ability to account for subnational variation, both over time and across space, has yielded important insights on the dynamics of violence, its reactive dimensions, and its relation to patterns of territorial control and ethnic settlement patterns, as was discussed with respect to the examples provided earlier. Designs that continue to use the country-year as their unit of analysis miss relevant action at finer temporal and geographic scales.



**Figure 2.1: Sample Conflict Intensity Maps Based on New Media Sources.** *Source:* Compiled by authors.

Meanwhile, the expansion of new media has been opening up productive avenues for policy-relevant analysis. Most notably, data collection relying on social media, including crowdsourcing and big-data approaches, is distinguished by the ability to cover conflict in close to real time. The opportunity for rapid, contemporaneous analysis represents a vast improvement relative to traditional approaches, which involve lags—often lengthy—between when conflict events occurred, information was collected from archives of mainstream media, datasets were made available, and studies were conducted. Now, up-to-date, detailed profiles and maps can be assembled on the course of conflicts all around the world in a matter of days or even hours, with information derived exclusively from new media sources.

Figure 2.1 presents the results of one such exercise, yielding visual timelines of the distribution, progression, and severity of violence during recent civil conflicts in Libya, Syria, Mali, and Niger. Different colors mark the areas affected in different phases of the conflicts in each of

## Chapter 2. Disaggregating Conflict by Actors, Time, and Location

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these countries; the darker the shading, the more intense the conflict. The conflicts and other aspects of these countries differ in consequential ways. Moreover, the settings present challenges to traditional data collection, such as difficult security environments and limited infrastructure, including low availability of technology and free media. Yet the patterns of violence in each country can be examined via content available on social media. Such capabilities, if employed effectively, enable more current, informed assessments of conflict risks and events, as well as faster, more targeted and otherwise better calibrated responses by a wide range of actors, including intergovernmental organizations, states, and civil society.

With respect to policy, research using disaggregated approaches highlights a need for systemic solutions to structural inequality, inclusion, and representation to dampen the incentives for conflict, among other things by paying greater attention to the security of land tenure and providing compensation to victims in the aftermath of violence. The findings also emphasize the importance of context and suggest that policy outcomes vary across conflict settings. It remains true that disaggregated research, based on reliable evidence, where available, is needed to ask the critical “what if” types of questions about addressing the causes, dynamics, and consequences of violence. Yet greater consistency and comparability across studies are required to facilitate the choice, design, and implementation of successful peacebuilding measures.

While the recent accumulation of literature reveals substantial theoretical and technical progress, the turn of research in this direction also presents significant obstacles. These include the need for appropriate theorizing of causal mechanisms, issues of data collection and quality, and decisions about appropriate units and methods of analysis. The findings suggest that features of study design, including the specific questions and hypotheses that are addressed and the data that are gathered, could account for at least some of the variation in what is observed across the country contexts. Another issue is source bias. Studies have shown that this can arise as a function of differences in observer interest, the type of event observed, and the context in which the event occurred (e.g., Davenport & Ball, 2002). Thus, disaggregation is not immune to the issues evident in other existing research, much less inherently superior to anything done at a more aggregate level. Instead, disaggregated analyses must still surmount significant hurdles—not least in the collection of data—to achieve greater rigor and yield better insight in the study of conflict.

# 3 Group Segregation and Urban Violence<sup>†</sup>

## Abstract

How does segregation shape intergroup violence in contested urban spaces? Should nominal rivals be kept separate or instead more closely integrated? We develop an empirically grounded agent-based model to understand the sources and patterns of violence in urban areas, employing Jerusalem as a demonstration case and seeding our model with microlevel, geocoded data on settlement patterns. An optimal set of parameters is selected to best fit the observed spatial distribution of violence in the city, with the calibrated model used to assess how different levels of segregation, reflecting various proposed “virtual futures” for Jerusalem, would shape violence. Our results suggest that besides spatial proximity, social distance is key to explaining conflict over urban areas: arrangements conducive to reducing the extent of intergroup interactions—including localized segregation, limits on mobility and migration, partition, and differentiation of political authority—can be expected to dampen violence, although their effect depends decisively on social distance.

## 3.1 Introduction

Recent outbreaks of violence in multiethnic cities across the world highlight the fragility of intergroup relations. Such conflict raises a fundamental issue: what can be done to foster harmonious coexistence in contested urban spaces? In particular, should nominal rivals be kept separate or instead more closely integrated? This question remains unresolved, given ambiguous empirical evidence and contrary theoretical perspectives about causal mechanisms, which together have engendered a vigorous, ongoing debate in the literature.

On the one hand, observations from numerous cities around the world suggest that to mitigate

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<sup>†</sup>This chapter is an edited version of the following article: Ravi Bhavnani, Karsten Donnay, Dan Miodownik, Maayan Mor and Dirk Helbing. (2014). “Group Segregation and Urban Violence.” *American Journal of Political Science* 58(1): 226–245. <http://dx.doi.org/10.1111/ajps.12045>

### Chapter 3. Group Segregation and Urban Violence

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intergroup conflict, nominal rivals are best kept apart. In Belfast during the 1970s, residential, social, and educational segregation attenuated hate crimes by diminishing opportunities for direct intergroup contact (MacGinty, 2001). During the Los Angeles riots of 1992, ethnic diversity was closely associated with rioting (DiPasquale & Glaeser, 1998), whether as a result of ethnic succession (Bergesen & Herman, 1998) or mixing that intensified ethnic competition (Olzak, 1992; Olzak et al., 1996). That same year, Indian cities in Maharashtra, Uttar Pradesh, and Bihar, each of which had a history of communal riots, experienced violence principally in locales where the Muslim minority was integrated. In Mumbai, where over a thousand Muslims were killed in predominantly Hindu localities, the Muslim-dominated neighborhoods of Mahim, Bandra, Mohammad Ali Road, and Bhindi Bazaar remained free of violence (Kawaja, 2002). Moreover, violence between Hindus and Muslims in Ahmedabad in 2002 was found to be significantly higher in ethnically mixed as opposed to segregated neighborhoods (Field et al., 2008). In Baghdad during the mid-2000s, the majority displaced by sectarian fighting resided in neighborhoods where members of the Shi'a and Sunni communities lived in close proximity, such as those on the western side of the city (Bollens, 2008).

On the other hand, from different cities the exact opposite conclusion emerges—members of rival groups should be more closely integrated to avert violence. Race riots in the British cities of Bradford, Oldham, and Burnley during the summer of 2001 were attributed to high levels of segregation (Peach, 2007). In Nairobi, residential segregation along racial (K'Akumu & Olima, 2007) and class lines (Kingoriah, 1980) recurrently produced violence. In cities across Kenya's Rift Valley, survey evidence points to a correlation between ethnically segregated residential patterns, low levels of trust, and the primacy of ethnic over national identities and violence (Kasara, 2012). In Cape Town, following the forced integration of blacks and coloreds by means of allocated public housing in low-income neighborhoods, a "tolerant multiculturalism" emerged (Muyeba & Seekings, 2011). And across neighborhoods in Oakland, diversity was negatively associated with violent injury (Berezin, 2010).

Scholars have advanced conflicting notions about why and when intergroup contact is associated with conflict, i.e., pronounced tension and its manifestation in violence (Dovidio et al., 2003; Pettigrew, 1998; Pettigrew & Tropp, 2000, 2006). A prominent segment of the literature indicates that because ignorance breeds prejudice and introversion reinforces intolerance, contact improves intergroup relations (Allport, 1954; Williams, 1947). More recent studies underscore the logic that positive contact between nominal rivals reduces social distance (Pettigrew & Tropp, 2000), prejudice (Pettigrew & Tropp, 2006), and sectarianism (Hayes et al., 2007), and increases the desire to have ongoing interactions (Gaunt, 2011). Meanwhile, low levels of contact have been associated with opposite effects, including reciprocal perceptions of animosity (Lichbach, 1995), more effective intragroup communication (Fearon & Laitin, 1996), heightened territorial attachment and greater ease of group-based mobilization (Toft, 2003), and resistance (Buhaug & Rød, 2006), all of which can be conducive to intergroup conflict. Furthermore, limited contact between groups often reflects geographic concentration, especially when congruent with dense social and economic in-group networks. Such concentration has been shown to alleviate collective action problems, providing members with a strategic advantage to communicate and coordinate

for conflict (Weidmann, 2009).

A competing perspective maintains that conflict occurs regularly alongside high levels of intergroup contact, which not only fail to undermine prejudice, but rather serve to reify cultural stereotypes and group differences (Forbes, 1997). Thus, conflict between rival groups does not necessarily abate with higher levels of contact, which instead seemingly enhance the prospects of violence in at least some cases. On these grounds, it appears that reducing intergroup interactions can actually serve as a peace building measure. Indeed, at the extreme, “intermingled settlement patterns create real security dilemmas that intensify violence, motivate ethnic ‘cleansing’, and prevent de-escalation unless groups are separated” (Kaufmann, 1996, 137).

While these competing perspectives can potentially be reconciled, further research is warranted to better understand the consequences of contact for conflict, including the mechanisms that affect this relationship, and to investigate more fully the merits of peace building approaches that seek to alter how members of different groups relate to one another. A key challenge in this regard is appreciating the repercussions of different options for the spatial and temporal patterns of violence, especially in places like cities, where heterogeneity is the norm and the combination of high population density and physical proximity heightens the latent potential for intergroup interactions. The research to date has been inadequate to assess those relationships, due to the limited availability of relevant microlevel data, study designs that consequently favor analysis at higher levels of aggregation, and a lack of rigorous inquiry into alternative scenarios.

Our goal is to develop, test, and apply a new framework to better understand the sources and patterns of intergroup conflict in urban areas, using an evidence-driven model seeded with microlevel, geocoded data on settlement patterns and violence. This approach allows us to replicate the spatial distribution of violence and model “virtual scenarios” to assess their relative impact on violence. We start by reflecting further on the empirical literature, identifying a causal mechanism that appears to consistently influence when and how segregation shapes violence. Next, we describe the structure and parameters of an agent-based model designed to examine this relationship by means of evidence-driven simulation. We then offer an overview of the empirical case—Jerusalem during 2001–2004 and 2005–2009—used to demonstrate the viability and utility of the framework and describe the empirical calibration and validation of the model. After seeding the model with relevant contextual data from Jerusalem, we optimize the model’s parameters such that the patterns of violence from the simulation closely fit the actual distributions in the city for each time period. We use the calibrated model to conduct a counterfactual analysis of how various “virtual futures” for the city shape the spatial distribution of violence. The counterfactual scenarios reflect different levels of segregation, including several that would likely ensue in the event of the implementation of peace proposals. We conclude by reflecting on the theoretical, policy, and methodological contributions of our results. Among the notable findings is that besides spatial proximity, social distance is key to explaining conflict over urban areas: while integration is a promising strategy when social distance is small, arrangements conducive to reducing the extent of intergroup interactions—including localized segregation, limits on mobility and migration, and differentiation of political authority—are more effective otherwise.

### 3.2 The Relationship between Segregation and Violence

The divergent findings concerning the relationship between segregation and violence underscore the need to identify a causal mechanism that may consistently account for both perspectives.

A logical explanation for results contrary to the expectations of contact theory is that the conditions necessary to realize the benefits of intergroup interactions do not prevail in all instances.<sup>1</sup> Incidents of conflict may occur between members of groups who cross paths with one another, even frequently, but perceive themselves as being of differing status, pursue divergent goals, prioritize intra- over intergroup cooperation, or receive unequal levels of public support (Horowitz, 1985, 2001). Likewise, in the context of intergroup competition in urban settings, collective oppression leads individuals to see members of other groups as potential threats, driven by an admixture of alienation, prejudice, belief stratification, and self-interest (Bobo & Hutchings, 1996). The obvious interpretation is that contact alone is insufficient without supporting attitudes, orientations, behaviors, institutions, and policies, which hardly can be taken for granted amid intergroup contestation and may require more intensive, sustained processes and commitments.

Another consideration is that the relationship between intergroup contact and conflict is likely endogenous, with multiple outcomes possible. For example, segregation in Belfast precipitated by violent conflict during the late 1960s and early 1970s (Doherty & Poole, 1997) facilitated the politicization of Catholic and Protestant identities and effectively abetted a resurgence of intergroup violence during subsequent decades (Shirlow & Murtagh, 2006). In Baghdad, ethnic migration following deadly attacks engendered a decline in violence between rival groups (Weidmann & Salehyan, 2013). Similarly, a survey of 6,275 households in Karachi found that in addition to income and ethnic composition, the incidence of violence was a major determinant of the neighborhood choice (Ahmad, 1993). In Guatemala City, among the most dangerous urban areas in Latin America, small-scale segregation—the creation of gated communities in peripheral areas—rose in response to high levels of crime and drug-related violence (Roberts, 2010). As these examples suggest, residential settlement patterns are endogenous to the very outcome of interest, violence.

#### 3.2.1 Specifying a Causal Mechanism

Acknowledging that contact alone is insufficient to explain the onset or absence of violence, we subscribe to the notion that a comprehensive measure of segregation should include a social

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<sup>1</sup>Allport (1954) posited four conditions for the benefits of such contact to materialize in practice: equal status of groups, goals shared by groups, instances of cooperation between groups, and institutional backing of intergroup interaction. Others have since reinforced, refined, and expanded Allport's hypotheses, arguing that the extent of bias against the out-group—or lack thereof—is influenced by many factors. The list includes perceptions of comparable status (Brewer & Kramer, 1985), the sense of common objectives (Chu & Griffey, 1985), and indications of intergroup collaboration (Blanchard et al., 1975). Among the additional factors that have been identified are the general nature of intergroup relations (Sherif et al., 1961), social identities and self-categorization (Tajfel & Turner, 1979; Turner et al., 1987), intergroup friendship (Brewer & Miller, 1984; Pettigrew, 1998), norms and practices (Landis et al., 1984), new information (Gaertner & Dovidio, 1986), and behavioral modification (Pettigrew, 1998).

### 3.2. The Relationship between Segregation and Violence

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distance matrix, alongside the essential spatial aspect (Reardon & Firebaugh, 2002). In this respect, we part company with Weidmann and Salehyan's (2013) analysis assessing the impact of segregation vis-a-vis the "surge" in mitigating violence in Baghdad. Weidmann & Salehyan (2013) utilize a geo-referenced model, integrating data on ethnic settlement patterns and the distribution of violence, optimized for a match between simulated and empirical data. We take their analysis as inspiration for our work, yet in contrast to their specification of either a constant attack probability or one that is shaped by the local ethnic mix, we choose not to focus strictly on how people are arrayed geographically and the frequency with which they interact. Rather, we consider that the nature of intergroup relationships, represented by social distance, matters decisively.<sup>2</sup>

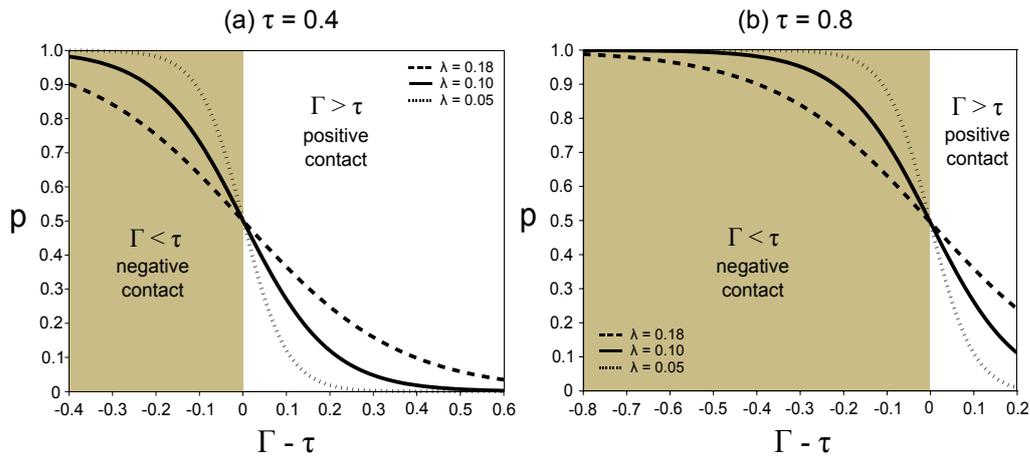
We take social distance to encompass a variety of intergroup differences, including those associated with class, ethnicity, religion, race, and gender, with specific variants labeled affective, normative, interactive, cultural, and habitual (Karakayali, 2009).<sup>3</sup> Our decision to consider how the nature of intergroup relationships shapes contact is bolstered by at least two reasons. First, relationships can exhibit the distrust, intolerance, and enmity that would seem to be necessary drivers of conflict, which the nature of physical separation alone cannot supply. Second, even if where people reside remains the same, relationships can still vary, providing a source of the dynamics that can account for periodic flare-ups of violence in otherwise static circumstances.

Consistent with the literature on conflict (Cederman & Girardin, 2007a; Fearon & Laitin, 1996; Gurr, 1970; Horowitz, 1985; Olzak, 1992), we treat individuals as being affiliated with groups. Of course, groups are neither monolithic nor homogenous. A group's members commonly vary along several pertinent dimensions, such as their affinity with the group, history of interaction with people from other groups, exposure to past episodes of violence, and disposition to participate in violence. Therefore, a proper analysis of the topic at hand cannot be conducted at the level of groups alone. Instead, we represent individuals as quasi-independent actors, while recognizing the influence on their attitudes and behaviors of their group ties, whether ascriptive, willfully adopted, socially constructed, or a by-product of profession. We go further still in linking variation in population distribution, policing, and violence at the level of localities or neighborhoods to variation in behavioral outcomes, an exception in the study of ethnic violence (Green & Seher, 2003).

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<sup>2</sup>Our approach differs from Weidmann and Salehyan (WS) in still other, notable respects. We specifically (1) analyze more than two groups; (2) endogenize the likelihood of a civilian perpetrating violence as a function of individual-, group-, and neighborhood-specific factors, rather than distinguish a priori between nonviolent civilians and insurgents who alone perpetrate violence; (3) relax the assumption that policing occurs with some constant success rate and results in the removal of an insurgent, instead allowing it to mitigate violence in the short term and heighten violence between civilians and security forces in the long term; (4) downplay the salience of migration—a far more central mechanism in the case WS analyze; (5) use fine-grained data on neighborhood ethnic composition, residential settlement patterns, and in- and out-migration; and (6) utilize stricter criteria along multiple dimensions in estimating our model.

<sup>3</sup>We opt to employ affective social distance, first popularized in the Bogardus Social Distance Scale (Bogardus, 1925), which focuses on the "reactions of persons toward other persons and toward groups of people" (Bogardus, 1947, 306)



**Figure 3.1: Causal Mechanisms: Linking Group Segregation to Violence.**  $p$  denotes the probability to engage in violence for different values of the transition parameter  $\lambda$ .

Our theoretical framework specifies the probability  $p$  that an individual engages in violence as a function of social distance  $\tau$  and a violence threshold  $\Gamma$ , such that  $p = f(\Gamma - \tau)$  with  $0 \leq \tau \leq 1$  and  $0 \leq \Gamma \leq 1$ . For any given social distance, the probability to engage in violence is assumed to increase as the violence threshold decreases. Figure 3.1 depicts the probability of violence for individuals with relatively low ( $\tau = 0.4$ ) and high social distance ( $\tau = 0.8$ ). All else being equal, the range of threshold values for which contact is violent will be considerably wider when social distance increases, as represented by the larger shaded area in Figure 3.1b relative to Figure 3.1a. While the two extremes—contact as exclusively positive or negative—are included in our framework as the limiting cases for  $\tau = 0$  and  $\tau = 1$ , respectively, it is the region between these extremes that is decisive for most acts of violence. A more detailed description of our theoretical framework follows, as we introduce the model designed to examine the relationship between segregation and violence below.

### 3.2.2 Model Description

For the purpose of our analysis, we opt to rely on agent-based modeling. This computational methodology is suitable and valuable to develop a more nuanced understanding of how the extent of contact between the members of groups—as influenced by segregation and other factors—affects spatial variation in intergroup violence in urban areas, for various reasons. One advantage is the ease of studying individuals, groups, and institutions simultaneously, in an integrated fashion. The flexibility to handle such agent granularity is a hallmark of agent-based modeling. Another advantage is the ability to represent actors interacting on physical landscapes, which enables the exploration of geography and the movement of actors, as well as the timing and sequencing of events. In adopting this methodology, we also extend a line of work that relies on agent-based modeling in studying civil conflict (Bennett, 2008; Bhavnani & Backer, 2000;

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Bhavnani et al., 2011; Cioffi-Revilla & Rouleau, 2010; Epstein, 2002), employing an explicitly data-driven approach in which disaggregated empirical data are used to seed, optimize, and validate the agent-based model (Benenson, 2004; Geller, 2008; Weidmann & Salehyan, 2013). As such, our framework refines the mechanisms that others have used to study the emergence of ethnic segregation and its link to violence in an effort to focus more sharply on the conditions under which segregation generates—and is in turn generated by—violence.

Our model studies the dynamics underlying violent events brought about by the interaction between members of  $g$  nominally rival groups in an urban setting, where the likelihood of conflict depends on the social distance between the groups.<sup>4</sup> Agents are geographically distributed in a discretized two-dimensional space that mirrors the actual physical geography of a city, specified with geocoded information on the location, size, and shape of neighborhoods, as well as the general location of housing settlements. The population of each neighborhood is likewise based on empirical data and dynamically updated for each group using a natural rate of growth that reflects statistics on births, deaths, and net migration. Agents interact within their local surroundings and migrate from one neighborhood to another in an effort to minimize their exposure to violence.

A simulation run begins with the random assignment of agents designed to constitute the aggregate population of each neighborhood  $N$ . Agents are then updated in a random sequential order, with a time step defined as the number of simulation steps in which 10% of the population has been updated.<sup>5</sup> In each step of a simulation run, agent  $i$  first interacts and then decides whether to migrate. Specifically, agent  $i$  engages in a pairwise interaction with another agent  $j$  randomly selected from her immediate surroundings  $R$ , which in contrast to the geographical neighborhood  $N$ , is constructed concentrically around every given site.<sup>6</sup>

Defining interactions on  $R$  rather than on the larger geographical unit, the neighborhood  $N$ , is both theoretically and empirically motivated. First, local contact between residents within  $R$ —interaction in areas smaller than the neighborhood  $N$ —is central to the theoretical question we address. To operationalize these “local interactions”, partners are chosen from the immediate surroundings in which contact takes place—ensuring the comparability of interaction areas across the city. Second, residential areas may only comprise a small part of a neighborhood, resulting in little or no sustained contact between residents located at opposing edges of  $N$ . Third, violence may often arise at the intersection of neighborhoods, along boundaries; simply selecting interaction partners from within  $N$  would effectively neglect these important dynamics, whereas permitting interaction with all surrounding neighborhoods would bring together residents characterized by little or no recurrent contact. Interactions on  $R$  naturally account for these dynamics since all residents in a locality—-independent of administrative boundaries—are considered.

The probability that agent  $i$  engages in violence when interacting with agent  $j$  is specified by the

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<sup>4</sup>The supporting information for this article (Appendix A) provides a detailed description of the model implementation, calibration and validation procedures, data and sources, as well as the operationalization of our counterfactual scenarios.

<sup>5</sup>This (arbitrary) definition of a time step is offset by only considering aggregate simulation statistics.

<sup>6</sup>See Section A.2.2 in the supporting information for further details.

following function, depicted in Figure 3.1:

$$p_{i,j}(t) = \left( 1 + \exp \left[ \frac{-(\tau_{i,j} - \Gamma_i)}{\lambda} \right] \right)^{-1} \quad (3.1)$$

The abstract social-distance metric  $\tau_{i,j}$ , which represents the level of tension between the groups that agents  $i$  and  $j$  represent, has  $(g^2 - g)$  nonzero entries. For intragroup relationships, we set  $\tau_{i,i} = 0$ , which implies that only interactions between members of different groups are assumed to generate violence. The transition parameter  $\lambda$  controls the shape of the violence probability curve (see Figure 3.1), and the parameter  $\Gamma_i$  constitutes a violence threshold. Thus, the degree of social distance influences whether contact is predominantly violent or nonviolent. For any given social distance, the probability of violence increases as the violence threshold decreases, whereas the likelihood that interaction is nonviolent, though conceivably hostile, rises with the threshold to engage in violence.<sup>7</sup>

The violence threshold  $\Gamma_i$  is calculated dynamically as a simple linear combination of three factors:

$$\Gamma_i = \frac{(1 - \nu_R) + (1 - d_G) + s_N}{3} \quad (3.2)$$

In this equation,  $\nu_R$  represents the memory of past violence in agent  $i$ 's locality  $R$ ,  $d_G$  is the perception of discrimination by members of agent  $i$ 's group  $G$ , and  $s_N$  represents the level of state policing in  $i$ 's neighborhood  $N$ . All three factors are drawn from the literature on intergroup conflict and particularly pertinent to the empirical case we examine, as key determinants of the propensity to engage in violence in an urban area.

The memory of past violence is an individual-level parameter that addresses several considerations. As mentioned earlier, segregation appears to be endogenous to violence. In addition, the diffusion and contraction of violence likewise appear to be endogenous. This is demonstrated by the fact that homicides are often retaliatory in nature (Black, 1983; Block, 1977; Morenoff et al., 2001). Also, prior riots have been found to increase the likelihood of racial strife (Olzak et al., 1996), resulting in relocation and escalation diffusion, i.e., the spread of violence to adjacent locations and an increase in its scale (Schutte & Weidmann, 2011). In our model, the memory of violence  $\nu_R$ , defined as the average of memories in agent  $i$ 's immediate surroundings  $R$ , is affected by both violent and nonviolent contact. At the outset, we assume all agents have no memory of intergroup violence. If violence ensues, the memory of violence increases among all affected neighbors in the victim  $j$ 's immediate surroundings. The outcomes of interactions further in the past are discounted relative to those of more recent interactions by having memories decay exponentially on a characteristic time scale  $t$ . Since  $\nu_R$  increases after episodes of violence, this raises the probability of future violence ( $\Delta\Gamma < 0$ ). By contrast, periods of nonviolence reduce  $\nu_R$  over time, thus lowering the likelihood of further violence ( $\Delta\Gamma > 0$ ).

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<sup>7</sup>Our specification ensures that while thresholds are situation-specific, behavioral decisions exhibit a measure of continuity (Granovetter, 1978).

### 3.2. The Relationship between Segregation and Violence

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Discrimination  $d_G$  is specified at the level of each group  $G$  and increases the likelihood of violence. The logic that frustration breeds aggression is demonstrated in various studies that highlight the link between violence and relative deprivation (Gurr, 1970; Østby et al., 2009), exclusionary policies targeting specific ethnic groups (Cederman & Girardin, 2007a; Horowitz, 1975; Wimmer et al., 2009), and the related notion of horizontal inequality (Cederman et al., 2011b; Østby, 2008; Stewart, 2008). Discrimination affects the orientations of members of a group toward the members of all other groups, with higher levels conducive towards a greater propensity to engage in violence.

State policing is defined at the neighborhood level and has the effect of deterring individuals from engaging in violent activity.<sup>8</sup> Policing has been shown to reduce violence when above a critical ratio of law-enforcement officers to residents (Fonoberova et al., 2012), consistent (Lichbach, 1987), effective (Fearon & Laitin, 2003; Poutvaara & Priks, 2006), timely (Weidmann & Salehyan, 2013), and capable of imposing high punishment costs (DiPasquale & Glaeser, 1998). In our model, the policing parameter  $s_N$  can vary from no police presence (0) to very strong police presence (1) and changes endogenously based on the model dynamics. Starting initially with a value of 0,  $s_N$  is set to 1 whenever an incident of violence occurs, then decreases on a characteristic time scale  $t$  when violence is absent. The impact of policing is also conditional on intergroup relations: for small social distances, policing will tend to result in less violence, whereas in the context of high social-distance policing—well intentioned or not—it is generally considered to be provocative and leads to more violence. Our specification reflects these features.

While the primary mechanism in our model is pair-wise interaction between agents, an endogenous link between the resulting dynamics and the distribution of the population on the model topology is established via migration. The migration mechanism permits individuals to relocate to less violent neighborhoods in which a majority or significant fraction of their group resides (Schelling, 1978). In addition, all individuals may migrate to less violent neighborhoods or out of the city under conditions of endemic violence (Doherty & Poole, 1997; Weidmann & Salehyan, 2013). Specifically, the migration of an agent from neighborhood  $N$  to a new neighborhood  $N'$  is executed with probability  $m_G$ , an empirically based mobility factor for each group.<sup>9</sup>

Since the outcomes of previous time steps affect the subsequent states of the simulation, the results of our agent-based simulations have an element of path dependence. While the occurrence of violence or nonviolence matters for what transpires subsequently, it does not define a single

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<sup>8</sup>We readily acknowledge that state-sanctioned and intergroup violence differ with respect to their causes and effects. As a result, we explicitly model violence perpetrated by social groups, whereas state-sanctioned violence is implicitly captured through the level of policing, which increases as a direct response to violent incidents rather than as a function of local conflict dynamics and intergroup tension. The primary effect of policing is to counteract further violence; however, an increased police presence also leads to more interaction between civilians and security forces and may therefore serve to incite violence directed at the police. In the model, security forces are assigned to each neighborhood in numbers proportional to the level of policing and have no specific location. Interaction partners are then randomly drawn from (1) all civilian agents within  $R$  and (2) security forces; the latter are selected with probability proportional to  $s_N$ . See Section A.2.2.

<sup>9</sup>See Sections A.2.1 and A.2.3 in the supporting information.

### Chapter 3. Group Segregation and Urban Violence

|                                    | Variables   | Values               |
|------------------------------------|---|----------------------|
| Explanatory Variables (estimated)  | $\tau$ : social distance between groups   | $0 \leq \tau \leq 1$ |
|                                    | $d_G$ : perception of discrimination of group $G$   | $0 \leq d_G \leq 1$  |
| Endogenous Variables               | $v_R$ : past violence in local surroundings $R$   | $0 \leq v_R \leq 1$  |
|                                    | $s_N$ : level of policing in neighborhood $N$   | $0 \leq s_N \leq 1$  |
| Empirical Parameters (fixed)       | $m_G$ : mobility of group $G$   | $m_U = 0.01$         |
|                                    | Empirical data also define the demography of each neighborhood, population size, city topography and locations of settlements | $m_S = 0.02$         |
|                                    |   | $m_P = 0.03$         |
| Interaction Parameters (estimated) | $r$ : size of local surrounding $R$   | $r = 5$              |
|                                    | $\lambda$ : scale of logistic threshold function  | $\lambda = 0.05$     |
|                                    | $t$ : time scale for violence memory and policing decay   | $t = 30$ sim. steps  |

**Table 3.1: Model Overview.**  $S$ : Secular/Moderate Orthodox Jews,  $U$ : Ultra-Orthodox Jews,  $P$ : Palestinians. The derivation of the values for  $m_G$  is detailed in Section A.2.3 in the supporting information.

course of events given several sources of variation: migration decisions are probabilistic and contingent on the continually changing context of group distribution and violence; agent pairings are randomized; agent behaviors are probabilistic and contingent on evolving conflict drivers; and the influence of past interactions progressively fades. Consequently, the model is not deterministic: identical parameter configurations yield a range of similar outcomes for different random simulation seeds. We provide a summary of the model’s parameters in Table 3.1.

As part of the analysis of a specific case, the model is calibrated and validated with respect to a baseline of empirical data on (1) the number of violent incidents per neighborhood; (2) the location of violence; and (3) the distribution of attack targets, by group, across the entire city (which ensures a correspondence to overall perpetrator/victim patterns).<sup>10</sup> This step involves an exhaustive, enumerative calibration procedure whereby we vary the social distance and discrimination parameters—i.e., the variables not endogenous to the simulation—and identify values for which the model best fits the baseline empirical data.<sup>11</sup> Social distance influences whether contact is predominantly violent or nonviolent, whereas discrimination alters the likelihood of violence independent of social distance; social distance is specified dyadically, whereas discrimination is not explicitly directed toward out-group members. Both parameters feature as key drivers of violence and have clear empirical referents. In addition, we include the full set of interaction parameters  $\lambda$ ,  $r$ , and  $t$ —the scale of the logistic threshold function, the size of the local surroundings  $R$ , and the time scale for memory decay—in our calibration routine.<sup>12</sup>

<sup>10</sup>There are a number of common techniques to quantify correspondence with empirical data; here, we employ Pearson’s correlation and various root-mean-square measures.

<sup>11</sup>Note that we optimize the model to account for aggregate violence statistics in each period, given that data are too sparse for a year-by-year matching; however, optimizing for subperiods also yields parameters consistent with those obtained for the aggregate statistics. See Section A.4.1 in the supporting information.

<sup>12</sup>See Section A.3 in the supporting information.

### 3.3 The Empirical Context: Segregation and Violence in Jerusalem

One Palestinian male was physically assaulted by Israeli settlers, who entered Jabal al Mukaber village and stoned Palestinians and their properties in response to the killing of eight Israelis on 6 March by a resident of the village.<sup>13</sup>

Tension ran high this week in the Sheikh Jarrah neighbourhood in East Jerusalem following the 2 August evictions of the two extended Hanoun and Al Ghawi families (nine family units) from two residential structures. Several confrontations occurred during the week between Palestinian residents of the neighbourhood and the residences' new Israeli occupants, with Israeli settlers harassing Palestinian residents of the neighbourhood, throwing stones, physically assaulting pedestrians, and in one incident, firing live ammunition into the air. On two occasions, unarmed clashes occurred between Palestinians and Israeli settlers resulting in the injury of five Palestinians and one Israeli settler.<sup>14</sup>

As these anecdotes illustrate, Jerusalem is among the most contested cities in the world, characterized by an unremitting struggle for territorial control—neighborhood-by-neighborhood and even house-by-house. Since the British control of Palestine (1917–1948), the city's geography has evolved from a unified, multiethnic entity to one that is physically, ethnically, and politically divided. Following the 1967 war and annexation of approximately 70 square kilometers to the east, north, and south of what was formerly Jordanian Jerusalem, all of the city's 77 neighborhoods fell under exclusive Israeli control. Widespread construction of new Jewish settlements around the city, facilitated in no small measure by the expropriation of nearly a third of all annexed territory, resulted in a patchwork of ethnic neighborhoods (Bollens, 1998; Margalit, 2006; Romann, 1984, 1989; Romann & Weingrod, 1991), depicted in Figure 3.2.

Two of the neighborhoods populated by predominantly Secular/Moderate Orthodox Jews, *Pisgat Ze'ev* (neighborhood #4) and *Gilo* (#65), are in areas annexed to the city after the 1967 war. Ultra-Orthodox Jews, who traditionally clustered and continue to reside in densely populated neighborhoods in and around West Jerusalem's center, have also migrated to neighborhoods in East Jerusalem. As a result, two of the most heavily populated Ultra-Orthodox neighborhoods, *Ramat Haredi* (#6) and *Ramat Shlomo* (#8), are also in annexed areas. Palestinians tend to reside in East Jerusalem, though some reside in West Jerusalem, and others have recently been migrating to Jewish neighborhoods in the north, creating small but notable minority clusters, such as those in *Pisgat Ze'ev* (#4) and *French Hill* (#13).

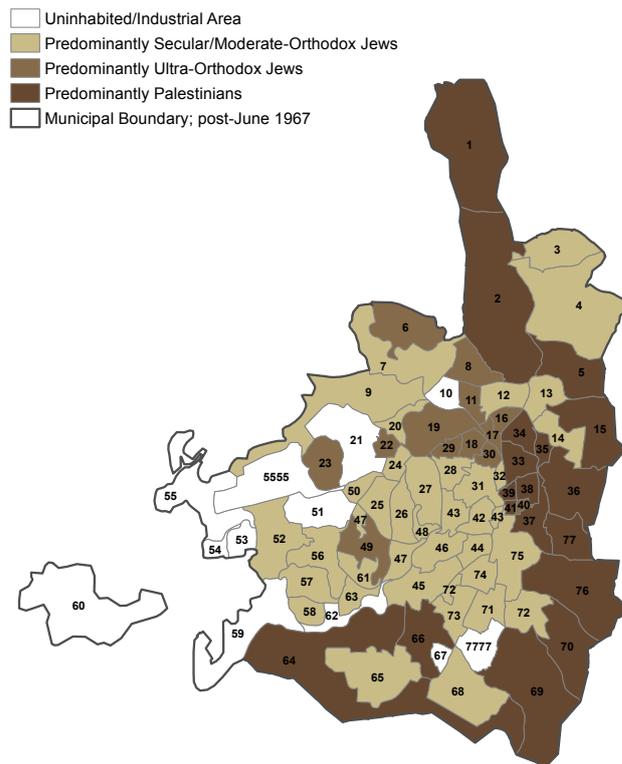
The recent construction of a barrier between Israel and the West Bank, which separates the city's Arab population from the Palestinian hinterland, has further altered Jerusalem's ethnic landscape by encouraging Palestinian Jerusalemites to resettle within the city's boundaries from the West Bank, overcrowding Palestinian residential areas and increasing intergroup animosity (Kimhi, 2008).

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<sup>13</sup>OCHA, Protection of Civilians Weekly Report, 12–18 March 2008.

<sup>14</sup>OCHA, 5–11 August 2009.

### Chapter 3. Group Segregation and Urban Violence



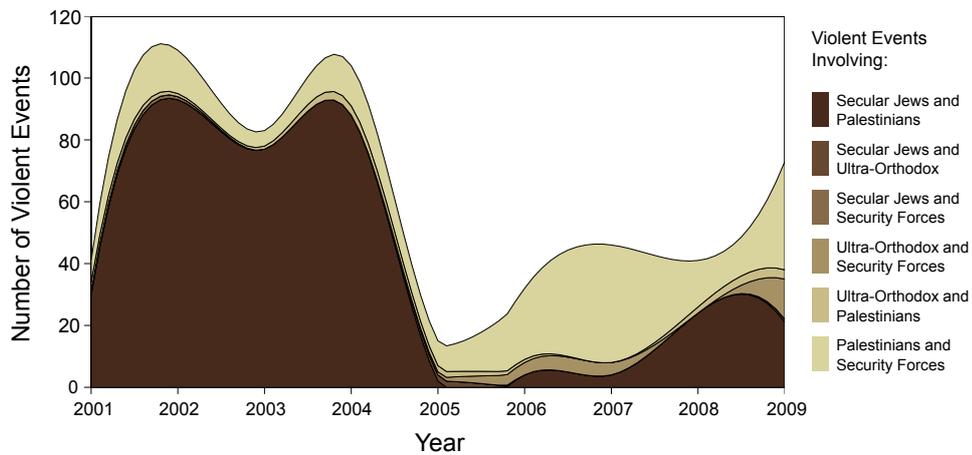
**Figure 3.2: Neighborhood Composition (2001–2009 Population Averages)**

Palestinian-Jewish civic relations are further strained by the asymmetric, disproportional distribution of public services and employment, as well as formal restrictions and pronounced inequities in the housing and construction sectors. The former has been exacerbated by the separation barrier, the latter exemplified by the expropriation of 40% of private land for public use and the inhibition of new Palestinian construction (Kaminker, 1997; Margalit, 2006). Indeed, discrimination of Palestinians by the Israeli state is repeatedly identified as a key conflict driver in Jerusalem (Margalit, 2006).

Policing also features prominently in Jerusalem. Non-resident Palestinians who wish to enter the city from the West Bank undergo physical checks at the separation-barrier checkpoints (OCHA, 2009). The Israeli Security Agency (i.e., Shabak) utilizes informants from Jerusalem’s Palestinian population to monitor political activity and conducts periodic arrests (Cohen, 2007). Barracks of the Israeli Border Police are stationed next to the former borderline, where Palestinian neighborhoods were taken over in north and south Jerusalem, as well as within the old city, in the Muslim and Jewish quarters (Israeli Police, 2012).

The scholarship on Jerusalem considers intergroup violence to be one of several aspects of Jewish-Palestinian and Secular-Ultra Orthodox relations (Hasson, 1996, 1999, 2007). Few studies focus on violence per se, much less its links to the social geography and contact between communities

### 3.3. The Empirical Context: Segregation and Violence in Jerusalem



**Figure 3.3: Empirical Perpetrator/Victim Dynamics, 2001–2009**

(see Hasson, 1996, 2001; Romann & Weingrod, 1991; Shilhav & Friedmann, 1997), with the exception of (Bollens, 1998, 2000), who examines how urban planning can intensify violence based on a comparison of Jerusalem, Johannesburg, and Belfast but stops short of probing the dynamics in depth. The question as to how further segregation of the city’s population or greater mixing will likely affect violence remains largely unaddressed.

In a concerted effort to study the spatial patterns of violence in Jerusalem, we consider murders, severe assaults (e.g., gunfire, stabbings, attempted suicide bombings) and minor assaults (e.g., stoning, throwing Molotov cocktails) within municipal boundaries and at permanent checkpoints on the city’s outskirts between 2001 and 2009.<sup>15</sup> Each event in our empirical data involves a member of a group—Secular/Moderate Orthodox Jews, Ultra-Orthodox Jews, Palestinians, security forces—attacking a member of another group.<sup>16</sup> The security forces are not a social group per se, but they represent an important actor in the conflict.

We consider two distinct time periods, 2001–2004 and 2005–2009, given an abrupt change in the nature of violence before and after 2004 in our empirical data (Figure 3.3). From 2001 to 2004 (the Al Aqsa Intifada), violence occurred primarily between secular Jews and Palestinians, whereas violence between security forces and Palestinians accounts for the largest share of events between 2005 and 2009. In addition, the violence during the second period is not limited to a single, central conflict, but rather it is composed of multiple, local conflicts between different social groups. Consequently, the spatial nature of violence differs across the periods (Figures 3.4a and 3.4b). During the first period, most parts of Jerusalem were affected, with a total of 337

<sup>15</sup>Note that our definition excludes domestic violence and violence against property.

<sup>16</sup>Our data sources include the Israeli Police Statistics and Mapping; *B’Tselem*, the Israeli Information Center for Human Rights in the Occupied Territories; OCHA oPT, the UN Office for the Coordination of Humanitarian Affairs; AIC, the Alternative Information Center; as well as content analysis of all the daily issues of *Yediot Aharonot* from 2001 to 2009. These sources were used to (1) assemble a wide universe of events of deadly and nondeadly violence in Jerusalem; (2) cross-check and validate the coding of events; and (3) compensate for biases in the data introduced by relying on a single source. See Section A.1 in the supporting information.

incidents of violence occurring in 53 of the city's 77 neighborhoods. A majority of events occurred along the border separating predominantly Jewish areas in the West from largely Palestinian areas in the East. By contrast, the second period exhibited a reduced number of violent events, 207 in all, which affected only 37 of the city's 77 neighborhoods and were concentrated in the East.

### 3.4 Model Results

Figure 3.5 displays the subset of social distance and discrimination-parameter combinations that generate the best fits with respect to the empirical data on the locations of violence, the number of violent events per neighborhood, and the targets of violence by group.<sup>17</sup> A circle denotes the occurrence of a given parameter value within the subset; the larger the circle, the more frequent its occurrence. A narrow distribution of values (i.e., fewer and larger circles) suggests that a parameter is particularly relevant for generating a good model fit; values of the best-fit parameter vector are circled in bold. As an indication of the internal validity of the mechanisms underlying our model, these values are both theoretically plausible and consistent with observed levels of intergroup tension and discrimination in Jerusalem: low social distance between Jewish groups, with considerably higher levels between Jews and Palestinians; high distance between Israeli security forces and Ultra-Orthodox Jews, reflected in the latter's relatively high perception of discrimination; even higher levels of discrimination and distance on the part of Palestinians, but little or no discrimination perceived by Secular/Moderate Orthodox Jews. Temporal and spatial slicing of the data set provides further confirmation that our model wields considerable in-sample predictive power. We further establish the significant value added of our model relative to a simple statistical (baseline) model that predicts future violence based on past violence.

The distributions of violence generated by the best-fit parameters underscore the internal validity of the model (Figures 3.4c and 3.4d). Our simulations accurately reproduce the occurrence of violence in 59 of 77 neighborhoods (76.6%) for the 2001–2004 period and in 64 of 77 neighborhoods (83.1%) for the 2005–2009 period (Figures 3.6a and 3.6b) and match the citywide distribution of targets for each group with high precision. The correlations between the simulated and actual numbers of violent events in neighborhoods are 0.33 and 0.65 for the 2001–2004 and 2005–2009 periods, respectively; the considerably higher quantitative agreement of the model in the latter period is a consequence of our model's ability to better capture spatially localized violence dynamics. The per-neighborhood predictions lie within two standard errors of the empirical data for all but three neighborhoods during the first period and all but four neighborhoods during the second period.

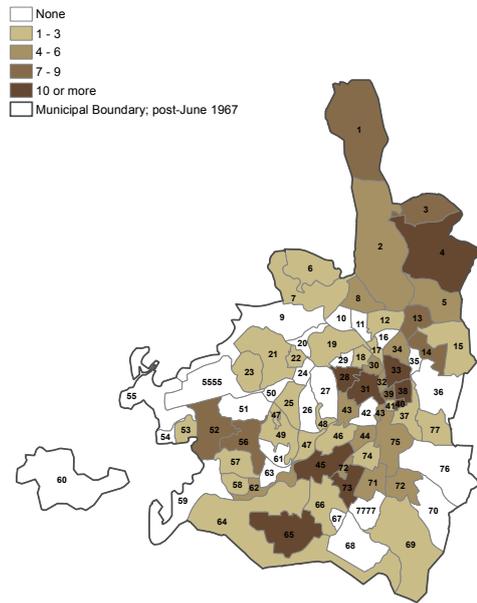
Overpredictions of the severity of violence during 2001–2004 were concentrated either in predominantly Jewish or Palestinian neighborhoods in East Jerusalem or along the pre-June 1967 East-West border, whereas notable underpredictions for the same period were observed principally in the Jewish neighborhoods of West Jerusalem (Figure 3.6c). These disparities are often

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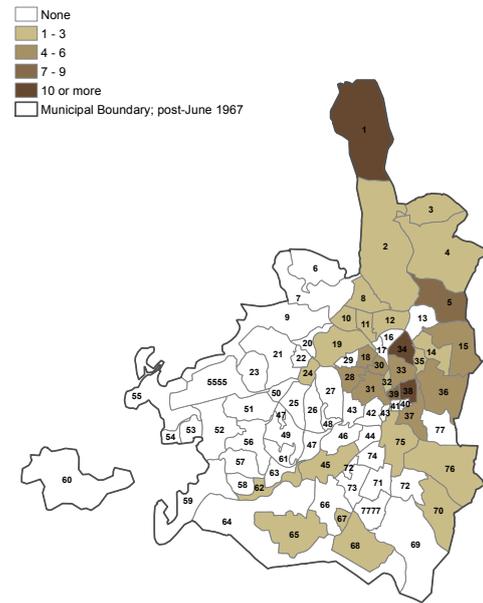
<sup>17</sup>Our approach follows Weidmann & Salehyan (2013). Here, we discuss results for the 2005–2009 period; corresponding results for 2001–2004 may be found in Section A.4.3 in the supporting information.

### 3.4. Model Results

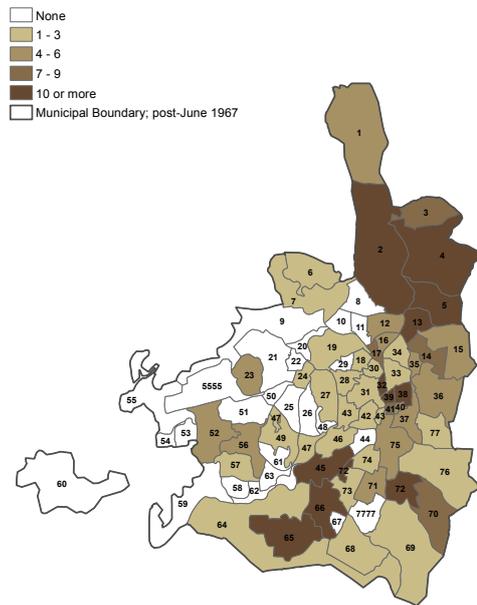
(a) Empirical, 2001-04



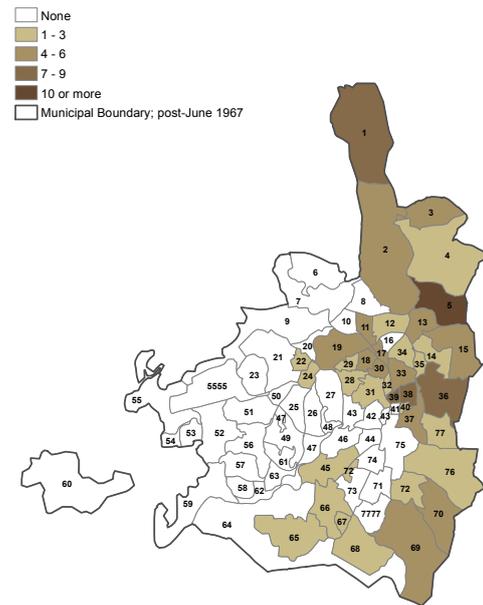
(b) Empirical, 2005-09



(c) Simulated, 2001-04



(d) Simulated, 2005-09



**Figure 3.4: Empirical and Simulated Results: Number of Violent Events by Neighborhood**

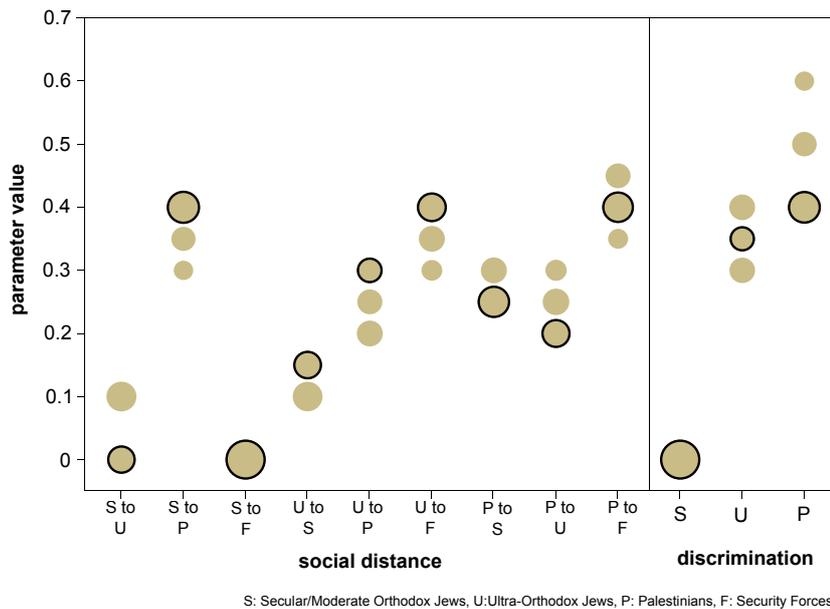


Figure 3.5: Distribution of Parameter Values in the Subset of Good Fits

consistent with aspects of the second Intifada that the model does not explicitly account for, including clashes over symbolic areas such as the old city and the Jewish city center and the fact that during the Intifada many individuals perpetrated violence in locations distant from where they resided. Notable overpredictions during 2005–2009 were observed for the southern and northern parts of the city (mostly in East Jerusalem), whereas underpredictions were clustered around the city center and in the *Atarot* neighborhood (#1) (Figure 3.6d). Both areas of the city are highly symbolic, with violence in the city center often triggering a response in *Atarot* and vice versa—nonlocal dynamics our model does not explicitly account for.

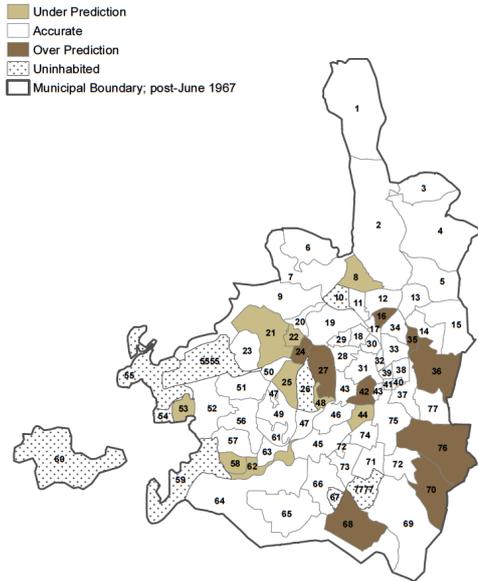
### 3.5 The Virtual Futures of Jerusalem

With confidence in the fidelity of our model, particularly in the recent post-Intifada period, we next undertake an exercise to estimate the expected impact on patterns of violence of alternative arrangements for dividing the city—the status quo (or *Business as Usual*), a *Return to pre-1967 borders*, the *Clinton Parameters*, and a *Palestinian Proposal*. Specifically, we explore (1) changes in the population structure; (2) variation in mobility within the city; and (3) the effects of the transfer of authority from Israelis to Palestinians. Simulations are used to generate corresponding counterfactuals, each of which is compared to a reference scenario based on the best-fit run for the 2005–2009 period. We report mean counterfactual trends,<sup>18</sup> illustrated by representative runs (Figure 3.7). Generally, we anticipate observing several patterns in the counterfactuals. One hypothesis is that levels of violence will be lowest for those measures that go the furthest in

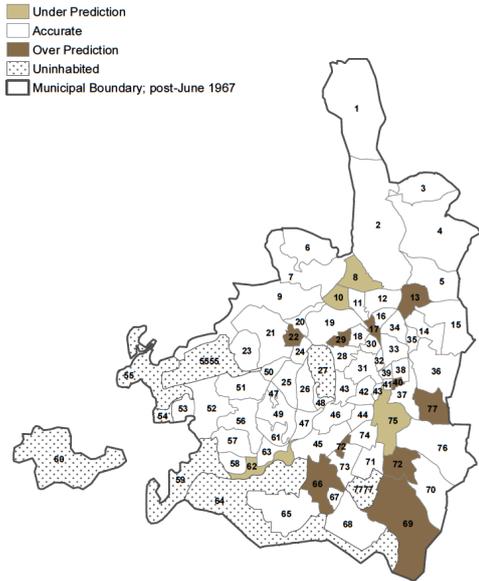
<sup>18</sup>To account for the influence of randomness in the model on the (potential) course of events, we simulate 100 realizations of each scenario that only differ in their random seed and report the average trends.

### 3.5. The Virtual Futures of Jerusalem

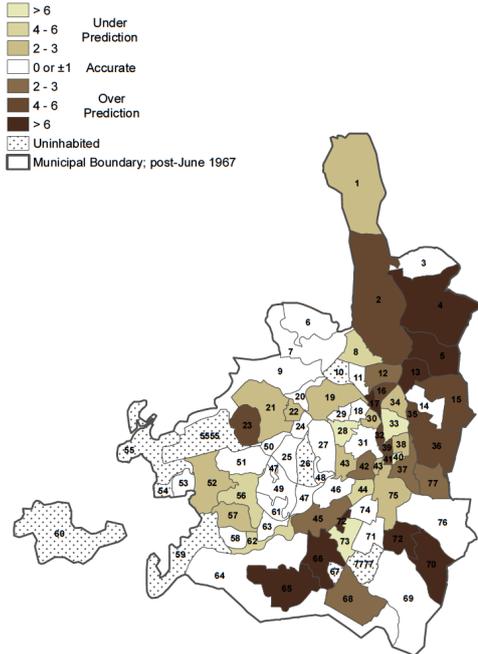
(a) Violence Onset, 2001-04



(b) Violence Onset, 2005-09



(c) Violence Magnitude, 2001-04



(d) Violence Magnitude, 2005-09

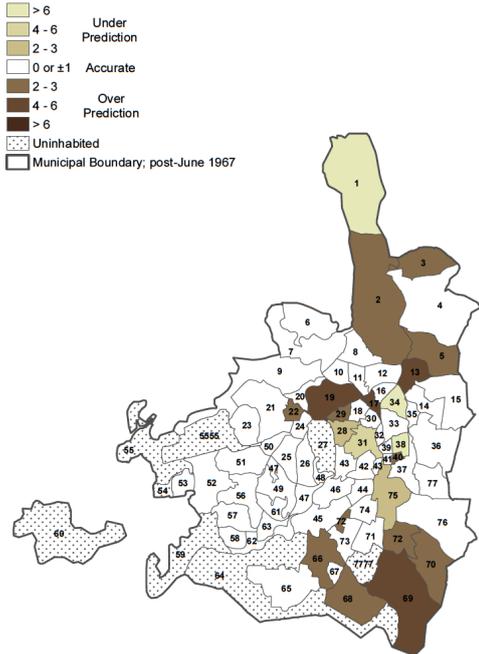


Figure 3.6: Comparison of Empirical and Simulated Data

## Chapter 3. Group Segregation and Urban Violence

segregating groups, given high levels of social distance and, hence, intergroup tension. Another hypothesis is that Jewish-Palestinian violence would occur primarily along new dividing lines. Table 3.2 summarizes both the structure of our experiments and associated results.

### 3.5.1 Business as Usual

The first counterfactual adopts Israel’s official stance on the future of Jerusalem, whereby Israel would retain full sovereignty over the city, maintain current municipal boundaries, and continue to encourage Jewish migration to East Jerusalem.<sup>19</sup> We start with the 2008 population for each neighborhood and then implement changes to reflect a preference for the Palestinian population to reside in the East, an increase in the growth of the Ultra-Orthodox population, and migration to neighboring Secular/Moderate Orthodox quarters. We further assume the continued expansion of Jewish settlement in the old city. Therefore, the scenario explores the impact of structural change and migration patterns within the city.

The Business as Usual counterfactual yielded a marginal increase in the frequency of violence (+6%), spread across a modestly greater number of neighborhoods (+3%) relative to the 2005–2009 reference scenario (Figure 3.7a).<sup>20</sup> The brunt of the impact continues to be in East Jerusalem (70% of the violent neighborhoods, of which 61% are predominantly Palestinian and 39% are predominantly Jewish), where the frequency of violence is significantly higher than in West Jerusalem. Thus, this scenario suggests that a future in which Israel continues to exert control over the entire city and continues its current policy would result in a modest increase in violence. While some new violence is also observed in Jewish neighborhoods in West Jerusalem, neighborhoods in the East would be most noticeably affected.

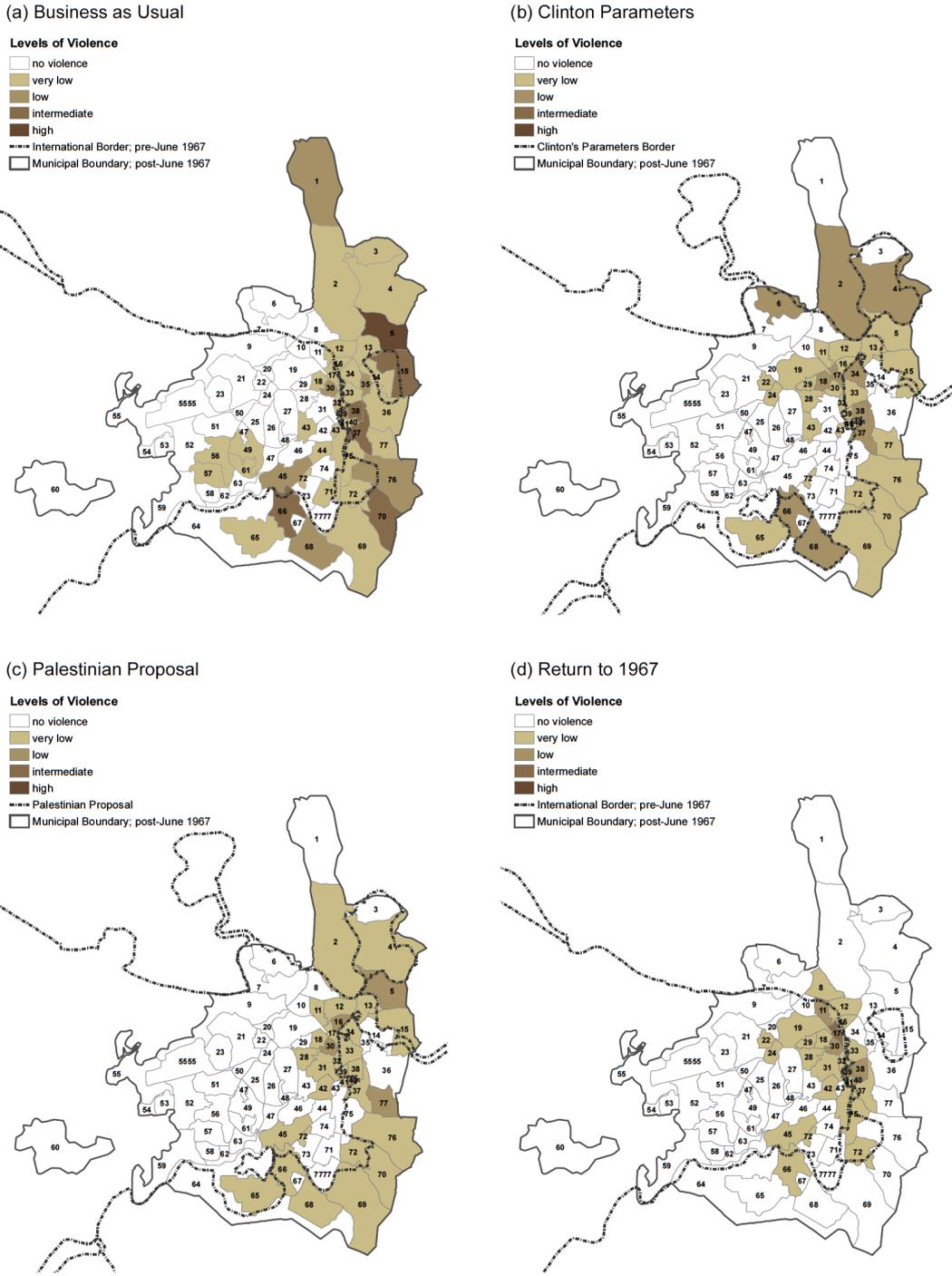
|                                 | <b>Business as Usual</b>          | <b>Clinton Parameters</b>                     | <b>Palestinian Proposal</b>                   | <b>Return to 1967</b>  |
|---------------------------------|-----------------------------------|---|---|--|
| Dimensions of change            | population structure and mobility | population structure, mobility, and authority | population structure, mobility, and authority | major changes to population structure, mobility, and authority |
| Number of Violent Neighborhoods | + 3%<br>(std. 8%)                 | – 10%<br>(std. 9%)                            | – 19%<br>(std. 9%)                            | – 32%<br>(std. 9%)   |
| Number of Violent Events        | + 6%<br>(std. 8%)                 | – 33%<br>(std. 8%)                            | – 42%<br>(std. 8%)                            | – 52%<br>(std. 8%)   |

**Table 3.2: Overview of Counterfactual Scenarios.** Results are relative to a baseline provided by the reference scenario depicted in Figure 3.4d.

<sup>19</sup>*The Jerusalem Post*, May 12, 2010.

<sup>20</sup>Relative to the reference scenario, we find no significant difference with regard to violent and nonviolent neighborhoods (McNemar test  $p > 0.1$ , using a binomial distribution).

### 3.5. The Virtual Futures of Jerusalem



**Figure 3.7: Policy-Relevant Counterfactual Results.** The categories of violence in these figures are comparable to those of the reference scenario (3.4d); we use qualitative categories to emphasize that the figures demonstrate forecasts of general trends and are not precise predictions of the expected number of violent incidents by neighborhood.

### 3.5.2 Clinton Parameters

The second counterfactual captures the idea that predominantly Palestinian and predominantly Jewish areas should be annexed by their respective states as part of a peace agreement.<sup>21</sup> The implication is that the city remains integrated with no territorial exchange, albeit with authority in significant parts of East Jerusalem transferred to the Palestinians, excluding Jewish neighborhoods that would remain under Israeli sovereignty. This de facto division of the city would limit mobility between Palestinian and Jewish neighborhoods, with any further migration preserving this division. Thus, the scenario goes beyond the previous counterfactual in exploring not only structural change and a major shift in mobility but also the potential impact of a transfer of authority. The simulation results exhibit a reduction in the number of violent events (–33%) and violent neighborhoods (–10%) relative to the reference scenario (Figure 3.7b).<sup>22</sup> Violence tends to be clustered in neighborhoods along the newly created divide and concentrated in areas under Israeli control (59% of violent neighborhoods), including parts of East Jerusalem that would be annexed to Israel as part of the agreement (26% of violent neighborhoods). The frequency of violence in East Jerusalem is nearly twice what is observed in West Jerusalem,<sup>23</sup> though well below the level of the reference scenario.

### 3.5.3 Palestinian Proposal

The third counterfactual is based on recent media revelations of an unofficial Palestinian framework.<sup>24</sup> The key details mirror the Clinton Parameters with several notable exceptions: (1) a strict division between East and West Jerusalem that would limit mobility; (2) the dismantling of Jewish neighborhoods constructed after the Oslo Accords, including the *Har Homa* neighborhood (#68) in Southern Jerusalem, which would be placed under Palestinian authority; and (3) as a concession to Israeli interests, Palestinian agreement to relinquish control over the controversial settlement *Shimo'n Hatzadik* in the *Sheikh Jarrah* (#34) neighborhood, including the nearby sacred graves and the Jewish and Armenian quarters in the old city. In line with the Clinton proposal, authority in East Jerusalem would be transferred to the Palestinians, including the responsibility for guaranteeing public security.

The simulation results (Figure 3.7c) indicate a more substantial decrease in violence relative to the reference scenario than in the *Clinton Parameters*, both in the number of violent events (–42%) and the number of violent neighborhoods (–19%).<sup>25</sup> Most of the violence would continue to appear along the inner-city divide and in areas under Israeli control (55% of the violent neighborhoods), including several of the Jewish enclaves in East Jerusalem that would be annexed and under

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<sup>21</sup>Israeli Ministry of Foreign Affairs, December 23, 2000.

<sup>22</sup>McNemar test  $p > 0.1$  indicates no significant difference relative to the reference scenario.

<sup>23</sup>East Jerusalem here still refers to the common distinction based on the 1967 boundaries; considering the redrawn boundaries in this scenario, the frequency of violence in the Israeli-controlled areas is around 40% higher than in the Palestinian-controlled neighborhoods.

<sup>24</sup>*The Guardian*, January 24, 2011.

<sup>25</sup>McNemar test  $p < 0.05$  indicates a significant difference relative to the reference scenario.

Israeli control (26% of violent neighborhoods). The frequency of violence in neighborhoods controlled by Israel is more than 30% higher than what is observed in the Palestinian-controlled areas. In sum, violence falls substantially but is not eradicated, and its locus shifts to the newly created boundary.

#### 3.5.4 Return to 1967

The fourth counterfactual approximates the official Palestinian position.<sup>26</sup> The centerpiece of this plan involves repartitioning the city along the borders of June 5, 1967, leading to a strict separation between East and West Jerusalem, with the East under Palestinian administrative and security control. Jewish neighborhoods in East Jerusalem would be dismantled and handed over, with the residents being relocated to West Jerusalem or to other Jewish cities, permitting the relocation of Palestinians to the vacated neighborhoods from other parts of Jerusalem as well as from the West Bank. A special international regime would be established to govern the *Old City* (#38- 41) and the *Mount Scopus* (#14) neighborhoods. The scenario reflects the most significant structural changes considered together with the most stringent restrictions on mobility; it also goes furthest with regard to transferring authority to the Palestinians.

The simulation results indicate a substantial reduction in violence: 52% fewer events and 32% fewer neighborhoods affected, relative to the reference scenario (Figure 3.7d).<sup>27</sup> Most of the violent neighborhoods are located along the reestablished inner boundary. A majority of these neighborhoods fall to the West of the new divide (52%). The frequency of violence in Israeli-controlled West Jerusalem is modestly higher (+10%) than what is observed in Palestinian-controlled East Jerusalem. Thus, a return to the 1967 boundaries can be expected to significantly reduce the points of friction and to decrease, but not eliminate, incidents of violence.

#### 3.5.5 Discussion

The results of the counterfactual analyses largely conform to our expectations, with some notable differences in the location and frequency of violence. In contemplating what is driving these results, it is crucial to consider the implications of the different alternatives for where people are allowed to go and live and those with whom they can conceivably come in contact with, including security forces. Because mobility is restricted in the *Return to 1967* and the *Palestinian Proposal* counterfactuals, the probability of residents of East and West Jerusalem interacting with one another is greatly reduced. Consequently, intergroup contact between Jews and Palestinians would be lower, relative to the reference scenario. Both of these counterfactuals also partition the city and limit migration options, such that Palestinians are confined to East Jerusalem and Jews to West Jerusalem. While the *Clinton Parameters* counterfactual involves fewer formal, strict constraints, in practice Palestinian access to majority Jewish neighborhoods would be

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<sup>26</sup>Haaretz, August 8, 2010.

<sup>27</sup>McNemar test  $p < 0.005$  indicates a highly significant difference relative to the reference scenario.

### Chapter 3. Group Segregation and Urban Violence

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lower relative to the reference scenario. Furthermore, in all three of these counterfactuals, East Jerusalem neighborhoods lie under Palestinian authority, thereby reducing friction between Palestinian civilians and Israeli security forces.<sup>28</sup> The *Business as Usual* counterfactual differs qualitatively from these previous scenarios, as the effort to expand the Jewish presence in East Jerusalem does not entail segregation and has the consequence of bringing more Jews and Palestinians into closer proximity.

Thus far, our counterfactual analyses rest upon the assumption that intergroup relations remain unchanged. Yet, the political wrangling behind the adoption of a particular policy for the city's future status may shift sentiments, with one group viewing the outcome as a victory or defeat. To develop an intuition for the degree to which the "futures" are contingent upon changes in intergroup relations, we explored a "worst" and "best" case realization of each scenario in which social distance between Palestinian and all Jewish groups was increased or decreased, as was discrimination toward Palestinians.<sup>29</sup> The analysis suggests that even small changes in intergroup relations profoundly alter the distribution of violence, albeit with a significant difference between the best and worst case, as these examples illustrate: in the best case, the *Clinton Parameters* scenario exhibits a decrease in the level of violence comparable to that of *Return to 1967*; in the worst case of the same scenario, however, any reduction in violence brought about by a repartitioning of the city is offset by deteriorating intergroup relations; in the best case, the *Business as Usual* scenario sees a reduction in violence similar to that observed in *Return to 1967*; whereas the worst case of the same scenario exhibits a sizeable increase in violence.

The results from our counterfactual analysis of Jerusalem are instructive in relation to debates about intergroup relations, peace building, and contact theory because they underscore the notion that the level of intergroup contact alone is insufficient to explain violence. These findings indicate that the effect of structural changes—segregation in particular—on violence depends decisively on levels of intergroup tension, i.e., social distance.

### 3.6 Conclusion

This study is motivated by the desire to better understand the relationship between factors that affect the extent of intergroup contact, including residential segregation, and spatial patterns of intergroup violence in urban areas. A vibrant, ongoing debate in the literature, to which this study contributes, is whether the basic tenet of contact theory is true: do measures that foster proximity and engagement between different groups curb or exacerbate the incidence, frequency, and severity of intergroup violence? And should nominal rivals then be kept separate, or instead more closely integrated?

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<sup>28</sup>Across all groups in Jerusalem, approximately 1 in 1,000 simulated interactions is violent, primarily as a consequence of state policing. When intergroup tension is most elevated, as in Palestinian interactions with the Israeli security forces, this rate rises to 1 in 10. Thus, for members of nominally rival groups, our model effectively captures the notion that interaction may be hostile but nonviolent when the threshold to engage in violence is sufficiently high.

<sup>29</sup>See Section A.5 in the supporting information.

Our approach suggests that the answer depends on social distance: while changes in settlement patterns shape the distribution and intensity of violence, levels of intergroup tension effectively moderate this relationship. Thus, short of fundamental changes designed to ameliorate group relations—curbing Jewish expansion in the Old City and East Jerusalem, increasing spending to improve Palestinian living conditions, raising investment to boost employment and improve infrastructure in Palestinian neighborhoods, programs that foster tolerance and mutual respect—our results suggest that arrangements conducive to reducing the extent of intergroup interactions—including localized segregation, limits on mobility and migration, partition, and differentiation of political authority—can be expected to dampen current levels of violence. Given high social distances, the greatest benefits in terms of conflict mitigation are achieved with comprehensive strategies that would transform the current geography of Jerusalem. To be clear, we are agnostic about whether such a fundamental reconfiguration of the urban space in this city or any other is necessarily desirable, even leaving aside issues of feasibility. This is especially the case, given our finding that even small changes in intergroup relations may profoundly offset any positive effects associated with group segregation.

Of course, reducing violence is a worthwhile ambition. Our mindset, in turn, is that decisions about peace building measures ought to be informed by reliable evidence, wherever available, about the repercussions for patterns of violence. Assessing the prospects of various political scenarios can present a challenge, given common inadequacies in the available data and hurdles to rigorously studying hypothetical scenarios. Therefore, we advocate using an empirically grounded agent-based approach to explore alternative scenarios that would otherwise not be quantitatively comparable. This powerful and versatile methodology is suited to simulate the geographically differentiated impact of different policy and programmatic options. It can do so in a manner that is amenable to calibration and validation and thus has real-world plausibility and applications. Our microlevel approach further reflects the limits of explaining violence exclusively through structural factors. Instead, we highlight the agency of individuals, who can have distinctive traits and exercise a degree of autonomy, but are also embedded within and influenced by a context that includes the residential landscape, their sphere of interpersonal interactions, and their links to social groups.

As with any modeling exercise, caveats are in order. Our use of an intentionally simple model of an otherwise complex environment yields a reliable match and meaningful interpretation of empirical data. Yet, we caution against reading too much into the numerical values of such results. Rather, it is the relative reduction in violence brought about by each alternative to the *Business as Usual* scenario that is noteworthy. We are, furthermore, fully aware that a sizeable proportion of violence is nonlocal in nature; that is, driven by the larger conflict at hand. And we have deliberately chosen to exclude political factors from the analysis, as well as income-based factors. Our effort highlights the plausibility of a simple, social distance-based mechanism—one that begins to untangle theoretical debates regarding the relationship between violence and the spatial separation of different groups.



# 4 Matched Wake Analysis: Finding Causal Relationships in Spatiotemporal Event Data<sup>†</sup>

## Abstract

This paper introduces a new method for finding causal relationships in spatiotemporal event data with potential applications in conflict research, criminology, and epidemiology. The method analyzes how different types of interventions affect subsequent levels of reactive events. Sliding spatiotemporal windows and statistical matching are used for robust and clean causal inference. Thereby, two well-described empirical problems in establishing causal relationships in event data analysis are resolved: the modifiable areal unit problem and selection bias. The paper presents the method formally and demonstrates its effectiveness in Monte Carlo simulations and an empirical example by showing how instances of civilian assistance to US forces changed in response to indiscriminate insurgent violence in Iraq.

## 4.1 Introduction

The study of political violence has benefited in recent years from a rapid increase in the availability of conflict event data sets (Raleigh et al., 2010; Sundberg et al., 2010). In these data, single instances of violence are coded together with their geographic coordinates and the date they occurred on. Several recent publications have successfully shed light on some of the micro-dynamics of civil conflict by analyzing such data (for example Buhaug, 2010; Hegre et al., 2009; O’Loughlin & Witmer, 2011; Raleigh & Hegre, 2009). However, while progress has been made in relating conflict intensity to geographic conditions, more complex endogenous mechanisms that drive conflict at the micro-level remain largely elusive to quantitative analysis, despite their theoretical prominence (e.g. Kalyvas, 2006).

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<sup>†</sup>This chapter is an edited version of the following article: Sebastian Schutte and Karsten Donnay. (2014). “Matched wake analysis: Finding causal relationships in spatiotemporal event data.” *Political Geography* 41: 1–10. <http://dx.doi.org/10.1016/j.polgeo.2014.03.001>

## **Chapter 4. Matched Wake Analysis: Finding Causal Relationships in Spatiotemporal Event Data**

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To fill this gap, we introduce a novel approach to causal inference in disaggregated event data that combines two techniques for ensuring robust and clean causal inference: sliding spatio-temporal windows (Braithwaite & Johnson, 2012; Kulldorff, 1997) and statistical matching (Iacus et al., 2012; LaLonde, 1986; Rubin, 1973). The presented approach clears the path for answering a whole class of high-profile research questions regarding the causal effects of specific types of events on future events. To demonstrate this approach and its capabilities, we show that the experience of indiscriminate insurgent violence in Iraq has led civilians to collaborate with the US military.

While presented in the context of conflict research, this method could be equally applied in other quantitative fields of research that rely on georeferenced event data: Criminologists might investigate the effects of law enforcement activities on subsequent levels of crime. Epidemiologists could analyze the spread of infectious disease as a function of specific types of interaction between individuals.

This paper proceeds as follows: After discussing the existing research and its shortcomings in the next section, we introduce our methodological contribution in detail and use a series of Monte Carlo simulations to test its capabilities and limitations. After that, we demonstrate the method in an empirical example by analyzing the effects of indiscriminate insurgent violence on civilian collaboration with US troops in Iraq.

### **4.2 Abilities and limitations of existing approaches**

The theoretical prominence of endogenous conflict dynamics (Kalyvas, 2006) has motivated a number of empirical studies in recent years. In order to understand how past conflict events shape future levels of violence, a rapidly growing number of studies rely on newly available event data (see: Leetaru & Schrod, 2013; Raleigh et al., 2010; SIGACT, 2010; Sundberg et al., 2010).

In principle, event data reflect changes in the trajectory of conflicts brought about by specific incidents. Along these lines, research into the causes and effects of violence against civilians in civil war (Kalyvas, 2006; Lyall, 2009) and escalation dynamics (Haushofer et al., 2010; Linke et al., 2012; Jaeger & Paserman, 2008) has drawn on conflict event data. Several studies have used village-level counts of violent events to investigate whether indiscriminate incumbent violence has a deterrent or escalating effect on subsequent insurgent activity. Especially Lyall (2009) and Kocher et al. (2011) pioneered this type of analysis with innovative matching designs and villages as units of analysis.

However, in many situations such natural spatial units of analysis are missing. Some studies have circumvented this problem by relying on artificial units of analysis, such as grid-cell months, and aggregated event counts and covariates accordingly. While introducing these artificial units conveniently clears the way for econometric analysis, it also leads to two problems widely described in the methodological literature. First, if cells of arbitrary sizes are the units of analysis,

## 4.2. Abilities and limitations of existing approaches

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the number of available observations directly scales with the chosen cell size: the smaller the cells, the more observations. Of course, regular null hypothesis tests crucially depend on the number of available observations. As  $N$  increases, the standard errors tend to decrease and even the smallest empirical signals becomes statistically “significant”. A second problem extensively described in the geographic literature is the “modifiable areal unit problem” (MAUP), i.e. the fact that the selection of artificial cell sizes drives spatial inference (Cressie, 1996; Dark & Bram, 2007; Openshaw, 1984).

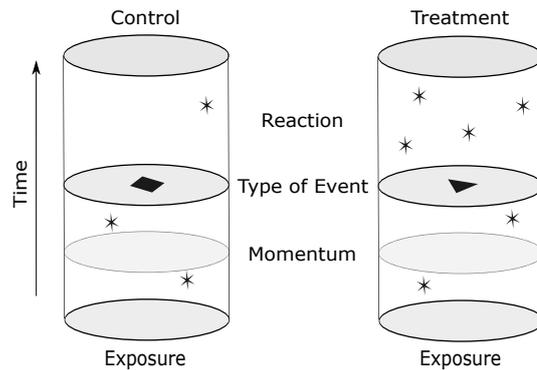
Approaches to overcoming the MAUP have been proposed in the past and also been applied in conflict research (O’Loughlin & Witmer, 2011). A commonly used method called “SaTScan” (Kulldorff, 1997) relies on sliding spatial and temporal windows to reveal clusters of events on different levels of aggregation.<sup>1</sup> Applied to epidemiology, SaTScan was originally introduced as a tool for testing whether a certain region faces an elevated per capita risk of disease. The method provides a fast assessment of whether event clusters could have been brought about by chance under corresponding distributional assumptions. To establish a baseline level of clustered events, SaTScan applies a simulation technique: For each size of the spatiotemporal window under consideration, the software allocates events randomly in space and time. Repeating this process in multiple iterations generates a distribution of simulated events under baseline assumptions. Significant empirical deviations from this baseline can then be identified for different cell sizes. In other words, comparing the distribution of artificial events to the empirical record yields an estimate of how likely is it that observed clustering was brought about by chance.

In the epidemiological case of Kulldorff (1997), this baseline is well justified as it assumes a constant per capita rate of instances of non-infectious disease. In conflict settings, however, finding suitable baselines is usually much more difficult. Instances of insurgent violence, for example, are likely to result from a host of factors, including geographic exposure and reaction to previous violence. Randomly allocating events in space and time might not adequately capture plausible counterfactual scenarios: Instances of violence against civilians, for example, might be simulated to take place in uninhabited areas and a simulated baseline would not reflect the causal order of events found in the empirical record.

Relaxing the assumption of a uniform spatial distribution of events, Braithwaite & Johnson (2012) apply a permutation test within the framework of sliding spatiotemporal windows to the analysis of violent events in Iraq. In this setup, a random baseline is also simulated, but not by relocating conflict events in space and time. Instead, events remain in their original positions but event categories are randomly swapped. By holding constant the location and timing of events while changing event categories, a baseline scenario can be established in which event types are independent of one another. Comparing this simulated baseline to empirical distributions of event categories shows whether or not specific classes of events tend to occur together, i.e. in clusters that are unlikely to have been brought about by chance. However, this measure of systematic co-occurrence, as well as SaTScan’s identification of event clusters, does not establish a clear

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<sup>1</sup>For another approach to identifying event clusters see Leslie & Kronenfeld (2011).



**Figure 4.1:** Illustration of the empirical strategy. Conflict events are divided into two classes of “treatment” and “control” events. For each event, previous levels of “dependent” events and their temporal trends and subsequent levels are established in an automated GIS analysis.

causal relationship between the event types.<sup>2</sup> We therefore decided to introduce a new framework for inferential analysis in conflict event data.

In the following section we describe a new method called *Matched Wake Analysis* (MWA) for finding causal relationships in event data that combines the best of the two most promising techniques reviewed above: sliding spatio-temporal windows to overcome the MAUP and statistical matching to allow for clean causal inference.

### 4.3 Matched wake analysis

Any attempt to overcome the discussed methodological shortcomings in the analysis of causal relationships in conflict event data must start with a theoretical understanding of the data generating process. A first crucial insight is that events come into existence through a variety of different mechanisms. In conflict research, there is the widely described effect of exogenous geographic conditions that drive overall levels of violence (Hegre et al., 2009; McColl, 1969; O’Loughlin & Witmer, 2011; Raleigh & Hegre, 2009). For example, strategic locations might see higher levels of violence. Ethnic settlement patterns have been linked to conflict events in Iraq (Weidmann & Salehyan, 2013) and in Israel (Bhavnani et al., 2014). For conceptual clarity one can refer to these factors as the a priori *exposure* of any location to violence. Furthermore, levels of violence generally vary over time. A negotiated ceasefire and seasonal cycles may drive the intensity of conflict across a war zone. These aspects can be referred to as the *momentum* of a conflict at any given time. Isolating the effects of exposure and momentum is a crucial prerequisite for cleanly analyzing the third mechanism driving levels of violence: *reaction* to specific events, i.e. the causal effect of specific interventions. Figure 4.1 illustrates the logic of this empirical strategy.

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<sup>2</sup>It should be mentioned, however, that SaTScan permits the simulation of non-uniform baselines which makes it a very versatile tool for the analysis of spatial event clusters.

In this conceptual sketch, three types of conflict events are depicted. The rectangular symbol in the center of the left cylinder represents an instance of violence assigned to the “control” category. The triangle in the right cylinder represents a “treatment” event and the star-shaped symbols represent events in the dependent category, which are possibly affected by treatment. In general, context information can be obtained with regard to *exposure* for both control and treatment events: spatial information such as local elevation (Gesch et al., 1999), natural land-cover (Hansen et al., 2000), the proximity of strategic locations such as the nearest international border (Weidmann et al., 2010), and the predominant ethnic group in the region (Wucherpfennig et al., 2011) can be calculated based on geocoded data.

Similarly, *momentum* of violence for all conflict events can be established by counting the number of previous dependent events. As Figure 4.1 indicates, the lower half of the cylinder is subdivided into two halves. A trend in the number of dependent events can be calculated. It is flat in both cases depicted here (one conflict event in each of the first two quarters of the cylinders). Of course, the quantity of interest in this setting is the number of subsequent events, i.e. the reaction to instances of treatment and control.

#### 4.3.1 Sliding window design

In principle, associating observations with static spatial covariates and dynamic counts of previous and subsequent dependent events would be sufficient to generate a statistical sample for subsequent analysis. This setup, however, still does not account for the MAUP since the size of cylinders in space and time cannot be identified based on theoretical expectations: Why should events at a distance of 20 kilometers be counted while events at a distance of 30 kilometers be excluded? It is exactly this type of arbitrary coding that Openshaw & Taylor (1979) have shown to obscure quantitative inference.<sup>3</sup>

As pointed out in the previous section, solutions to this problem have been identified in terms of sliding spatiotemporal windows. In this setup, the entire procedure of counting previous and subsequent events for every intervention is repeated for multiple sizes of spatiotemporal cylinders. This helps us to overcome the problem of inference hinging on arbitrary cell sizes and to distinguish among small- and large-scale effects empirically. For example, the effect of a treatment event on the level of dependent events might be stronger in its direct spatial and temporal vicinity and not affect more distant locations. Moreover, averaging the effects for different window sizes allows us to calculate a bottom-line effect.

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<sup>3</sup>Of course, applied researchers are not always in the comfortable position to have exact data on the locations of the events they study. Some data are only available on the level of administrative units or pre-aggregated into artificial cells. This methodological discussion is no way intended to discredit the corresponding studies, but merely an attempt to encourage researchers to use the full geographic information that is available to them.

## Chapter 4. Matched Wake Analysis: Finding Causal Relationships in Spatiotemporal Event Data

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### 4.3.2 Statistical matching

In the previous step, interventions were associated with counts of previous and subsequent dependent events for different spatiotemporal windows. Moreover, spatially referenced data – such as distances to major cities and population numbers in the area – were used to provide context information for each event. However, without explicitly accounting for confounding factors, causal inference in this setup can still be biased. In line with studies using natural spatial units of analysis (Lyall, 2009; Kocher et al., 2011), we apply statistical matching in order to compare treated and untreated observations under otherwise comparable conditions.

The general idea behind matching is to approximate as closely as possible experimental conditions in observational data (Rubin, 1973). Matching has become an important tool in the social scientific toolbox, although its effectiveness has been disputed (LaLonde, 1986). In experimental settings, treatment is applied randomly and its effects are observed in comparison to an untreated control group. Exactly this type of randomization that is so critical for unbiased inference is frequently absent in observational data. To emulate randomization, several techniques have been proposed. In the most simple setting, a large quantity of observations for both treatment and control are available and *exact matching* can be applied. In exact matching, only those observations are retained in the treatment group for which a corresponding observation can be found in the control group with identical numerical values for all relevant confounding variables. Exact matching entails that these observations only differ with regard to treatment being applied or not. Clearly, under such ideal conditions, the treatment effect can be directly estimated through the difference in means between the groups for the dependent variable (Iacus et al., 2012, 1). Unfortunately, such conditions are hard to find in practice. Usually, the confounding variables between treatment and control observations are comparable, but not completely identical. Several strategies exist to alleviate this problem. One approach is to capture the effect of the confounding factors on the probability of treatment assignment in a propensity score model (Rosenbaum & Rubin, 1983). Propensity score matching essentially amounts to predicting the probability of treatment assignment with a binary dependent variable regression model. The predicted probabilities of treatment assignment for each observation are used as the “propensity score” and observations from treatment and control group with similar scores are used in the subsequent analysis.

There is a practical problem associated with this technique for sliding spatiotemporal windows. A propensity score model requires as much care in post-estimation analysis as any other binary dependent variable model. Moreover, since the goal of matching is to increase *balance*, i.e. to make the empirical distributions of the covariates more similar, the balance has to be assessed for each covariate before and after matching. In practice, researchers have to go back and forth between propensity score model specifications and assess the improvements in balance. Poorly performing propensity score models can very well decrease the overall balance and therefore completely defeat the purpose of matching.

Clearly, a more robust and automated technique is needed for MWA: Due to the sliding window design, matching has to be performed repeatedly for all spatial and temporal parameter combina-

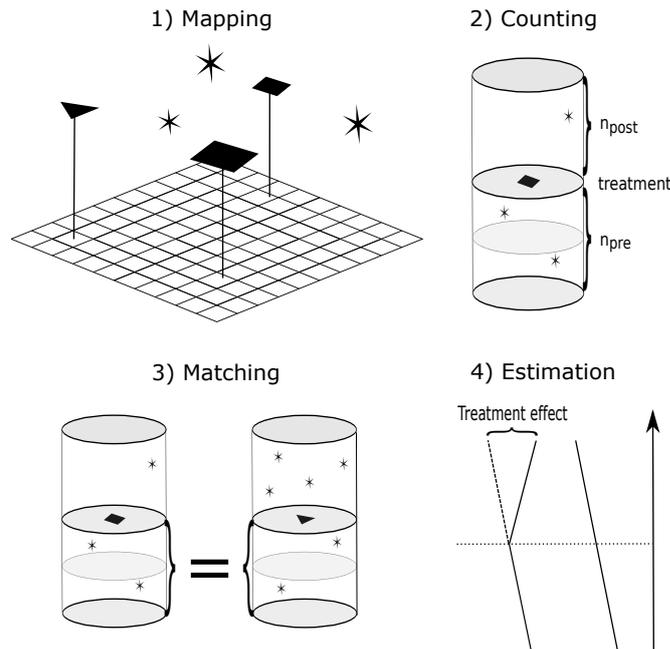
tions, and manual readjustments after post-estimation analysis are not an option. A very recent and computationally efficient automated matching technique alleviates this problem: *Coarsened Exact Matching* (CEM) (Iacus et al., 2012). In CEM, substantially identical but numerically slightly different values are collapsed into bins of variable sizes for each covariate. Matching is then performed for observations belonging to the same bins. Finally, a subsequent analysis can be performed for matched observations, but with the original numerical values. CEM generates well-balanced data sets by choosing bin sizes for different variables based on their empirical distributions. This method is much faster and more transparent than its alternatives and we therefore rely on CEM for automated matching.

#### 4.3.3 Estimation of causal effects

Several methods exist that are commonly used to estimate the causal effect of the treatment after matching is performed. For example, a Difference-in-Differences design (DD) (Angrist & Pischke, 2009) has been proposed and used in related empirical studies (Lyall, 2009). To assess the within-subject before and after change, DD performs an OLS regression on the matched data set to estimate changes in the number of dependent events brought about by the treatment. The dependent variable in this model is the number of dependent events after interventions. The number of dependent events before the intervention is also necessarily included in the model. Note that counts were aggregated for each of the pre- and post-intervention period which solves the problem of serial correlation that DD designs are otherwise prone to (Bertrand et al., 2004, 252). Moreover, the setup accounts for changing conflict dynamics unrelated to the interventions by matching on the trend in the dependent variable before interventions. The trend itself is calculated simply by subdividing the lower half of the spatiotemporal cylinder into two periods (see Figure 4.1). The resulting DD specification is then:

$$n_{post} = \beta_0 + \beta_1 n_{pre} + \beta_2 treatment + u \quad (4.1)$$

In this model,  $\beta_2$  is the estimated average treatment effect of the treated, i.e. the quantity of interest in the analysis. In the result presentation below, estimates for  $\beta_2$  are shown for each spatiotemporal window under investigation. We further provide detailed summary statistics for the matching procedure in terms of the multivariate *LI* imbalance measure and the percentage of common support (Iacus et al., 2012). *LI* is a multivariate distance metric expressing the dissimilarity between the joint distributions of the covariates in treatment and control groups. To calculate this statistic, the joint distributions are approximated in fine-grained histograms. Average normalized differences between these histograms are expressed in the *LI* statistic ranging from complete dissimilarity (1) to full congruence (0). A similarly intuitive measure is common support: It expresses the overlap between the distributions of matching variables for treatment and control groups in percent (Iacus et al., 2012). 100% common support refers to a situation where the exact same value ranges can be found for all matching variables in both groups. A formal description of *LI* and common support can be found Iacus et al. (2012).



**Figure 4.2:** Graphical overview of the MWA procedure: In a first step, observations are associated with geographic information via nearest neighbor mapping. After that, previous and subsequent instances of “dependent” events are counted. In step three, observations are matched with regards to previous events, event trends, and geographic information. The method of choice in this procedure is coarsened exact matching. Finally in step four, the treatment effect on the dependent variable is established in a Difference-in-Differences regression design for the matched sample.

In summary, a suitable setup for the causal analysis of conflict events has been sketched out in four steps. Intervention events are associated with geographic context information and counts of previous and subsequent events. After that, they are matched with regard to previous event counts, trends, and geographic variables. Finally, they are analyzed in a Difference-in-Differences regression design. Figure 4.2 provides a graphical representation of this procedure.

### 4.3.4 Limitations of the approach

While the underlying logic of matching designs is sound and widely used in empirical social science (see Abadie & Imbens, 2006; Diprete & Engelhardt, 2004; Herron & Wand, 2007), spatiotemporal data introduce potential pitfalls. Most importantly, the spatiotemporal cylinders around interventions can overlap partially. If they do, the “Stable Unit Treatment Value Assumption” (SUTVA) inherent to matching is violated. It states that the treatment effect of any observation should be independent of the assignment of treatment to other units (Cox, 1958). Violating this assumption can lead to biased estimates. Two MWA scenarios are imaginable in which the SUTVA assumption would be clearly violated. First, multiple treatment events could overlap in space and time. Assuming a positive treatment effect, the corresponding estimates are

likely to be biased upward in this scenario. Second, treatment and control events could overlap and thereby “water down” the treatment effect. In this case, the estimate for the treatment effect would be biased downward. To address this problem, we match on the number of intervention events that precede each intervention. This remedy and its effectiveness will be discussed in more detail below.

While SUTVA violations may indeed pose a problem to clean causal inference in MWA, there are ways to mitigate this problem. First, spatiotemporal overlaps are easily identified in empirical data. As described above, counting previous and subsequent instances of violence is part of the data preprocessing, and multiple instances of overlapping treatment and control events can be counted as well. The simplest way to avoid drawing false inference is therefore to check the data for overlaps of treatment and control events and select subsets that are not affected by this problem. For example, a civil war might go through phases of intense violence (e.g. summer offensives) and calmer periods, and researchers could test the causal effects of different types of events in the calmer periods to avoid false inference from overlapping events. However, empirical insights into the conflict dynamics would then, of course, be exclusively limited to such calmer periods instead of the entire conflict.

Second, if substantial numbers of overlapping cylinders cannot be avoided, data can still be analyzed using MWA. In this situation, the following problem has to be accounted for: Interventions of different types prior to the intervention under investigation can affect subsequent levels of dependent events. As a result, the causal effect attributed to the intervention would be in fact the product of a specific mix of different interventions (a double treatment, for example). A simple remedy in this situation is to match on the numbers of previous treatment and control events. This ensures that the interventions retained in the post-matching sample have similar histories of treatment and control events.

Another effect of matching on previous interventions is that non-overlapping treatment and control events have a higher probability of being selected into the post-matching sample. This is due to the fact that overlapping cylinders tend to differ with regard to the previous number of treatment and control events because the earlier event will be counted as a previous event for the later one. This effect leads to a matched data set with fewer overlapping events. A side effect of this approach is that it decreases overall balance between the treatment and control groups with regard to *exposure*, since overlapping events yield similar values for the related spatial confounding factors.

A third strategy is to simply remove overlapping observations from the sample. The obvious problem with this approach is the potential bias arising from non-random deletion itself. In a benchmark analysis using simulated data, we show that this strategy still performs better than the baseline method for smaller overlaps, but for larger overlaps the problems associated with non-random deletion are very noticeable. The strategy also appears to lead to less robust estimates for overlapping cylinders than matching on the number of previous treatment and control events. We demonstrate quantitatively in the next section how these remedies perform.

### 4.4 Monte Carlo simulations

In this section, we demonstrate the performance of MWA based on simulated event data. We rely on artificial data to maximize the transparency of the setup and generate benchmarks under controlled conditions that include simulated causal effects, but also random noise that can be expected in any empirical application. Two scenarios were used for the simulations. First, as a proof-of-principle, a treatment effect was established under ideal circumstances: Cleanly separated “treatment” and “control” events were analyzed under otherwise comparable conditions. Second, data with increasingly stronger overlaps were analyzed to illustrate the resulting biases. Remedies such as deletion of overlapping events and matching on previous intervention events were tested.

#### 4.4.1 Data generating process

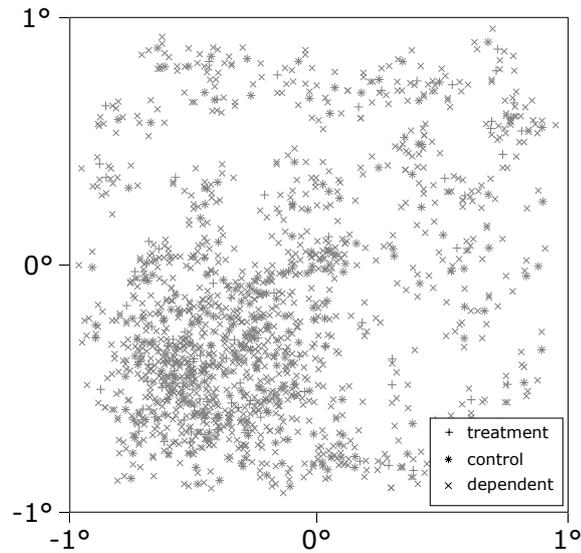
In order to emulate some of the empirical complexity of event data, we constructed artificial samples using three types of events. One type of event represents the “dependent” category and our quantity of interest was changes in the frequency of these events after interventions. The other two types are intervention events, which are labeled “treatment” and “control” in compliance with the matching terminology. The artificial causal effect was modeled in two steps. Events of the “dependent” category were placed prior to interventions and exhibited varying trends. Dependent events following interventions were placed in fixed temporal and spatial distances from the interventions.

The frequency of dependent events was increased such that one additional dependent event occurred after treatment. For events of the “control” category, the number of dependent events following interventions remained unchanged in comparison to the number of preceding events. An increase of one event is the smallest possible effect for discrete event counts and provides a difficult test situation: the larger the effect, the more easily it is recovered by the the method. Absolute counts and trends in dependent events were varied to increase the realism of the simulations. The data contained 200 “controls” and 100 “treatments”. This imbalance was intentionally chosen to emulate the complications of empirical data. We account for this difference by using weighted regressions for the DD analysis in the simulations and the empirical section.

For each intervention event, we also assigned two stylized confounding variables which were simply numerical values drawn from the same random distributions. For the simulation, we ignored the potential effects of confounding factors on the probability of treatment being applied, since they would be mitigated by the matching if they were present.<sup>4</sup> Artificial intervention events were distributed over a geographical region of 2 by 2 degrees around the Equator, which corresponds to an area of roughly 220 km by 220 km. Figure 4.3 depicts the spatial setup for the simulations. Of course, intervention events were separated temporally. By varying the simulated time period, the probability of events overlapping in this simulated setup could be adjusted: the

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<sup>4</sup>For more details on the generation of our test data, please refer to Section B.3.1 of the supplementary information.



**Figure 4.3:** Map of the simulated data distributed over the region within the 1st degree latitude North and South and the 1st degree longitude East and West, an area that corresponds to roughly 220 km by 220 km. This generic spatial setup was used for all Monte Carlo simulations.

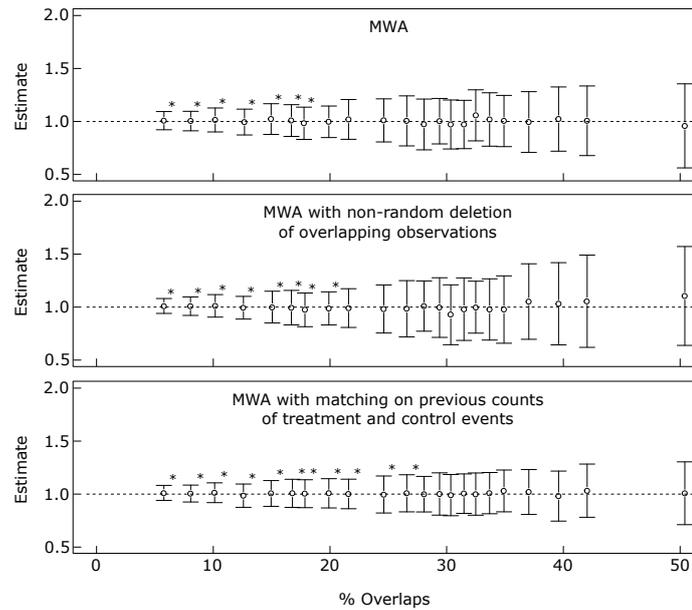
longer the simulated time span, the smaller the probability of overlaps. By varying the time span under investigation, we could assess the effects of increasing overlaps on the estimation of the treatment effect.

#### 4.4.2 Simulation results

To overcome MAUP, MWA establishes event counts and estimates for the treatment effect for different spatial and temporal cylinder sizes. The corresponding insights can be communicated graphically as a contour plot: The lighter the color the larger the estimated treatment effect ( $\beta_2$  in formula 1). The corresponding standard errors are indicated by shading out some of the estimates: No shading corresponds to  $p < 0.05$  for the treatment effect in the DD analysis. Dotted lines indicate  $p$ -values between 0.05 and 0.1 and full lines indicate  $p > 0.1$ . The cells indicating effect size and significance level are arranged in a table where each field corresponds to one specific combination of spatial and temporal sizes of the cylinders depicted in Figure 4.1 (see Figure 4.4).

To illustrate the ability of MWA to reveal the spatiotemporal distances at which reaction to intervention occurs, the dependent events after interventions (i.e. reactive events) were placed at distances of eight days and eight km. Figure 4.3 shows how the resulting clusters of events are distributed randomly in space. The probability of clusters overlapping was minimized as they were spread out over a temporal span of 20 years. In this case, clean causal inference is possible and the method clearly recovers exactly the simulated causal effect in the number of dependent events at eight days, eight km (Figure 4.4). Note that larger spatial and temporal window sizes





**Figure 4.5:** Average estimates with 95% confidence intervals as a function of the overlaps of the spatiotemporal cylinders. The graph shows estimates for MWA (top), MWA with non-random deletion of overlapping observations (middle), and MWA with matching on counts of previous treatment and control events (bottom). Asterisks indicate that all estimates for all simulated data sets were significant at the 0.05 level and the dotted line marks the true effect.

The true treatment effect in all simulations is 1 and indicated with a dotted line. Estimates for this true effect vary for the different simulation runs: Mean values are shown as circles and 95% confidence intervals are shown as whiskers. The asterisk above many data points indicates that all simulation runs yielded p-values smaller than 0.05.

The figure clearly indicates that all three methods produce correct estimates on average also for larger overlaps, but substantial differences exist when it comes to the reliability of the different approaches. For the normal MWA procedure, overlaps affecting up to about 20% of the observations still yield consistently significant results. For slightly higher levels of overlaps, deletions of overlapping observations more reliably produces correct p-values for the treatment effect, as shown in the middle panel. However, for highly clustered data, non-random deletion performs worse than standard MWA. The best results for all ranges of overlaps can be achieved by matching on counts of previous interventions. This approach is demonstrated in the lowest panel: For overlaps of up to 28%, the analyses correctly reveal a positive and significant treatment effect for all 100 simulated data sets at a given overlap. Moreover, confidence intervals are smallest for this procedure.

This analysis shows that the method robustly identifies the true causal effect for a given spatiotemporal lag for situations of moderate overlaps (up to 20%). In the cases of stronger overlaps,

does not strongly affect the results as shown in Section B.3.2 of the supplementary information.

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matching on the number of previous treatment and control events improves the accuracy of the estimated treatment effect, in line with our theoretical arguments in section 4.3.4 but only to a point: Beyond 25-30% overlaps, inference becomes less robust.

In the next section we turn to our analysis of an empirical example and investigate the effects of insurgent violence on civilian cooperation with the US military in Iraq. Based on the results of our Monte Carlo simulations we use MWA with additional matching on previous treatment and control events for our empirical analysis.

### **4.5 Empirical case: civilian collaboration in Iraq**

This section demonstrates that MWA can provide substantive insights into the turmoil of civil conflict and the causal effects of specific types of events. The ongoing war in Iraq was identified as a suitable test case as it lends itself both conceptually and empirically to testing micro-level hypotheses.

After the 2003 US-led invasion, the country went through several phases of intense political violence. Following the initial occupation in 2003, a low-level insurgency developed and grew in subsequent years. This sequence of macro-events is typical of a wider class of cases: A government is replaced through outside intervention and subsequent occupation of the country. The new government faces a problem of legitimacy and is heavily reliant on outside support. Elements loyal to the former administration start a protracted campaign to topple the new incumbent.

An additional source of violence in Iraq were sectarian clashes between Sunni and Shia that intensified after the Al-Askari Mosque bombing in February 2006. In the following 24 months, sectarian violence escalated dramatically. During 2007, 20,000 additional US troops were deployed in the country to contain the escalating civil war and to strengthen the Iraqi security apparatus. During the same period, the Sons of Iraq movement began assisting incumbent forces in fighting foreign insurgents. During 2008 and 2009, violence against incumbent forces steadily declined, while sectarian tension continued to claim civilian lives.

In 2010, a large number of temporally and spatially referenced conflict events recorded by the US military were released to the general public through the online platform wikileaks.org (SIGACT, 2010).<sup>6</sup> Several inquiries into the conflict dynamics in Afghanistan and Iraq have been published recently that focus on the spatial and temporal distribution of conflict events (O'Loughlin et al., 2010), conflict dynamics (Linke et al., 2012), the clustering of conflict events in space and time

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<sup>6</sup>We decided that these illegally distributed data could be used in a responsible manner for basic research, given that the empirical analysis would not in any way harm or endanger individuals, institutions, or involved political actors. To ensure this, our analysis only focuses on the events in the statistical aggregate. Moreover, the matching design entails that no marginal effects are estimated for confounding factors, which further strengthens the anonymity of the findings. Based on these precautions, the ethics committee of ETH Zurich reviewed a proposal for this study carefully and then allowed it to proceed.

## 4.5. Empirical case: civilian collaboration in Iraq

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(Braithwaite & Johnson, 2012), and violence-induced migration (Weidmann & Salehyan, 2013). However, micro-level conflict dynamics and causal relationships between events remain heavily understudied.

Following a line of argument that predicts increased civilian collaboration with the strategic adversary in reaction to indiscriminate violence by either side, we assume indiscriminate insurgent violence to increase civilian collaboration with the US military in Iraq (see Kalyvas (2006, 144), Kocher et al. (2011); Linke et al. (2012); Ellsberg (1970); Mason & Krane (1989)). More specifically, we assume that civilians are more likely to deny insurgents access to explosives in response to indiscriminate violence. But how can such an expectation be tested empirically?

First, it is important to understand how a substantial fraction of insurgent violence was applied in Iraq. To compensate for the lack of heavy weaponry, Improvised Explosive Devices (IEDs) have been used against both military and civilian targets. In many cases, IEDs are military-grade explosives obtained from unexploded ordnance. These explosives are combined with improvised trigger mechanisms. Unlike landmines, many IEDs are attacker activated and can therefore be used both selectively against adversary combatants or indiscriminately against civilians.

Due to these technical particularities, obtaining unexploded ordnance is a crucial prerequisite for generating a constant supply of new IEDs. Confronted with unexploded ordnance, civilians face a strategic choice: They can either remain passive and thereby allow explosives to be obtained by insurgents, or they can turn in explosive remnants of war. Arguably, civilians will be more inclined to do so if other civilians have been harmed with IEDs in their spatial and temporal vicinity. We therefore test the following hypothesis: *Indiscriminate insurgent violence using IEDs increases civilian handover of unexploded ordnance to US troops compared to selective insurgent violence using IEDs.*

Testing this hypothesis based on MWA requires three event categories to be specified. First, the dependent variable has to be selected. In this case, instances of civilians *turning in* unexploded remnants of war is the dependent variable. The treatment category is *IED Explosions* that have led to civilian casualties, while events that have not claimed civilian lives are used as the control category. Instead of relying on exact casualty counts which might be difficult to obtain under wartime conditions, we relied on so-called “friendly force information requirements” that are associated with many SIGACT observations. We used this information to focus the analysis on events that the reporting unit classified as severe.

### 4.5.1 SIGACT data and event categories

The version of SIGACT (Significant Activity) files used for this study cover the time period from 2004 to 2009 and amount to 391,832 records. However, the data provide different spatial resolutions for different parts of the country: Events coded in the Baghdad region are coded with a spatial resolution of approximately 1 km while events for the rest of the country are only accurate to about 10 km.

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We decided to analyze the Baghdad subset of the data in MWA and focused on the last two recorded years (2008 and 2009). As mentioned above, the conflict went through numerous phases that can be roughly divided into an initial insurgency (2003-2006), sectarian civil war and the rise of pro-government militias (2006-2008), and a mixture of all of these conflicts with reduced intensity since 2008.

Especially the last phase of the war covered by the data (2008 and 2009) is suitable for testing the proposed hypothesis as events are not as densely clustered as during the most intense violence in 2006 and 2007. Moreover, collaboration with incumbent forces is more frequent than during the initial insurgency. In total, 2,484 events were used for testing the proposed mechanism in the 2008-2009 period for the Baghdad area. The substantive findings generalize well for the rest of the country, as shown in Section B.2.1 of the supplementary information in a separate analysis.

Civilian collaboration with US forces can be measured directly in the data set. Three event categories reflect direct civilian assistance in terms of civilians passing on information or turning in evidence or weapons.<sup>7</sup> We used instances of “turn in” (667 events in the sample) as the dependent type. To distinguish among two types of events that affect subsequent levels of civilian collaboration, IED explosions that harmed (killed or injured) at least one civilian were coded as “treatment” (254 incidents), and those that did not were used as “controls” (177 incidents). Figure 4.6 shows the geographic locations of events in the treatment, control, and dependent categories.

Generally, casualty reports in military data collection might be affected by biases. For example, soldiers might underreport civilian casualties that they have caused themselves, or give too optimistic accounts of enemy casualties. When it comes to civilian casualties caused by insurgents, there are no obvious incentives for misreporting in an internal data collection. We nevertheless use these data conservatively by focusing on reportedly severe incidents. This information was obtained from another field in the SIGACT data, the “friendly force information requirements”. We also used information on casualties conservatively and only checked whether or not civilian were harmed to code “treatment” and “control” events.

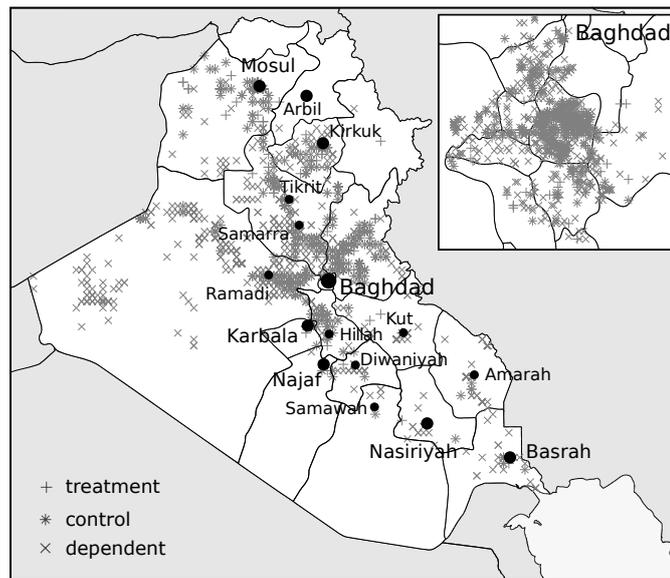
Geographic matching variables were coded for all SIGACT events under investigation. We obtained geocoded data on approximate population figures for the year 2000 (CIESIN, 2005), distances to Baghdad’s “Green Zone”, and stable nighttime light emissions for the year 2008 as a proxy for infrastructural development (NGDC, 2012). The ethnic composition of the neighborhood under attack could not be established based on existing data sources. For central Baghdad, Weidmann & Salehyan (2013) have coded time variant data on ethnic groups, but their data only cover a fraction of the greater Baghdad area under investigation.<sup>8</sup> Summary statistics for the matching variables can be found in Section B.1.2 of the supplementary information.

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<sup>7</sup>These categories are tagged as “turn in”, “explosive remnants of war/turn in”, and “erw/turn-in” in the SIGACT data.

<sup>8</sup>For the analysis of the whole country reported in Section B.2.1 of the supplementary information, we used data on ethnic settlement regions from Wucherpfennig et al. (2011)

## 4.5. Empirical case: civilian collaboration in Iraq



**Figure 4.6:** Map of Iraq and Baghdad showing the location of all events (treatment, control, and dependent) included in the analyses

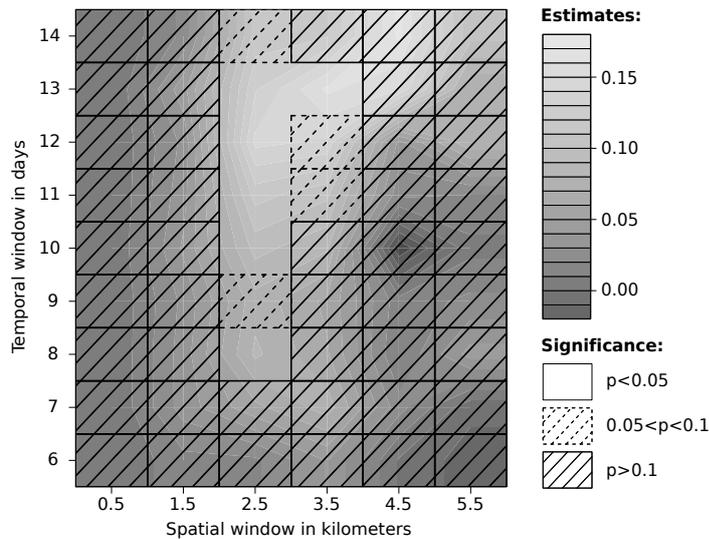
The spatial variables were coded through nearest neighbor mapping between SIGACT observations and the mentioned data sets. Beyond these variables, we also matched on the pretreatment trend in civilian assistance and previous “treatment” and “control” counts, which is in line with the previous discussion.

### 4.5.2 Empirical results

The results grant nuanced insight into how violence changes patterns of collaboration at specific temporal and spatial distances from the intervention. Figure 4.7 gives an overview of the central findings. Almost all estimates for all cylinder sizes are positive. As visible in the center of the plot, significant increases in collaboration occurred in response to IED attacks with civilian casualties in comparison to attacks that did not harm civilians. For distances of up to 2.5 kilometers from the incident, a robustly significant effect can be found for a range of temporal offsets from 8 to 14 days. Again, p-values are communicated as shaded areas in the plot. Table 4.1 also communicates the effects, as well as the fraction of incidents that have seen previous interventions numerically. Based on the almost exclusively positive estimates, we conclude that indiscriminate insurgent violence led to increased civilian collaboration with US ground forces in the later phases of the war in Iraq. This effect is present in the close spatial vicinity of the attack, but with a delay of one to two weeks.

While the effect is significant and robust, it is only moderately strong: For small spatial distances (between 1.5 and 2.5 kilometers) and temporal distances between 1 to almost 2 weeks after the

## Chapter 4. Matched Wake Analysis: Finding Causal Relationships in Spatiotemporal Event Data



**Figure 4.7:** Empirical results of the MWA analysis of civilian collaboration in Baghdad for the 2008-2009 period. The underlying contour plot shows the estimated effect of insurgent violence against civilians on civilian collaboration with the incumbent. Non-shaded areas are significant at  $p < 0.05$ , dotted lines indicate  $p < 0.1$ , and full lines indicate  $p > 0.1$ .

event, levels of civilian support of the treatment group are significantly higher than in the control group. The estimated treatment effect peaks at 0.16 (for 13 days and 3.5 km). Averaging over the interpreted effects, for every 100 IED attacks against civilians one would expect up to 12 more instances of civilian assistance to US ground forces. Of course, this insight only holds for the Baghdad area and the time period under investigation.

This moderate effect size is empirically plausible. Not every IED attack with civilian casualties would directly lead to an instance of collaboration. Civilians that are inclined to assist US forces would also have to know where unexploded ordnance can be found to actively assist US troops. Clearly, this condition is not met in all situations. It is more plausible that only some incidents happen under circumstances that allow civilians to actively support US troops. Moreover, the results indicate that reactions to insurgent attacks take place with a certain temporal delay that may result from the lack of opportunity to collaborate with US forces but may also reflect risk aversion. In order to conceal their assistance to incumbent forces, civilians might let a few days go by before approaching US troops.

Summary statistics for the matching procedure are presented in Table 4.2. The upper section of the table refers to the empirical sample before matching is applied. The lower section refers to the matched sample. The summary statistics that express the similarity of the joint distributions of the matching variables show a substantive improvement after matching. *Common support* doubles from approximately 25% to approximately 50% and the *LI* distance metric changes in similar magnitude. In summary, the automated matching procedure based on *Coarsened Exact Matching* proves very efficient in this case and substantively improves the balance of the sample.

#### 4.5. Empirical case: civilian collaboration in Iraq

| Time (days) | Space (km) | Treatment effect | P-value | SO   | MO   |
|-------------|------------|------------------|---------|------|------|
| 8           | 2.5        | 0.08             | 0.03    | 0.20 | 0.13 |
| 10          | 2.5        | 0.09             | 0.03    | 0.23 | 0.15 |
| 11          | 2.5        | 0.11             | 0.01    | 0.24 | 0.16 |
| 12          | 2.5        | 0.14             | 0.01    | 0.26 | 0.17 |
| 13          | 2.5        | 0.13             | 0.03    | 0.27 | 0.18 |
| 13          | 3.5        | 0.16             | 0.03    | 0.37 | 0.30 |

**Table 4.1:** Summary statistics for the interpretable areas of the contour plot in Figure 4.7. The estimated treatment effect for these statistically significant areas averages to 0.12. The acronym SO (“same overlap”) refers to situations where either the cylinders of two or more treatment events or two or more control events overlap. MO (“mixed overlap”) refers to situations where treatment and control cylinders overlap.

| Time (days) | Space (km) | Controls <sub>pre</sub>  | Treatments <sub>pre</sub>  | L1 <sub>pre</sub>  | %Support <sub>pre</sub>  |
|-------------|------------|--------------------------|----------------------------|--------------------|--------------------------|
| 8           | 2.5        | 171                      | 244                        | 0.50               | 26.20                    |
| 10          | 2.5        | 171                      | 242                        | 0.52               | 25.40                    |
| 11          | 2.5        | 171                      | 242                        | 0.52               | 27.40                    |
| 12          | 2.5        | 170                      | 242                        | 0.53               | 27.50                    |
| 13          | 2.5        | 169                      | 242                        | 0.53               | 25.70                    |
| 13          | 3.5        | 169                      | 242                        | 0.57               | 22.90                    |
| Time (days) | Space (km) | Controls <sub>post</sub> | Treatments <sub>post</sub> | L1 <sub>post</sub> | %Support <sub>post</sub> |
| 8           | 2.5        | 118                      | 160                        | 0.30               | 51.00                    |
| 10          | 2.5        | 118                      | 153                        | 0.32               | 51.90                    |
| 11          | 2.5        | 118                      | 151                        | 0.32               | 57.70                    |
| 12          | 2.5        | 118                      | 149                        | 0.33               | 57.10                    |
| 13          | 2.5        | 120                      | 152                        | 0.34               | 56.40                    |
| 13          | 3.5        | 116                      | 128                        | 0.32               | 50.00                    |

**Table 4.2:** Summary statistics for the matching procedure showing results for the interpretable areas of Figure 4.7. The upper half of the table refers to the original sample and the lower half shows summary statistics for the matched sample.

In the area of the substantive effect, the data include more instances of IED attacks that harmed civilians (~140) than those that did not lead to civilian casualties (118), but this slight difference in the number of corresponding observations is accounted for by the weighted regression.<sup>9</sup>

In summary, we find that there was a significant increase in civilian collaboration with US troops in Iraq during 2008-2009 as a result of insurgent IED attacks with civilian casualties: Up to 12 more instances of civilian assistance for every 100 indiscriminate IED attacks can be attributed to the presented mechanism. This effect is present in the close spatial vicinity of the attack, but with a delay of about one week.

<sup>9</sup>A robustness check reported in Section B.2.2 of the supplementary information without weighted regression leads to almost identical results.

### 4.6 Discussion and conclusion

In this paper, we have discussed the need for better methodology in the analysis of causal relations in conflict event data. Existing approaches based on inferential methodology only work reliably when data are available in natural spatial units of analysis. In many scenarios, such data are absent, and relying on artificial units bears the risk of generating false inference. Sliding window designs have been previously applied in these contexts. While adequately accounting for the MAUP, corresponding studies are rather weak on the inferential side: Usually, sliding window designs can only show that spatial and temporal clustering in empirical data significantly deviates from the clustering that can be expected under simulated baseline conditions.

Combining the best of both worlds, MWA applies a sliding window and an automated matching technique, offering an analysis of the causal connections between different types of events for different spatial and temporal distances from a given intervention. The sliding windows entail that pre-aggregated events cannot be easily analyzed, but the matching procedure is generic enough to work with fixed spatial cells, such as administrative units or settlement regions of ethnic groups. In numerical simulations, the method has revealed artificially constructed causal relationships. We have also shown that substantive inference can still be performed when small fractions of interventions overlap in space and time. Higher levels of overlaps (that indicate SUTVA violations) can still be analyzed – albeit less reliably – if numbers of previous treatment and control events are included in the matching procedure.

Applying these lessons to an empirical example yielded novel insights into the ongoing conflict in Iraq. Instead of being mere fence-sitters, civilians in Iraq actively supported incumbent forces in reaction to indiscriminate insurgent violence. This result is a strong reminder of the importance of civilian agency in asymmetric, population-centric conflicts and the negative repercussions that can result from indiscriminate violence.

All results reported in this study were produced using custom *R* code designed to automatically and efficiently perform all steps of MWA, including the sliding window analysis, automated matching using CEM, and the graphical presentation of the results. A corresponding “mwa” package for the *R* programming language has been released to the public and is available at <http://cran.r-project.org/package=mwa>.

A number of applications of this method for future research also spring to mind. The effectiveness of different kinds of peacekeeping interventions on subsequent levels of conflict could be analyzed, for example. In criminological studies, different containment strategies could be tested against one another with regard to subsequent crime rates. A prerequisite for such analyses is detailed data on locations and timings of events and relevant geographic information for the matching procedure. If such information is available, the presented method can be used to generate relevant insights.

# 5 Views to a war: systematic differences in media and military reporting of the war in Iraq<sup>†</sup>

## Abstract

The quantitative study of violent conflict and its mechanisms has in recent years greatly benefited from the availability of detailed event data. With a number of highly visible studies both in the natural sciences and in political science using such data to shed light on the complex mechanisms underlying violent conflict, researchers have recently raised issues of systematic (reporting) biases. While many sources of bias are qualitatively known, biases in event data are usually not studied with quantitative methods. In this study we focus on a unique case—the conflict in Iraq—that is covered by two independently collected datasets: Iraq Body Count (IBC) reports of civilian casualties and Significant Action (SIGACT) military data. We systematically identify a number of key quantitative differences between the event reporting in the two datasets and demonstrate that even for subsets where both datasets are most consistent at an aggregate level, the daily time series and timing signatures of events differ significantly. This suggests that at any level of analysis the choice of dataset may substantially affect any inferences drawn with attendant consequences for a number of recent studies of the conflict in Iraq. We further outline how the insights gained from our analysis of conflict event data have broader implications for studies using similar data on other social processes.

## 5.1 Introduction

In recent years the increasing availability of detailed data on conflict events has led to a number of highly visible studies that explore the dynamics of violent conflict (Bohorquez et al., 2009; Clauset et al., 2007; Johnson et al., 2011; Zammit-Mangion et al., 2012). Taking a natural science or complex systems perspective, these studies complement a quickly growing quantitative

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<sup>†</sup>This chapter is an edited version of the following article: Karsten Donnay and Vladimir Filimonov. (2014). “Views to a war: systematic differences in media and military reporting of the war in Iraq.” Forthcoming in *EPJ Data Science*.

## Chapter 5. Views to a war: systematic differences in media and military reporting of the war in Iraq

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literature in political science that heavily relies on detailed empirical records to systematically study the micro-dynamics of conflict, in particular how individual- or group-level interactions lead to the larger conflict dynamics we observe (Bhavnani et al., 2011, 2014; Linke et al., 2012; Schutte & Weidmann, 2011; Weidmann & Salehyan, 2013).

The conflict event datasets used in these studies primarily draw on media reports and rely to varying degrees on automatic coding as well as the expertise of country or subject experts for coding decisions and quality control (Raleigh et al., 2010; Sundberg et al., 2010). In specific cases—for example in studies focusing on single countries, cities or regions—data may also be based on records collected through Non-Governmental Organizations (NGOs), local newspapers or researchers' own field work (Bhavnani et al., 2011, 2014; Lyall, 2010). These conflict event data, however, have been found to be prone to bias (Chojnacki et al., 2012; Eck, 2012; Raleigh, 2012; Weidmann, 2013). Even for otherwise unbiased and flawless research designs this may strongly affect any inferences with regard to conflict dynamics and mechanisms. Data biases do not only arise from variations in data quality and coding across different datasets but also from systematic uncertainties associated with the data collection efforts themselves. Unfortunately, such issues are notoriously hard to identify and difficult to eliminate in the process of data collection, even within institutionalized large-scale collection efforts. Furthermore, identification of potential biases in existing datasets is complicated by the fact that usually not more than one independently generated dataset exists, essentially making it impossible to infer any biases *post hoc*.

In this study, we focus specifically on a unique empirical case—the conflict in Iraq—that is covered by two independently collected datasets, one of them based on media sources (Iraq Body Count or “IBC”), the other collected “on the ground” by the U.S. military (Significant Action or “SIGACT” data). We use these data to quantitatively test agreement of the event reporting in the two datasets at different temporal resolution and thus systematically identify relative biases. In particular, we find that even for subsets where both datasets are most consistent at an aggregate level the daily time series of events are significantly different. This suggests that whether analyses are based on IBC or SIGACT data may substantially affect the inferences drawn. Our findings are thus highly relevant to a number of recent studies that investigate detailed event dynamics of the war in Iraq using both IBC (Bohorquez et al., 2009; Condra & Shapiro, 2012; Johnson et al., 2011; Lewis et al., 2012; Lewis & Mohler, 2011) and SIGACT data (Braithwaite & Johnson, 2012; Linke et al., 2012) and contribute to the ongoing debate on issues and implications of data quality in conflict event data.

More broadly, our study speaks to a quickly growing literature that systematically analyzes highly resolved data on social processes. This includes work that uses news media articles to detect international tensions (Chadefaux, 2014) or analyzes Twitter messages to detect mood changes (Golder & Macy, 2011). In fact, much of “Big Data” derived from artifacts of human interactions corresponds to time-stamped information about social processes. Studies analyzing such data, however, only very rarely consider the potentially substantive biases arising from how they are generated. In fact, these data are subject to much of the same structural limitations

as conflict event data (see Section 5.2.2), with resulting biases that are just as hard to identify and difficult to infer from data *post hoc*. Inferences based on such data may thus similarly be substantially affected by the choice of dataset, its characteristics and limitations.

This study is structured as follows. Section 5.2 introduces the empirical case and the datasets used: IBC data and the U.S. military (SIGACT) dataset made available by *The Guardian*. In Section 5.3 we systematically compare the reporting of events in both datasets, starting with an aggregate comparison before turning to an in-depth analysis of the time series of number of events and event severity. We further analyze the timing signatures in each dataset separately. Section 5.4 discusses implications of our findings for quantitative analyses of conflict and, more broadly, for studies of social processes that rely on similar data.

## 5.2 The case of Iraq

The Iraq conflict ranks among the most violent conflicts of the early 21<sup>st</sup> century and is characterized by excessive violence against civilians with fatality estimates exceeding at least 130,000 by mid-2014 (IBC, 2014).<sup>1</sup> In mid-2003 the conflict began as an insurgency directed at the U.S. military, its allies and the Iraqi central government. Attacks were initially largely carried out by forces loyal to Saddam Hussein but by early 2004 radical religious groups and Iraqis opposed to the foreign occupation were responsible for the majority of attacks. The insurgency subsequently intensified throughout 2004 and 2005. Increasingly marked by excessive sectarian violence between the Sunni minority and Shia majority the conflict rapidly escalated in 2006 and 2007. Following the U.S.-led troop ‘surge’ in 2007, a massive increase of U.S. boots on the ground accompanied by a major shift in counter-insurgency tactics (Kagan, 2009; Petraeus, 2006, 2010), the conflict eventually de-escalated significantly throughout 2008. After the U.S. withdrawal from Iraq in 2011 the country continues to experience acts of violence on a (close to) daily basis, both as a result of the continued insurgency against the central government but also increasingly again as a consequence of a renewed escalation of sectarian violence. The recent take-over of the north-western (Sunni) provinces by the Islamic State of Iraq and the Levant (ISIL), an Al-Qaeda affiliate, now even threatens the very existence of a multi-ethnic Iraq.

### 5.2.1 Data sources

In our analysis we draw on data from the two most commonly used Iraq-specific datasets: Iraq Body Count (IBC), a web-based data collection effort administered by Conflict Casualties Monitor Limited (London) (IBC, 2014), and U.S. military (SIGACT) data available through *The Guardian* (Rogers, 2010a). We are very mindful of the sensitivity of the SIGACT data and the debate surrounding their use in academic studies.<sup>2</sup> While this debate continues studies are

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<sup>1</sup>The estimates of the total fatalities over the course of the Iraq war differ substantially. For a detailed discussion please refer to <http://www.iraqbodycount.org/analysis/beyond/exaggerated-orb/>.

<sup>2</sup>For reactions by leading conflict researchers to the release of the data see Bohannon (2010), for more general statements regarding their relevance and impact see (The Guardian, 2010). We contend that the data can be used in

## Chapter 5. Views to a war: systematic differences in media and military reporting of the war in Iraq

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making use of these data, most notably a recent political science publication on Iraq (Linke et al., 2012) and an analysis published in the Proceedings of the National Academy of Science (PNAS) using data on Afghanistan (Zammit-Mangion et al., 2012). Note that subsets of the SIGACT Iraq data had previously been made accessible to selected researchers and institutions (Berman et al., 2011; Condra & Shapiro, 2012; Weidmann & Salehyan, 2013) making SIGACT one of the two leading sources of data on the war in Iraq.

The IBC dataset covers violent events resulting in civilian deaths from January 1, 2003 onward until present day and is being updated continuously. We rely here on the publicly available version of the IBC records that does not disaggregate by perpetrator group (IBC, 2014). The data made available through *The Guardian* contains information on all “significant actions” (SIGACTs) reported by units of the U.S. military in Iraq that resulted in at least one casualty. The dataset covers the period January 1, 2004 until December 31, 2009 but is missing 2 intervals of 1 month each (from April 30, 2004 to June 1, 2004 and from February 28, 2009 to April 1, 2009) (Rogers, 2010a). In order to be consistent in our dataset comparison we have selected our study period as ranging from June 1, 2004 to February 28, 2009—a period covered by both datasets without any gaps. This period covers the main phases of the conflict described above.<sup>3</sup>

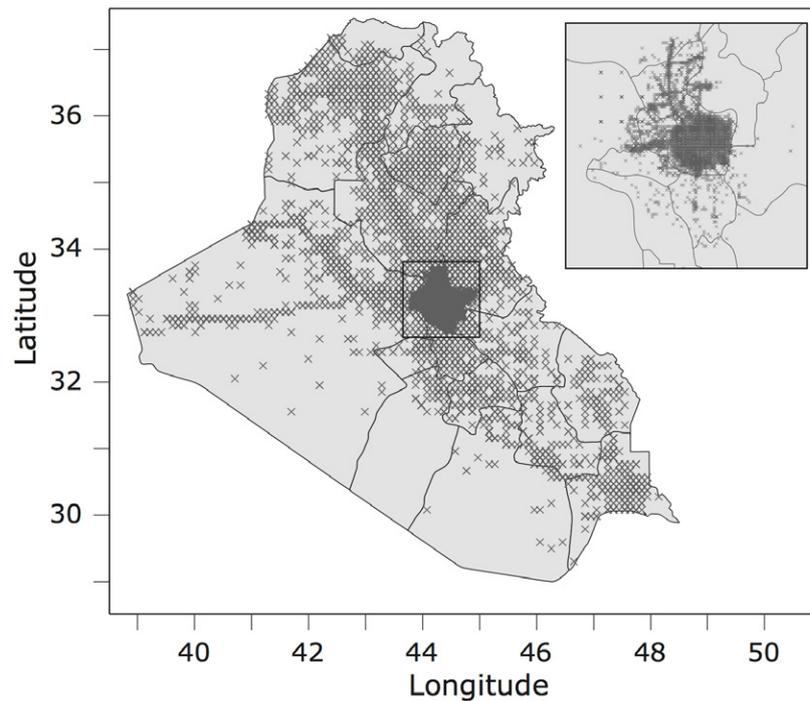
The two datasets differ significantly with regard to the geocoding of conflict events. IBC provides “human description” of the location (such as “near Birtilla, east of Mosul” or “behind al-Faiha’a hospital, central Basra”) which implies limited spatial accuracy. In comparison, SIGACT data entries are categorized by U.S. military regional command but more importantly geo-tagged with latitude and longitude coordinates. These coordinates are truncated at a tenth of a degree (about 10 km) for Iraq outside of Baghdad (Figure 5.1) and at a hundredth of a degree (about 1 km) for the military zone of Baghdad (Figure 5.1, inlay). The two datasets further differ with regard to their temporal resolution. SIGACT events carry timestamps with a resolution of minutes while IBC events are generally coded to daily precision only. Finally, in contrast to SIGACT data which reports the number of individuals killed (KIA) and wounded (WIA) for both military actors and civilians, the IBC dataset exclusively covers deadly violence against civilians.<sup>4</sup> In order to compare the two datasets we thus restricted the SIGACT data to entries pertaining to deadly violence directed at civilians. Note that focusing on civilian casualties exclusively rather than including incidents that wounded civilians may, in fact, lead to a biased view of the violence dynamics in Iraq—simply because whether an attack lead to casualties or not may dependent more on chance than intent (Rogers, 2010b). To control for this, we performed robustness checks where we additionally included the number of wounded civilians reported in SIGACT; these results are included in Section C.3 of the supplementary information.

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a responsible manner for academic research, given that the empirical analysis does not in any way and under any circumstances harm or endanger individuals, institutions, or any of the political actors involved. Note in particular that all data used here has been intentionally stripped of any detailed information on specific incidents beyond information on timing, severity and location of attacks.

<sup>3</sup>Details on data format, preparation etc. are provided in Section C.1 of the supplementary information. Data used in this study is provided as .csv files for download.

<sup>4</sup>We include all SIGACT events independent of perpetrator identity consistent with the coverage of IBC.



**Figure 5.1:** SIGACT data for all of Iraq and for the Baghdad regional command (inlay); shape files for the country and district boundaries were downloaded from the database of Global Administrative Areas (GADM), <http://www.gadm.org>.

### 5.2.2 Structural differences in reporting

There are a number of significant differences between the reporting underlying the IBC and SIGACT datasets that may introduce systematic biases in their respective coverage of violent events. An important source of data bias in geo-referenced event datasets arises directly from the ‘spatial’ nature of the data, i.e., the location of where a violent event occurs may already strongly influence both its chance of reporting and how it is reported (Eck, 2012; Raleigh, 2012). Such biases may simply be structural, for example, due to the fact that newspapers and their local sources—NGOs, development agencies etc.—often only maintain a constant presence in cities or certain regions of a country. Consequently, reporting likely has a specific *urban or regional bias*, i.e., a more complete coverage of events in those areas compared to others with only limited access (Raleigh, 2012). This is often aligned with or equivalent to a *center-periphery* bias since the access and coverage of the media and its sources generally tend to be much lower in remote, peripheral regions compared to the capital or population centers (Raleigh, 2012). The same may apply for government or military reporting, simply because administrative infrastructures and a permanent government presence (offices, police and military installations etc.) are often much less developed in the periphery. In volatile states a central government might even effectively not have any control over large parts of the country.

## Chapter 5. Views to a war: systematic differences in media and military reporting of the war in Iraq

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In Iraq the media-based reporting of IBC is quite likely affected by issues arising from limited coverage, especially for locations outside of the main population centers. SIGACT data may also be prone to spatial bias since the U.S. military or coalition forces did not maintain a constant presence everywhere in the country (Rogers, 2010b). This limitation, however, should be minimal in a highly patrolled region such as Baghdad. For our quantitative analyses we have thus chosen to focus exclusively on the greater Baghdad area, by far the most violent region during the entire conflict. This choice guarantees that our analysis is not systematically affected by geographic reporting bias since within Baghdad both media-based data and SIGACT's field report-based reporting are least likely to be systematically constrained in their coverage.<sup>5</sup> Focusing on a comparably small and coherent spatial region also avoids the fallacy of studying time series of potentially unrelated or only weakly related incidents that are geographically far apart. The violence dynamics in Kirkuk in the predominantly Kurdish north, for example, are very different from the dynamics in Baghdad. In fact, we contend that since Baghdad was the main locus of violence during the conflict but least prone to geographically biased coverage, it represents the “best case” scenario for the reporting of violent events in Iraq and any systematic differences in reporting we uncover should also apply to the full datasets.

Notice that even when focusing exclusively on the Baghdad area, IBC's reporting may be prone to additional biases that arise from its reliance on the quality and accuracy of the media coverage. There is ample evidence that newspaper reports of incidents are subject to a number of biases including selective reporting of certain types of events (Earl et al., 2004; Oliver & Maney, 2000) as well as better coverage of types of events that have occurred before and of larger events compared to smaller events (McCarthy et al., 1996). Such size bias should be especially pronounced in situations with a high density of incidents and only limited reporting capacity—in Iraq this is most relevant during the escalation of the conflict in 2006–2007. SIGACT data on the other hand is directly based on military reports from the field and should therefore, as long as military presence is high as in the case of Baghdad, cover more incidents regardless of size. Based on these structural differences in the reporting we can therefore expect that:

**(I)** IBC should cover systematically fewer low casualty events than SIGACT.

but also that

**(II)** Differences in reporting, in particular of events with few casualties, should be greater the more intense the conflict.

Note that (II) also extends beyond mere coverage—i.e., whether an incident is reported at all—to the quality of reporting. The more intense the fighting the less accurately field reports are able to reflect casualty counts, simply because soldiers may not always be able to reliably account for

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<sup>5</sup>Events in Baghdad make up about 35% of all events in IBC and 50% in the SIGACT data suggesting that there is indeed an element of relative geographic reporting bias.

all casualties in such situations (Rogers, 2010b). Similarly, media reports may also not always precisely reflect “true” casualty counts—in fact, IBC explicitly codes for lower and upper bounds of casualty estimates.<sup>6</sup>

In the case of events with larger casualty counts the reliance of SIGACT on field reports may negatively affect reporting accuracy. One key reason is that longer and intense confrontations involving multiple units may be falsely reported as several separate incidents by each unit instead of being coded as one large episode. This may lead to over-reporting of the number of incidents and under-reporting of the number of casualties per incident. Note further that the categorization of incidents and identification of victims, in particular, may sometimes be ambiguous (Rogers, 2010b). In fact, prior quantitative research confirms that the interest of the observer tends to affect how incidents are reported (Davenport & Ball, 2002). Ideological biases in media reporting—such as government-directed negative reporting on the opposition or simply general limitations to press freedom—result in an inaccurate representation of the situation in a country/region and may thus bias how events are reported (Raleigh, 2012).

In Iraq, we would further generally expect coalition troops’ reporting of civilian casualties to be comparably more conservative than the news media. Modern counterinsurgency doctrines emphasize the importance of “population-centric” warfare, favoring tactics and rules of engagement that minimize collateral civilian casualties (DoS, 2009). In turn, this implies strong incentives for U.S. troops to keep civilian fatality reports of operations as low as possible. These incentives are strongest for comparably larger incidents with significant unintentional (“collateral”) civilian casualties. Note, too, that especially during the escalation of violence in 2006–2007 the conflict in Iraq became highly politicized along the Sunni/Shia divide. This provided strong incentives for newspaper from either side to emphasize the atrocities of the other, i.e., to provide less conservative casualty estimates, especially for large incidents. Overall we can thus expect that

**(III)** IBC should report comparably more events with many casualties than SIGACT.

Note that in general the timing (and location) of attacks can be expected to be more accurate when derived from field reports compared to IBC, whose coverage is fundamentally constrained here since newspaper articles usually only report approximate times and locations. However, it is also known that SIGACT reporting in Iraq did not adhere to homogenous reporting standards throughout the entire conflict, including the integration of reports (or initial lack thereof) from Iraqi military units (Rogers, 2010b). There is also a known issue of field reports being entered with midnight timestamps if the exact reporting time is unknown. These differences should not systematically affect aggregate agreement between the two datasets but may be important when analyzing the micro structure of the data and when matching entries day-by-day. It is important to also mention that both IBC and SIGACT improved their overall reporting throughout the conflict. Taking into account that additional biases may arise from reporting during intense conflict periods

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<sup>6</sup>In our analysis we always rely on the lower bound as it is the most conservative estimate; see Section C.3 of the supplementary information for details and sensitivity analyses.

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as discussed before, we would therefore expect that

(IV) The most accurate day-by-day agreement between the two datasets should be found in the later, less violent stages of the war.

We will return to these four theoretical expectations when analyzing and interpreting the results of our quantitative data comparisons.

Before turning to our analysis of the data on Iraq we would like to emphasize that issues of data bias are, of course, not unique to conflict event data. Researchers, for example, increasingly rely on social media data—such as Twitter messages—to analyze social dynamics (Golder & Macy, 2011). Similar to conflict event data, these messages are time-stamped and carry location information. The same is true for data on human mobility derived from mobile phone traces that provide detailed time-resolved information about the location of users (González et al., 2008). In both cases, data is subject to biases that arise from non-uniform geographic coverage: globally Twitter is known to be heavily biased towards users from North America, Europe and Asia (Leetaru & Schrodt, 2013) but it also tends to be biased towards urban populations in each country (PewResearch, 2013). Mobile phone traces rely on data released by phone companies. Since customer base and coverage of companies tend to vary across regions, they may also have a distinct geographic bias.<sup>7</sup> As in the case of conflict event data the character of the data source may also lead to bias. Twitter, for example, only represents a small, non-representative sample of the overall population (PewResearch, 2013). And a recent study of the web presence of scientist on Wikipedia found that influential academic scholars are poorly represented (Samoilenko & Yasseri, 2014). This suggests that any scientometric analyses based on Wikipedia entries would have a strong relative bias compared to studies based on Facebook and Twitter, which tend to be much more consistent with citation-based metrics of academic impact (Thelwall et al., 2013). The similarities in the sources of bias thus suggest that analyzing the implications of systematic bias in conflict event data also has broader implications for analyses using similar data on other social processes.

### 5.2.3 Baghdad data

The IBC Baghdad subset we analyze comprises events location-coded as “Baghdad” but also those that carry more precise location tags such as “Sadr City” or “Hurriya”. In the SIGACT data we rely on the U.S. military’s definition of the greater Baghdad area and the corresponding regional command “MND-BAGHDAD”. As a robustness check we then perform each of our analyses for subdatasets generated by selecting all events in SIGACT that fall within a radius of 20 km, 30 km and 40 km from the city center. These analyses confirm that the choice of dataset does not affect our substantive findings—whenever not directly reported in the manuscript the results can be found in Section C.3 of the supplementary information.

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<sup>7</sup>In the U.S., for example, the geographic coverage of different providers varies significantly, independent of population density.

| Codename       | Number of events |         | Number of casualties |         |
|----------------|------------------|---------|----------------------|---------|
|                | KIA              | KIA+WIA | KIA                  | KIA+WIA |
| IBC Baghdad    | 9068             |         | 29359–31128          |         |
| SIGACT Baghdad | 18157            | 18504   | 33688                | 59276   |
| SIGACT 20km    | 17533            | 17854   | 32522                | 57151   |
| SIGACT 30km    | 18548            | 18919   | 34450                | 60465   |
| SIGACT 40km    | 19369            | 19782   | 36061                | 63215   |

**Table 5.1:** Datasets

Table 5.1 shows comparative statistics of the five Baghdad subdatasets used in our analysis: (a) IBC data filtered for events in the greater Baghdad area, (b) SIGACT data filtered by Baghdad regional command and by geo-coordinates for a radius of (c) 20 km, (d) 30 km and (e) 40 km from the city center. In the aggregate it appears as if IBC reports a much smaller number of events (approximately 2–3 times smaller than in the SIGACT data). The total number of deaths over the period of analysis also differs but is comparably more consistent. Figure 5.2a and 5.2b show time series of events per day and casualties per event for both datasets. Visual comparison already suggests that at a disaggregate level the datasets differ substantially with regard to the number of events per day and casualties per event reported. Note further that while both datasets capture the escalation of violence in 2006–2007, not only the number of events and casualty counts differ but also the timing of when violence escalated most.

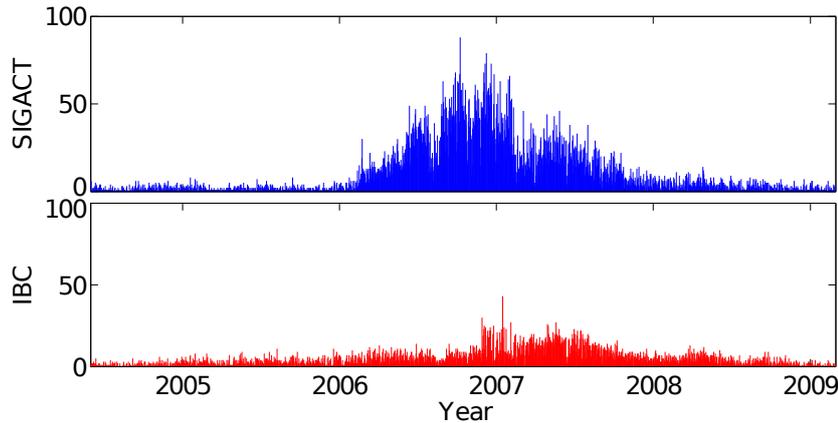
## 5.3 Results

In recent quantitative studies casualty distributions in Iraq have been analyzed in aggregate form (Bohorquez et al., 2009; Clauset et al., 2007) but studies mostly focus on time series of events—monthly, bi-weekly or most often daily (Bohorquez et al., 2009; Braithwaite & Johnson, 2012; Condra & Shapiro, 2012; Johnson et al., 2011; Linke et al., 2012). In line with these different levels of analysis we will compare the reporting of IBC and SIGACT at different levels of disaggregation. We start with aggregate data and then compare the datasets at increasingly smaller temporal resolutions. The (relative) biases we identify at each level of disaggregation can then be related to our theoretical expectations on structural differences in reporting.

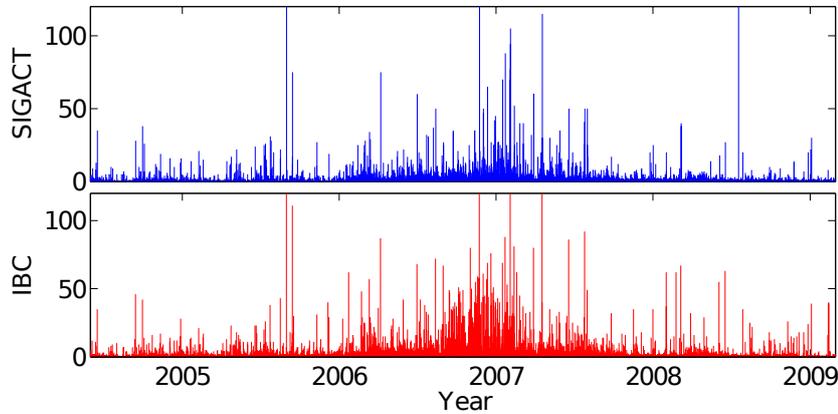
### 5.3.1 Aggregate comparison

The two Baghdad datasets are relatively consistent in the total number of casualties reported: 29441–31222 in IBC and 32531–36213 in SIGACT (see also Table 5.1). They do, however, differ noticeably in the numbers of casualties reported per event (see Figure 5.2b). These differences in overall casualty counts can be best quantified by analyzing aggregate casualty size distributions. Figure 5.3 shows the complementary cumulative distribution function (ccdf) of the number of casualties in the datasets “IBC Baghdad” and “SIGACT Baghdad” on a log-log scale. The

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(a) Number of events per day



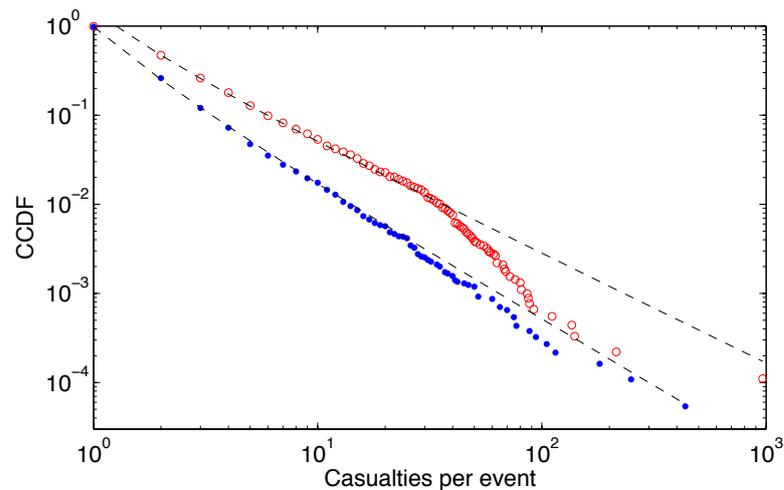
(b) Number of casualties per event

**Figure 5.2:** Time series comparison. The top panel in each graph shows SIGACT, the bottom panel IBC data.

distributions for IBC and SIGACT both appear to follow a power law distribution but differ noticeably in their slopes and their tail behavior. Note that the distributions for the geo-filtered datasets (“SIGACT 20km”, “SIGACT 30km” and “SIGACT 40km”) only differ slightly from “SIGACT Baghdad” and are therefore not discussed separately here. In the case of discrete data such as the casualty counts analyzed here, the ccdf of a power law distribution is given by:

$$P(x) = \frac{\zeta(\alpha, x)}{\zeta(\alpha, x_0)}, \quad x \geq x_0, \quad (5.1)$$

where  $P(x) = \Pr(X \geq x)$  is a probability of finding event with no less than  $x$  casualties,  $\zeta$  is a generalized Hurwitz zeta function (Abramowitz & Stegun, 1965),  $\alpha$  is the exponent of the power law distribution and  $x_0$  is the lower bound of the power law behavior.



**Figure 5.3:** Complementary cumulative distribution function (ranking plot) of the number of casualties in the “IBC Baghdad” (red circles) and “SIGACT Baghdad” (blue dots) datasets. Dashed lines correspond to power law fits using maximum likelihood estimation (details provided in the text).

To verify formally whether or not the distributions do indeed exhibit power law behavior we performed a maximum likelihood fit for a power law distribution using the methodology developed by Clauset et al. for analyzing power law behavior in empirical data (Clauset et al., 2009). The SIGACT data exhibits clear power law scaling (with exponent 2.57) starting at  $x_0 = 2$ , which is valid for almost 2.5 decades. In the IBC data, however, the presence of power law behavior is highly doubtful from a statistical point of view: the power law fit returns an exponent of 2.23, but the scaling is observed for only one decade and the tail clearly deviates from a power law distribution. Note that the power law shape of casualty event size statistics is a well known empirical fact. It has been studied historically in the context of inter-state wars (Cederman et al., 2011b; Richardson, 1948) and more recently for terrorism (Clauset et al., 2007) and intra-state conflict (Bohorquez et al., 2009; Johnson et al., 2011). We here do not intend to discuss the scaling relation of the distribution of event sizes and their possible origins but rather take these as “stylized facts” and good quantitative indicators for marked differences between the two datasets. We would, however, like to note that in complex social or socio-economic systems deviations from power law may be indicative of incomplete data—see, for example, the discussion in Maillart & Sornette (2010) with respect to cyber-risk applications.

The significant upward shift of the IBC ccdf with respect to the SIGACT ccdf indicates the presence of much less small events (1–2 casualties) in the IBC data compared to SIGACT.<sup>8</sup> In order to quantify this difference we used a two-sample Anderson-Darling test (Pettitt, 1976; Scholz & Stephens, 1987). The test is a modification of the Kolmogorov-Smirnov (KS) test that gives more weight to the tail of the distribution and is thus a much better choice in the case of

<sup>8</sup>Note that some of the “missing” small events in IBC might at least be partially accounted for in the aggregated monthly (morgue or hospital) reports that were excluded from our study.

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| Threshold | Number of events |       |       |       |       | $A^2$ statistic |             |             |             |
|-----------|------------------|-------|-------|-------|-------|-----------------|-------------|-------------|-------------|
|           | (i)              | (ii)  | (iii) | (iv)  | (v)   | (i)-(ii)        | (i)-(iii)   | (i)-(iv)    | (i)-(v)     |
| 1         | 9004             | 18157 | 17533 | 18548 | 19369 | 1098.13         | 1103.76     | 1095.52     | 1088.41     |
| 2         | 4273             | 4813  | 4611  | 4940  | 5201  | 84.11           | 81.82       | 87.03       | 85.04       |
| 5         | 1163             | 876   | 851   | 901   | 952   | 6.60            | 7.11        | 7.81        | 7.92        |
| 10        | 484              | 323   | 310   | 325   | 340   | 7.25            | 6.72        | 6.63        | 6.82        |
| 15        | 296              | 159   | 154   | 161   | 169   | <b>1.93</b>     | <b>1.86</b> | <b>1.58</b> | <b>1.94</b> |
| 20        | 206              | 105   | 100   | 105   | 108   | <b>2.29</b>     | <b>1.77</b> | <b>1.54</b> | <b>1.47</b> |
| 25        | 159              | 77    | 75    | 79    | 82    | 3.09            | <b>2.82</b> | <b>2.49</b> | <b>2.57</b> |
| 30        | 123              | 47    | 47    | 51    | 52    | <b>1.43</b>     | <b>1.43</b> | <b>1.28</b> | <b>1.30</b> |
| 40        | 69               | 29    | 29    | 31    | 32    | <b>1.85</b>     | <b>1.85</b> | <b>2.02</b> | <b>1.87</b> |

**Table 5.2:** Results of the pairwise comparison of the distributions of casualties. The datasets are (i) “IBC Baghdad”, (ii) “SIGACT Baghdad”, (iii) “SIGACT 20km”, (iv) “SIGACT 30km” and (v) “SIGACT 40km”. We used a two-sample Anderson-Darling tests (adjusted for ties) for comparison. Bold font marks cases where the value of the Anderson-Darling statistic  $A^2$  is smaller than the critical level  $A_{0.05}^2 = 2.492$  (large-sample approximation) and the hypothesis of two datasets being sampled from the same distribution can not be rejected at a 5% significance level.

fat-tailed data (Frederick, 2006). Specifically, we use it to find the minimal threshold of casualty numbers for which the hypothesis of equal distribution of the two datasets can not be rejected. For this we proceeded as follows: For a given threshold, we select from both datasets only events with casualty counts greater or equal than a given threshold. We then apply a two-sample Anderson-Darling test (adjusted for ties) to test if both datasets were chosen from the same distribution. Varying the threshold value finally allows us to identify the minimal threshold for which the two datasets are statistically not distinguishable.

The results are shown in Table 5.2. The relative comparison of IBC data (i) and SIGACT data (ii)-(v) clearly shows that IBC under-reports small events and over-reports larger events compared to SIGACT. While the total number of events in the IBC dataset is almost two times smaller than in SIGACT, the number of events with 2 or more casualties in both datasets are almost equal. For larger casualty sizes IBC even reports almost twice as many events with 25 casualties and more compared to SIGACT. Note that this, of course, also implies a considerably larger absolute fraction of events with 2 and more casualties in IBC which is clearly reflected in the flatter slope of the IBC cdf compared to SIGACT. Overall, this points to very significant differences in the aggregate casualty statistics between the two datasets.

These differences are also confirmed by our statistical tests. The hypothesis that the casualty distribution in IBC and SIGACT were sampled from the same distribution can be easily rejected for small thresholds (1–10 casualties per event, see Table 5.2 columns 7–10). The Anderson-Darling  $A^2$  statistic reaches the critical value for a significance level of 0.05 and stays below it only for thresholds starting at 15 and more casualties. The hypothesis of agreement can again be rejected for threshold values between 22–28 where the value of the  $A^2$  statistic stays slightly higher than critical level. Note, however, that a threshold of 15 casualties already selects only

a very small subset of events from the whole dataset—less than 300 in IBC and less than 160 in SIGACT for the whole 5 years of data, i.e., less than 3% and 0.8% correspondingly. For thresholds greater than 25 casualties, subsets of the SIGACT datasets are even smaller (less than 100 events). In the quantitative comparisons of the two datasets in the following sections we therefore focus only on reasonably small thresholds of 1–10 casualties.

At an aggregate level our analysis overall quantitatively confirms that IBC both reports systematically less events with few casualties (I) and more events with many casualties (III) compared to SIGACT—we can not test expectation (II) or (IV) here since these require a disaggregated comparison. It is important to point out that the differences in the casualty reporting we observe extend to the four most violent incidents in the period analyzed. In fact, their casualty counts in IBC and SIGACT disagree significantly, with IBC reporting more casualties in all four cases (Table 5.3).

### 5.3.2 Monthly time series comparison

While aggregate distributional measures of conflict event signatures may already provide unique insights into conflict dynamics (Bohorquez et al., 2009; Clauset et al., 2007), the majority of recent studies analyzing conflict mechanisms in Iraq relies on more detailed time series of incidents and their severity (Braithwaite & Johnson, 2012; Condra & Shapiro, 2012; Johnson et al., 2011; Lewis et al., 2012; Lewis & Mohler, 2011; Linke et al., 2012). In this section we first focus on monthly time series. Note that we again consider a number of subsets with different minimal event sizes to account for the fact that the agreement between the two datasets may vary with the size of the events reported.

Figure 5.4a shows the number of events, Figure 5.4b the number of casualties per month in all five Baghdad datasets (see Table 5.1) for thresholds of 1, 2, 5, 7, 10 and 15 casualties per event. The panel in the upper left hand corner of each graph depicts the full IBC and SIGACT data (threshold equal to 1). It suggests that at the monthly level the two datasets provide distinctly

| Date              | Event                               | IBC report | SIGACT report |
|-------------------|-------------------------------------|------------|---------------|
| August 31, 2005   | Baghdad bridge stampede*            | 965–1005   | 436           |
| November 23, 2006 | Sadr City car and mortar bombings** | 215        | 181           |
| April 18, 2007    | Baghdad car bombings†               | 140        | 115           |
| February 3, 2007  | Baghdad market bombing‡             | 136–137    | 105           |

**Table 5.3:** Most violent events and number of casualties reported by IBC and SIGACT

\* “A cry of suicide bomber, and 700 perish in Iraq stampede”, The Guardian, <http://www.guardian.co.uk/world/2005/sep/01/iraq.rorycarroll1> (accessed: 08/07/2013)

\*\* “Iraq, Nov 23, 2006: A Day in Hell”, Spiegel Online, <http://www.spiegel.de/international/world/iraq-nov-23-2006-a-day-in-hell-a-722544.html> (accessed: 08/07/2013)

† “Up to 200 killed in Baghdad bombs”, BBC News, [http://news.bbc.co.uk/2/hi/middle\\_east/6567329.stm](http://news.bbc.co.uk/2/hi/middle_east/6567329.stm) (accessed: 08/07/2013)

‡ “Terror takes toll on market, vendors”, The Washington Times, <http://www.washingtontimes.com/news/2007/feb/6/20070206-115808-3925r/> (accessed: 08/07/2013)

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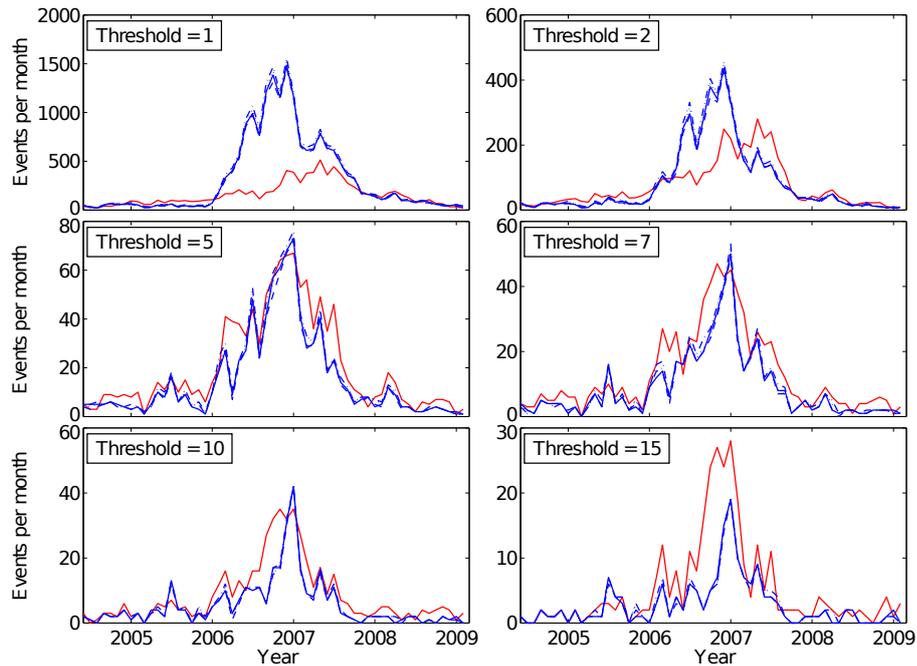
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different accounts of the violence dynamics in Baghdad. These differences in the number of events appear to be most substantial during the escalation of violence in 2006–2007 and for low and high thresholds. If we only exclude events with less than 5 to 10 casualties per event—i.e., intermediate thresholds—the monthly dynamics in the two datasets qualitatively agree much better (Figure 5.4a).

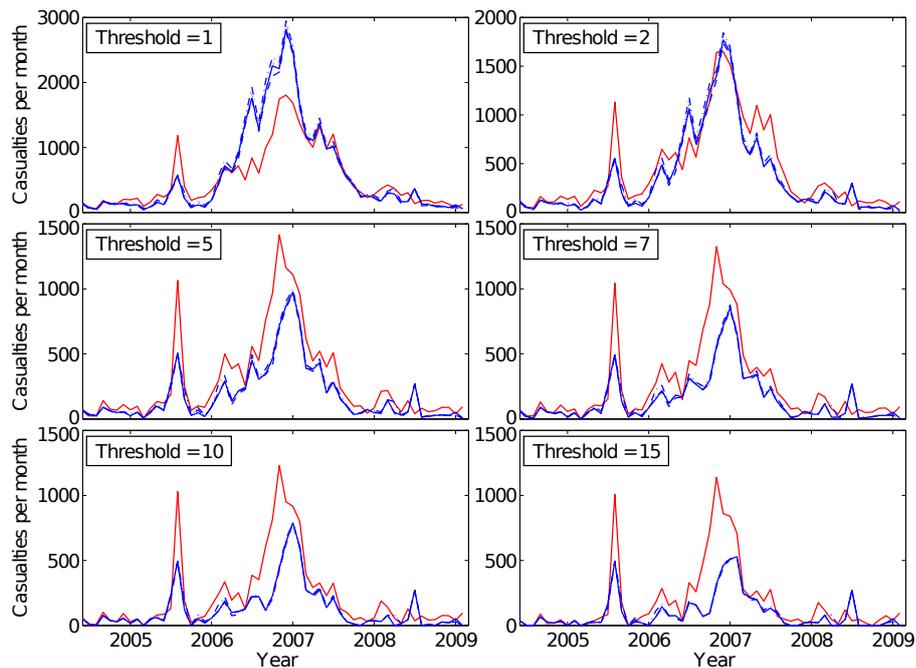
Before turning to a more detailed analysis of the differences in the monthly IBC and SIGACT reporting, we first tested whether at least the overall trends in both the number of events and casualties per month are consistent. A two-step Engle-Granger cointegration test (Engle & Granger, 1987) with an augmented Dickey-Fuller test of residuals (Dickey & Fuller, 1979; Said & Dickey, 1984) can reject the null hypothesis of no-cointegration at a 5% significance level for almost all thresholds analyzed here. In other words, the differences in reporting between IBC and SIGACT generally do not affect the agreement of the coarse-grained trends. The exception are the dynamics of the number of events per month for thresholds of 1, 2 or 3 casualties per event (top panels of Figure 5.4a). Here the Engle-Granger test can not reject the null of no-cointegration (with p-values of Dickey-Fuller test equal to 0.653, 0.650 and 0.503 respectively), which suggests that even the long-term trends in the complete IBC and SIGACT datasets are statistically significantly different.

Overall, the differences in the monthly reporting of IBC and SIGACT are consistent with those observed in the aggregate statistics (Section 5.3.1). We also find the same casualty size dependent relative bias between the two datasets at the level of months. In particular, we again find significantly more small events in SIGACT compared to IBC in line with (I). However, this is only true during the 2006–2007 escalation of violence. In fact, before 2006 IBC even reports more small events and 2008 and onward the two datasets largely agree. This is consistent with our assertion that reporting differs more noticeably the more intense the conflict (II) and also suggests that—apart from the escalation in 2006–2007—IBC and SIGACT reporting of small events is, in fact, quite consistent. Note, however, that we also clearly see an overall tendency of IBC to report more events with many casualties almost all throughout the conflict (III). This attests to differences in reporting also in the less intensive phases of the conflict prior to 2006 and after 2007.

Figure 5.4a and 5.4b also suggest that there is not one threshold value for which IBC and SIGACT reporting agrees both in terms of number of events and casualties per month. While they show the best visual agreement with respect to casualty counts for a threshold of 2 (Figure 5.4b, upper right panel), the corresponding events per month statistics differ markedly (Figure 5.4a, upper right panel). Recall, however, that we argued before that coverage in IBC should be much more limited for small events than in SIGACT. This implies that we should actually not expect an agreement in the number of events per months for thresholds of 1 and 2. In fact, the number of events per month are most consistent for thresholds between 5 and 10 where media-based coverage should be more complete. Since the casualty counts in IBC are significantly larger for these thresholds, this appears to suggest that overall IBC systematically reports more casualties than SIGACT.



(a) Number of events per month



(b) Number of casualties per month

**Figure 5.4:** Dynamics of the number of (a) events and (b) casualties per months in “IBC Baghdad” (red line), “SIGACT Baghdad” (solid blue line), “SIGACT 20km” (dashed blue line), “SIGACT 30km” (dotted blue line) and “SIGACT 40km” (dash-dotted blue line). The panels correspond to subsets of events for thresholds of 1, 2, 5, 7, 10 and 15 casualties respectively. Note that the plots for the different SIGACT datasets (blue lines) are almost indistinguishable.

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It is important to keep in mind, however, that we previously also identified a second possible source of bias that may lead to a similar effect: the reporting of one composite episode as several incidents with less fatalities in SIGACT. In fact, for large events in the SIGACT dataset one can typically find a counterpart in the IBC dataset within the same day or two. In contrast, quite a number of events reported by IBC do not have an equally sized counterpart in the SIGACT dataset (see also Section 5.3.3). Since there are typically many events within a short time window one can, unfortunately, typically not convincingly establish if there are a number of smaller incidents reported in SIGACT that taken together match or approximate the total casualty count of an episode in IBC. This makes it impossible to estimate the extent to which possible mis-reporting of episodes as separate incidents may affect the reporting in SIGACT. Overall, we can therefore only say with certainty that the differences in casualty reporting observed at a monthly level are consistent with IBC systematically reporting more casualties than SIGACT, mis-reporting of episodes as separate incidents in SIGACT, and/or a combination of both.

### 5.3.3 Daily time series comparison

Many of the recent quantitative studies of the conflict in Iraq rely on detailed daily time series. We therefore now turn to a statistical analysis of deviations in the day-by-day microstructure of reporting between IBC and SIGACT. Note that in the period 2004–2009 both datasets exhibit a high degree of non-stationarity (see Figure 5.4a). In fact, the number of events in the second half of 2006 and first half of 2007 is up to 10 times larger than in 2005 or 2009. Any statistical analysis of these data thus requires us to explicitly model this non-stationarity, for instance using parametric methods. Alternatively, we can restrict our analyses to sufficiently small time windows, in which the dynamics can be assumed to be (approximately) stationary. In line with previous works (see for example Bohorquez et al., 2009) we here pursue the latter approach and employ standard non-parametric tests to moving time windows. The choice of appropriate window size is subjected to trade-offs: it should be as small as possible to guarantee a stationary regime but also sufficiently large to contain sufficiently many events for robust statistical tests. We found that time windows ranging from 4 months to half a year ( $T = 120$  days to  $T = 180$  days) fulfill both of these conditions.<sup>9</sup> However, we also performed our tests for a window size of 1 year ( $T = 360$ ) as a robustness check.

For every window size  $T$  we slide the moving window across the whole range of data in steps of one month and extract the subset of events in both IBC and SIGACT within each time window. For each of the (approximately) stationary periods we can then compare the distribution of events per day as a measure of the day-by-day microstructure of the data using a two-sample Anderson-Darling test. The Anderson-Darling test rejects the hypothesis of both time-series being sampled from the same distribution if the statistic  $A^2$  is smaller than the critical level  $A_{0.05}^2$  for a significance level of 0.05. Since the number of samples (window size  $T$ ) is sufficiently large we use the large sample approximation for the critical level  $A_{0.05}^2 = 2.492$  (Pettitt, 1976).

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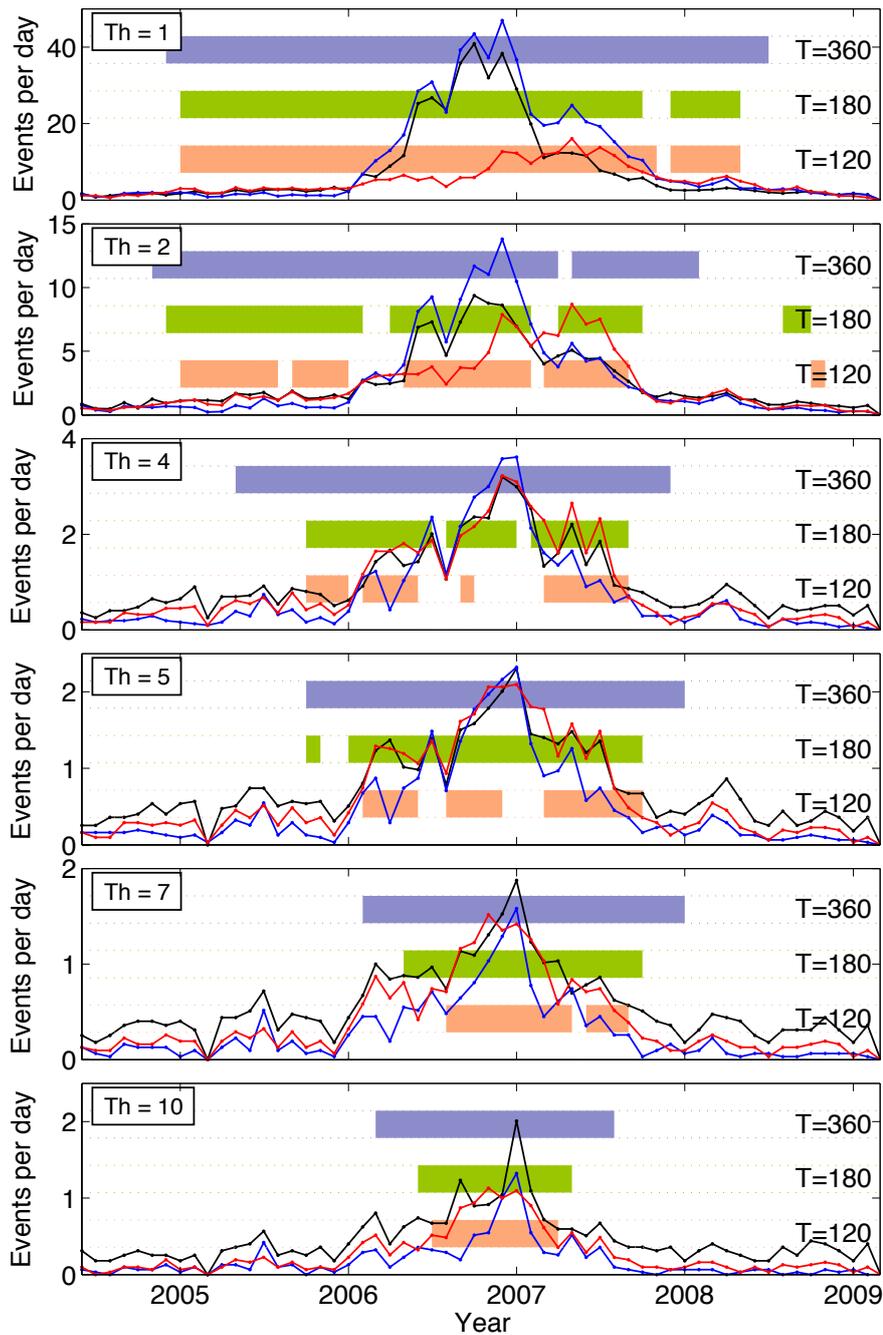
<sup>9</sup>A previous analysis of the number of events per day in Iraq also used a half year temporal window size (Bohorquez et al., 2009).

Note that in contrast to the distribution of casualties per event (Figure 5.3), the distributions of events per day do not have fat-tails and typically decay almost exponentially (Figure C.7 in the SI). A Kolmogorov-Smirnov test would thus also in principle be applicable here (Frederick, 2006). However, in order to be consistent throughout the paper and to account for the slower-than-exponential tails in case of small thresholds of 1 and 2 casualties per event, we here also rely on the more rigorous Anderson-Darling test.

Figure 5.5 graphically illustrates the results of the Anderson-Darling test for different thresholds and different window sizes. Color bars indicate the center of all windows of size  $T$  for which the null hypothesis of the number of events per day in both datasets being sampled from the same distribution can be rejected at a 5% significance level. The figure clearly illustrates that the two datasets significantly differ with respect to the distribution of events per day: the distributions in the two full datasets (threshold equal to 1, top panel) are statistically distinguishable from 2005 through 2007; only in the initial phase of the conflict and in the calmer phase after the U.S. military troop “surge” in 2007 we can not detect significant differences. The higher the threshold, i.e., the more small events we exclude, the better the distributional agreement. It is important to note that in case of large differences in the numbers of events per day, the Anderson-Darling test will indicate significant deviations of one sample from another irrespective of the temporal characteristics. This certainly contributes to the strong disagreement for thresholds of 1 and 2 casualties in 2006–2007 but should not affect the results elsewhere where the numbers of events are much more similar. In general, the results for different window sizes are quite consistent and we can be confident that the exact choice of time window does not systematically drive our results.

The analysis in Figure 5.5 highlights that even though the average number of small events (thresholds 1 and 2) are relatively similar in IBC and SIGACT prior to 2006 and after 2007 the detailed daily reporting may still significantly differ, for example, in 2005 or in early 2008 (top panel). In the period 2006–2007 the daily structure of small events reported in the two datasets is almost everywhere significantly different except for a short episode in early 2007. For larger events (threshold 4 and larger) the average number of events per day is much more consistent throughout but in the most intense phase of the conflict 2006–2007 the distributions of events per day remain statistically distinguishable. For events with 10 casualties and more the difference is only significant mid-2006 through early 2007 at the height of the escalation. The fact that the micro structure of the datasets become statistically indistinguishable does of course not imply that they necessarily correspond to the same day-by-day occurrence of events. The test simply determines whether or not the overall distributions of events per day in a given (comparably large) time window are distinguishable or not. Consider, for instance, the very simple example of two time series with alternating 1 and 3 events on two subsequent days but where the occurrence of events in the second series is shifted by one day. These time series have the same average number of events per day and are statistically absolutely not distinguishable even though each day their number of events differs by two, their average number of events per day.

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**Figure 5.5:** Distributional agreement of “IBC Baghdad” and “SIGACT Baghdad”. Color bars illustrate the results of a 2-sample Anderson-Darling tests for the distribution of number of events for time windows of  $T = 120$  days (orange bars),  $T = 180$  days (green bars) and  $T = 360$  days (violet bars) for thresholds equal to 1, 2, 4, 5, 7 and 10 casualties. The bars indicate the center of those time windows for which the hypothesis of agreement of the distribution of events per day can be rejected at a 5% significance level. The black line represents the RMS difference between “IBC Baghdad” and “SIGACT Baghdad”, red and blue lines are the monthly averages of the number of events per day for the two datasets respectively.

In order to better quantify the actual day-by-day correspondence between IBC and SIGACT we therefore additionally consider the root mean square (*RMS*) difference of the number of events in IBC ( $n_{IBC}(t)$ ) and SIGACT ( $n_{SIGACT}(t)$ ) for a sliding window of size  $T_2 - T_1 = 1$  as a simple quantitative metric of (average) daily agreement (black line in Figure 5.5):

$$RMS = \sqrt{\frac{1}{T_2 - T_1 + 1} \sum_{t=T_1}^{T_2} (n_{IBC}(t) - n_{SIGACT}(t))^2}. \quad (5.2)$$

This difference can be directly compared to the average numbers of events per day in both IBC and SIGACT for the same moving time window (red and blue line in Figure 5.5 respectively):

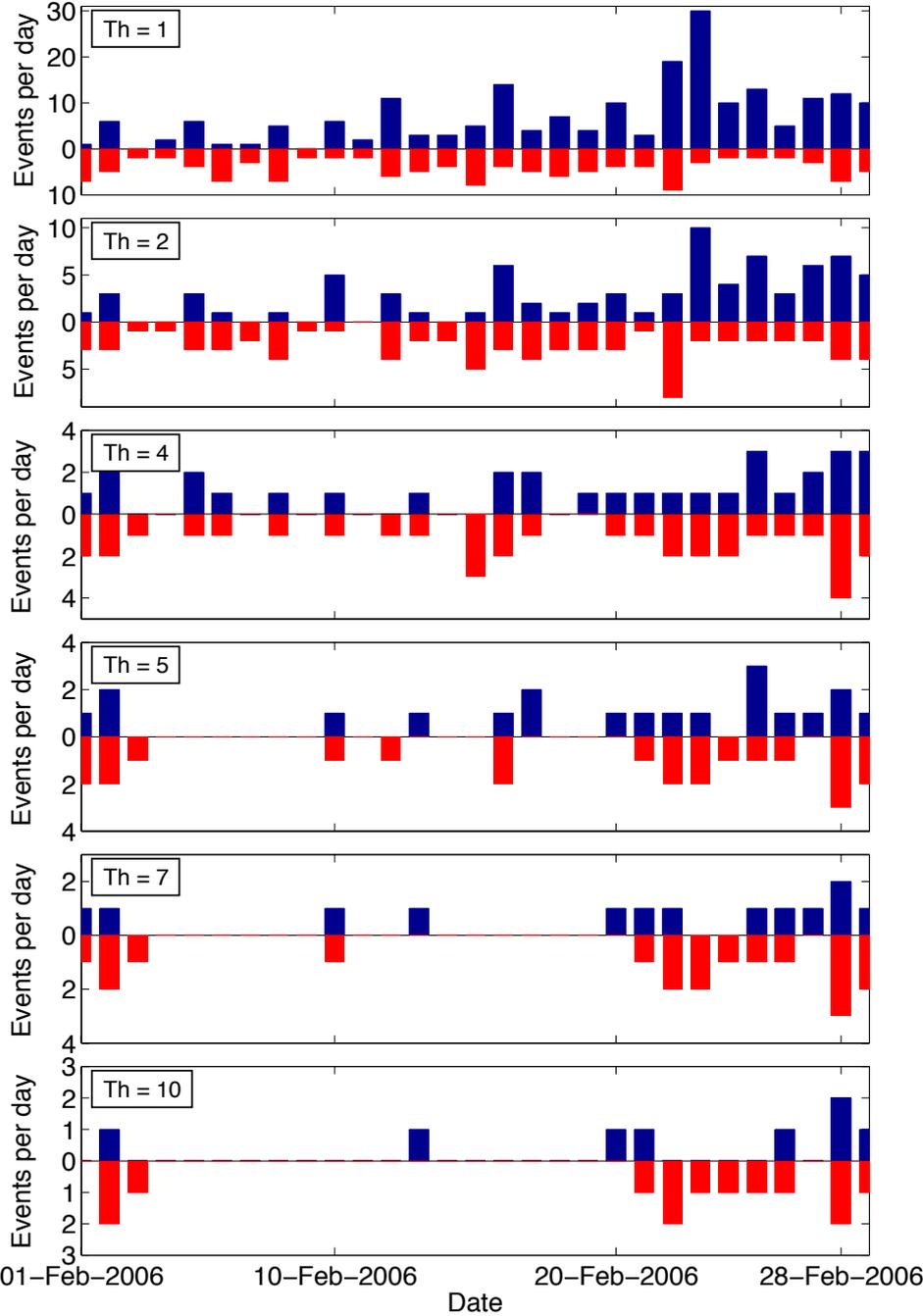
$$\overline{n_{IBC}} = \frac{1}{T_2 - T_1 + 1} \sum_{t=T_1}^{T_2} n_{IBC}(t), \quad \overline{n_{SIGACT}} = \frac{1}{T_2 - T_1 + 1} \sum_{t=T_1}^{T_2} n_{SIGACT}(t). \quad (5.3)$$

We find that the *RMS* difference is always of the order of magnitude of the average numbers of events per day for all thresholds we consider. In other words, the typical difference between two datasets is equal to the typical number of events per day. This is true even for intermediate thresholds of 5–10 casualties per event where the cumulative monthly number of events reported in IBC and SIGACT agree quite well. Note further that the *RMS* differences 2008 and onward is not significantly smaller than prior to 2006 contrary to our theoretical expectation that difference in reporting should be smallest in the later, less violent phases of the conflict (IV).

To test our intuition for how day-by-day difference relate to distributional agreement, we analyze the daily agreement in IBC and SIGACT in February 2006. We chose this period specifically such that the two datasets are statistically distinguishable for small and indistinguishable for large thresholds (see Figure 5.5). Figure 5.6 graphically illustrates the direct comparison of the number of events reported in each dataset. It is visually apparent that the number of events per day with thresholds of 1 and 2 casualties (upper two panels) reported in SIGACT and IBC differ. Specifically, on some days SIGACT reports more events, on others IBC does, and there are also days when one of the datasets reports no event but the other one does. For larger events (up to 4 and 5 casualties, third and fourth panel) the numbers of events per day in both datasets are much more consistent but there are still significant differences. SIGACT, for example, at a threshold of 5 reports significantly more days with one event than IBC and less days with two events. For thresholds of 7 and larger (lower two panels) the distributions of events per day are statistically not distinguishable anymore. In the day-by-day comparison we see that each daily signature is dominated by days with no, one or two events and the occurrence of these days is overall quite similar. Note, however, that at the same time for well more than 50% of the days these counts do not coincide, which explains the day-by-day mismatch represented by the comparably large *RMS* differences (Figure 5.5).

The large *RMS* difference we observe throughout the whole dataset should therefore be an indication that the day-by-day structure of event reporting in SIGACT and IBC does indeed significantly differ—despite the fact that they may be statistically indistinguishable at an aggregate

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**Figure 5.6:** Dynamics of the numbers of events per day for “IBC Baghdad” (red) and “SIGACT Baghdad” (blue) in February 2006 for thresholds equal to 1, 2, 4, 5, 7 and 10 casualties. The vertical axis for the IBC dataset was mirrored for clarity purposes.

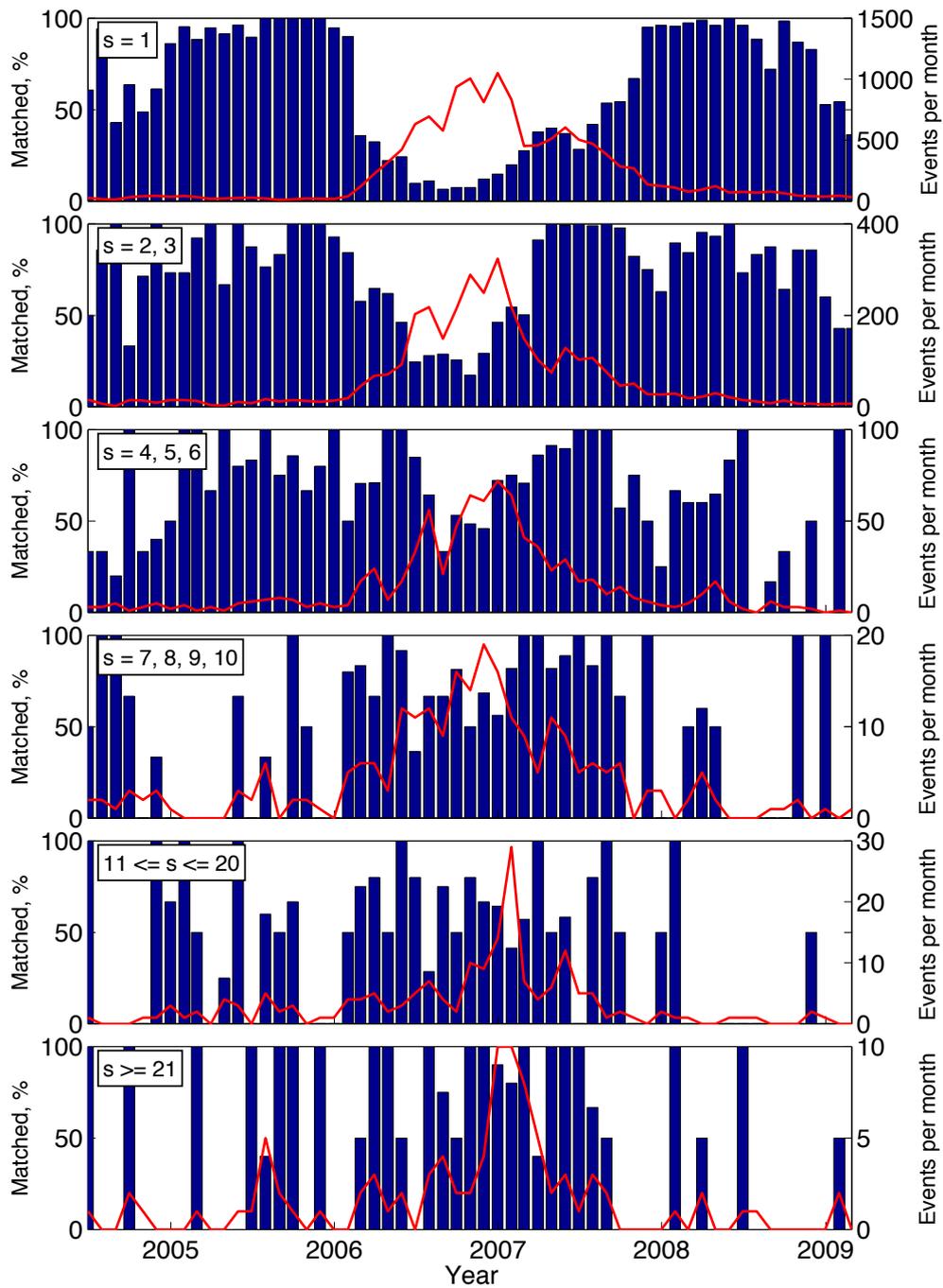
or distributional level. In order to quantitatively estimate this daily mismatch, we compared how many events of a given size in SIGACT—the dataset with more events—can be matched to events in IBC. In matching events we allow for an uncertainty of  $\pm 1$  day. Please refer to Section C.2 of the supplementary information for the details of our automated matching procedure. Figure 5.7 shows the number of matched events (blue bars) as a fraction of the total number of events in SIGACT (red line) for every month in the dataset. For simplicity we have grouped casualty sizes in categories. Note that for months with no events in a given casualty category, the fraction of matched events is set to 0 by default.

The figure suggests that daily SIGACT and IBC records are most consistent outside of the escalation of violence in 2006–2007—this is particularly true for events with less casualties. Excluding the escalation phase 2006–2007 we find that on average 85.8% of the entries with 1 casualty and 82.3% of the entries with 2 or 3 casualties in SIGACT coincide with an entry with the same number of casualties within  $\pm 1$  day in IBC (Table 5.4). In contrast, during the period 2006–2007 only 24.6% of SIGACT reports with 1 casualty—by far the largest share of incidents—can be matched to IBC entries. In the same period, 50.9% of SIGACT records with 2 and 3 casualties have a corresponding entry in IBC within  $\pm 1$  day. For events with few casualties we can thus also confirm at a day-by-day resolution that differences in the reporting are generally larger the more intense the conflict (II). In contrast, the day-by-day agreement of events with 4 and more casualties is generally better in the 2006–2007 period (see Table 5.4 for details). Notice that especially the match of very large events (more than 20 casualties) is generally very good throughout (77.8% match). Finally, we do not find any systematic evidence that the detailed match of SIGACT and IBC has increased significantly after 2008, contrary to our theoretical expectation (IV).

It is important to emphasize here that we thus far only considered a one-sided comparison that matches SIGACT events to IBC. We previously observed that IBC reports more events with many casualties than SIGACT (Figure 5.4a), i.e., matching IBC to SIGACT events will yield a noticeably lower match. For example, the match of events with more than 20 casualties in this case is only 37.3% (please refer to Section C.2 of the supplementary information for the full comparison). The large RMS difference in Figure 5.5 reflects this mismatch. Note, too, that the RMS difference is a measure of daily agreement whereas we here allow for a timestamp uncertainty of  $\pm 1$  day—it is consequently a much more conservative estimate of the agreement of the two time series than the one tested here. As we would expect, using smaller tolerance ( $\pm 0$  days) to match events generally decreases agreement while using larger tolerance ( $\pm 3$  days) increases agreement of SIGACT events with IBC (see Section C.2 of the supplementary information for details). There is one notable exception though: very large events (with more than 20 casualties) are equally well matched for all tolerances suggesting that their reporting is clearly the most consistent.

We validated our day-by-day comparison by comparing it to results of a study performed at Columbia University. In the study a small random sample of SIGACT events with civilian casualties was compared to entries in the IBC database (Carpenter et al., 2013). Specifically,

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**Figure 5.7:** Day-by-day match of events of a given size  $s$  in “SIGACT Baghdad” to entries in “IBC Baghdad”. Blue bars indicate the number of matched events as a fraction of the total number of events in SIGACT for every months in the dataset (left axis), the red line illustrates the overall number events per months for the given casualty sizes (right axis). When matching events we allow for a timestamp uncertainty of  $\pm 1$  day.

| Casualties | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|------------|-------------------|-------|-------|---------|-------|-------|
|            | matched           | total | %     | matched | total | %     |
| s = 1      | 1264              | 1473  | 85.81 | 2925    | 11871 | 24.63 |
| s = 2, 3   | 343               | 417   | 82.25 | 1556    | 3054  | 50.94 |
| s = 4–6    | 86                | 133   | 64.66 | 480     | 693   | 69.26 |
| s = 7–10   | 22                | 45    | 48.88 | 149     | 202   | 73.76 |
| s = 11–20  | 18                | 36    | 50.00 | 83      | 143   | 58.04 |
| s > 20     | 15                | 23    | 65.21 | 55      | 67    | 82.08 |

**Table 5.4:** Number of SIGACT reports matched to IBC entries

students were tasked to manually match SIGACT entries to IBC events following a specific detailed protocol. The analysis revealed that only 23.8% of the events in their SIGACT sample had corresponding entries in IBC. The Columbia researchers noted though that most of the events in their sample had only very few casualties—a consequence of the fact that by randomly sampling events for their study they mainly selected incidents during the period 2006–2007 where by far the most SIGACT events were recorded. In fact, the large majority of records in this period reports only one casualty per event (see Table 5.4). In our analysis we find an agreement of 24.6% for these events in the 2006–2007 period, which is very consistent with the Columbia estimate. For events with more than 20 casualties 94.1% of the SIGACT entries could be matched to entries in IBC in the Columbia study. The estimate of 82.1% based on our automated comparison is similar but somewhat more conservative. Note that the specification of timestamp uncertainty of  $\pm 1$  day used in our automated procedure is equivalent to the matching prescription used in the Columbia study (see Section C.2 of the supplementary information for details).

It is important to emphasize two key shortcomings of the manual, in-depth comparison performed in the Columbia study. Most importantly, the random selection of events across the whole dataset effectively limits their analysis to the period 2006–2007—the period in which all of our previous analyses find the most significant disagreement between IBC and SIGACT. Their findings thus likely systematically underestimate the overall match of events. In fact, our analysis shows that for the full period of analysis 38.5% of all SIGACT records could be matched to IBC entries with the same number of casualties. This is significantly more than the 23.8% reported in the Columbia study. Furthermore, manual comparisons are only possible for small (random) subsets of event. Having verified that we obtain results consistent with an in-depth comparison by human coders, the clear advantage of an automated comparison is its coverage, i.e., it efficiently yields estimates of the correspondence of daily reports in IBC and SIGACT for the full period of analysis.

In summary, our results strongly suggest that at any level of analysis—aggregate statistics, monthly statistics, detailed distributional level and daily time series—IBC and SIGACT reporting differ significantly, most strongly for events with few casualties but also for larger event sizes where aggregate event statistics are comparably more consistent. Consequently, we can expect that the choice of dataset would strongly affect any inference we draw from these data, simply

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because the conflict dynamics represented in each datasets at any level of analysis are indeed quite different.

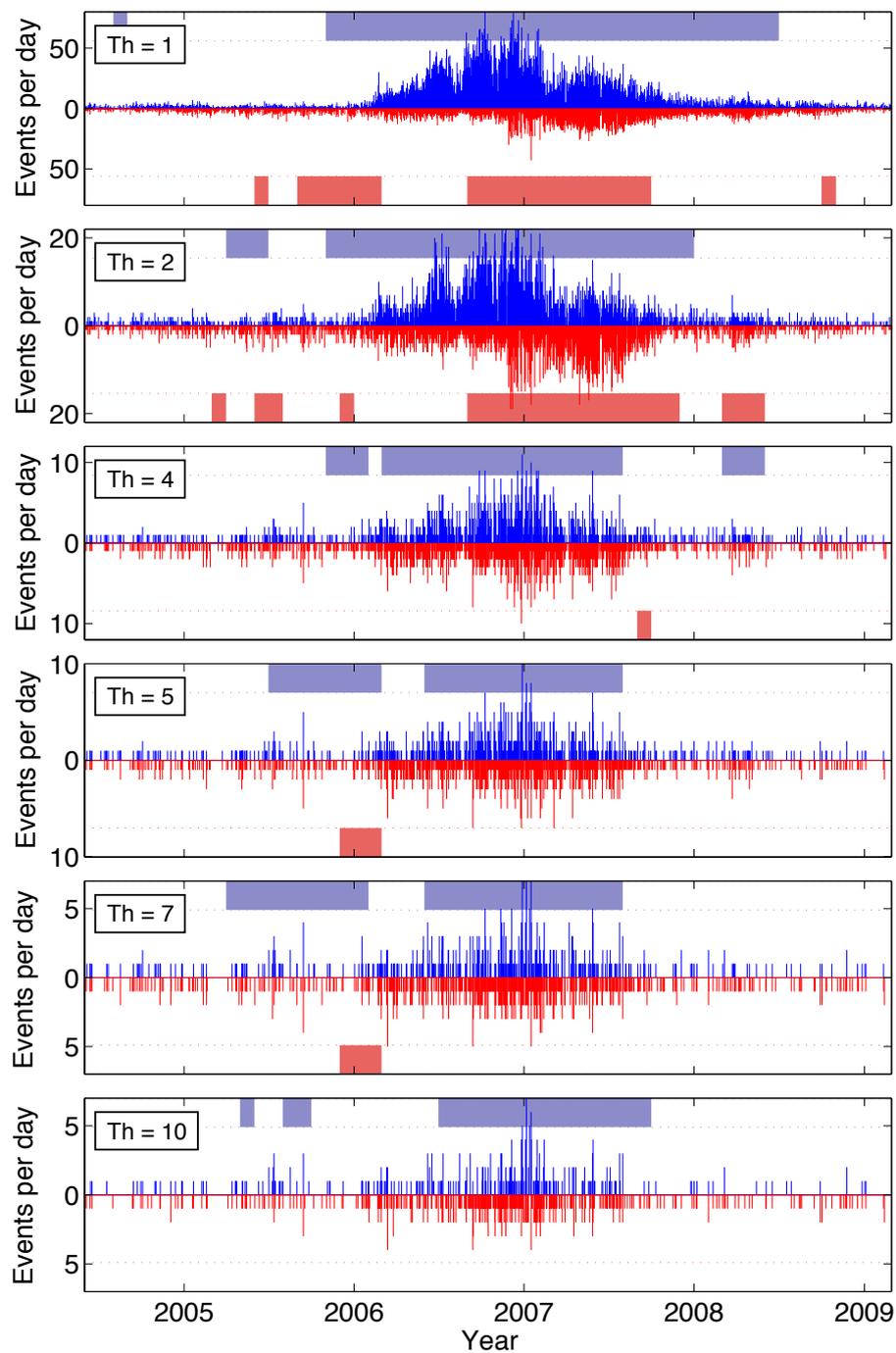
In the following sections we complement these comparative insights with an in-depth analysis of the reporting in each dataset. Specifically, we explore if and where the two datasets contain non-trivial timing information—i.e., information about the occurrence of subsequent events—and how robust these are to uncertainty in timestamps. This is, of course, a critical precondition for the use of the datasets for any kind of timing or causal analysis. It is complementary to our prior comparative analysis in the sense that both, either or neither of the datasets may actually be suitable to study event dynamics in Baghdad, regardless of the relative differences in reporting we have already identified.

### 5.3.4 Distributional signatures

In Section 5.3.3 we used the distribution of events per day to characterize day-by-day event dynamics. A second very common measure that captures the micro-structure of event data is the distribution of times between incidents, or inter-event times (Johnson et al., 2011). The latter is always favorable if the data resolution is more fine-grained than days. Inter-event timing distributions at a resolution of hours, for example, provide a much more detailed characterization of the dynamics of subsequent events. We here chose to rely on the distribution of inter-event times because it also tends to be more sensitive to differences in the distribution of sparse data for which it is generally more difficult to detect deviations from a trivial timing signature. As before, we consider the dynamics in a given time window of length  $T$  within which the conflict dynamics can be assumed to be (approximately) stationary. Notice that the results for the event per day statistics are substantively equivalent; please refer to Section C.5 of the supplementary information for details.

In structureless datasets, i.e., in datasets where the timing of events is statistically independent, the distribution of events per day simply follows a Poisson, the corresponding distribution of inter-event times an exponential distribution. The deviation of timing signatures from a Poissonian or exponential is thus mainly indicative of the usefulness of the dataset because a featureless dataset is essentially useless for any kind of quantitative (causal) inference or timing analysis. We would, however, also like to note that empirically and theoretically it is not plausible that the timing of conflict events in Iraq is completely independent. In fact, most theories of political violence prominently feature mechanisms that emphasize reciprocity and reactive dynamics (Haushofer et al., 2010; Linke et al., 2012), spatial spillover effects or diffusion of violence (Schutte & Weidmann, 2011).

Figure 5.8 shows the number of events per day for both datasets and graphically illustrates the results of a Kolmogorov-Smirnov test for a moving window of 180 days (results for larger window sizes are consistent and are discussed in Section C.5 of the supplementary information). Specifically, bars indicate the center of time windows for which the Kolmogorov-Smirnov test



**Figure 5.8:** Inter-event timing signatures. Color bars illustrate the results of a KS-test for exponential distribution of the inter-event times in time windows of  $T = 180$  days for thresholds equal to 1, 2, 4, 5, 7 and 10 casualties (see text for details). The bars indicate the center of those time windows for which the hypothesis of agreement of the distribution of inter-event times with an exponential distribution can be rejected at a 5% significance level. (i.e., the datasets exhibits a non-trivial timing structure). The graph also shows the dynamics of the number of events per day in “IBC Baghdad” (red) and “SIGACT Baghdad” (blue). The vertical axis for the IBC dataset was mirrored for clarity purposes.

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rejects the hypothesis of agreement of the distribution of inter-event times with an exponential distribution at a 5% significance level. The analysis suggests that in the full SIGACT Baghdad dataset the timing of events deviates significantly from that of a Poisson process all throughout 2006 to mid-2008. In the much calmer periods prior to 2006 and after mid-2008 the timing signature, however, does not deviate significantly from that of a featureless process. For events larger than thresholds of 2, 4, 5, 7 and 10 casualties, SIGACT still consistently features periods where the timing of events does not follow a featureless Poisson process, mainly in the most violent period mid-2006 to mid-2007.

In the full IBC dataset and for events with more than 2 casualties the timing of events also has a significant non-trivial timing structure that allows to reject the null hypothesis of Poisson dynamics for periods throughout late 2005 to 2007. This finding, however, is much less robust than for the SIGACT data. In fact, there is a half-year stretch in early 2006 for the full IBC dataset that features only a trivial timing signature. For a threshold of 2, the inter-event signature is also not distinguishable from a Poissonian in a period from late 2005 to late 2006. Notice that in both periods the number of events per day is quite large. The differences between the signatures in IBC and SIGACT are most pronounced for subsets of events with minimally 4 or more casualties. Even though the overall number of events in SIGACT and IBC is comparable for those subsets, there is hardly any time window for which the timing signature in IBC significantly differs from that of a featureless process. This is especially obvious in the escalation phase mid-2006 to mid-2007 where the timing of events in IBC is statistically independent everywhere but deviates significantly from a featureless process in SIGACT.

As emphasized before, based on theories of political violence, we would expect that the timing of events should not be independent. The empirical narrative of the conflict in Iraq similarly suggests that events tend to be related. It is, however, in general not possible to decide whether or not the absence of non-trivial signatures in these periods is a consequence of incomplete reporting or evidence that the timing of events of a given size is indeed uncorrelated. The fact that both datasets feature time windows with trivial timing signatures thus simply suggests that it would be ill-advised to use the respective datasets in these periods to study (causal) relations between the timing of events. This is true for large parts of the IBC data—especially for larger thresholds—whereas SIGACT generally features more and longer time windows with non-trivial timing signatures (Figure 5.8). Notice though that in the low intensity conflict phases prior to 2006 and also after mid-2008 our statistical tests do not indicate any non-trivial timing signatures in SIGACT either.

Overall IBC appears to be much less suitable to study timing dynamics and thus to infer (causal) relationships between events. This is consistent with our observation in Section 5.2.2 that the reporting of timestamp in IBC may be more constrained through the use of approximate—or possibly misreported—timing of events provided in newspaper articles. It is important to keep in mind though that we only tested for non-trivial timing signatures for the full Baghdad data—significant correlations in the timing of events may, for example, simply be limited to smaller geographic scales.

### 5.3.5 Uncertainty of timestamps

We now turn to a systematic test of the effect of timestamp uncertainty on the distributional features analyzed in the previous section. In other words, we address the question of how robust the timing signatures we find are to uncertainties in the coding of timestamps. The robust coding of event timestamps is critically important for any quantitative technique where inferences hinge on the (causal) order of events. Examples of commonly used techniques using such time-ordered data include point process models, such as self-excited Hawkes processes (Hawkes, 1971a,b), Autoregressive Conditional Durations (ACD) (Engle & Russell, 1997, 1998) or Autoregressive Conditional Intensity (ACI) (Russell, 1999). Note that in both IBC and SIGACT the reporting of event timing may, in principle, be subject to systematic coding inaccuracies. The media sources IBC relies on may report events with a delay, provide only approximate timing information or may misreport the timing of an event altogether. SIGACT data is compiled from field reports, which may also systematically miscode the true timing of an event. Common problems include delayed reporting in situations of heavy engagement with enemy forces, reporting post hoc on incidents that a unit was not directly involved in and for which the timing is not precisely known, or summary reports filed at the end of a day (see also Section 5.2.2).

In order to statistically characterize the effect of timestamp inaccuracies on the day-by-day signatures of events, we again rely on the distribution of inter-event times  $\tau_i = t_i - t_{i-1}$ . We further assume that both IBC and SIGACT report events with timestamp uncertainties  $\Delta_{IBC}$  and  $\Delta_{SIGACT}$ . Note that the IBC dataset only codes timing of events with a precision of days, i.e.,  $\Delta_{IBC} \geq 1$  day. SIGACT on the other hand carries much more precise timestamps with a resolution of minutes and thus does not have this constraint. In order to account for uncertainties  $\Delta$  in the timestamps we adopted the methodology proposed in Filimonov & Sornette (2012) and assume that the difference between the real time of an event  $\tilde{t}_i$  (which is unknown) and the timestamp  $t_i \geq \tilde{t}_i$  is some effective “noise”  $\xi_i = t_i - \tilde{t}_i < \Delta$ .

To test the impact of a given uncertainty  $\Delta$  on the timing signature in each time series we then proceed as follows. For a given time window  $T$  we draw random variables  $\xi_{i,IBC}$  and  $\xi_{i,SIGACT}$  from the uniform distributions  $U([0, \Delta_{IBC}])$  and  $U([0, \Delta_{SIGACT}])$  respectively. We then construct time series  $\hat{t}_{i,IBC} = t_{i,IBC} - \xi_{i,IBC}$  and  $\hat{t}_{i,SIGACT} = t_{i,SIGACT} - \xi_{i,SIGACT}$ , and calculate the distribution of inter-event times  $\hat{\tau}_{i,IBC} = \hat{t}_{i,IBC} - \hat{t}_{i-1,IBC}$  and  $\hat{\tau}_{i,SIGACT} = \hat{t}_{i,SIGACT} - \hat{t}_{i-1,SIGACT}$  for each. Note that the values  $\hat{\tau}_i$  represent proxies for the unobserved real values of inter-event times  $\tilde{\tau}_i$ . We then apply a two sample Anderson-Darling test to the distributions of these inter-event times (for both IBC and SIGACT independently). We repeat this procedure  $M = 100$  times, generating a set of binary values  $\{h_{j,IBC}\}$  and  $\{h_{j,SIGACT}\}$ ,  $j = 1, \dots, M$ , where  $h_j = 0$  if we can reject the null hypothesis at a 5% significance level, and  $h_j = 1$  if the null hypothesis can not be rejected.

The effective measure for whether or not the timing distributions of the two time series with uncertainties are distinguishable is then simply the fraction of cases when the null hypothesis can not be rejected:  $F_{IBC} = \sum_{j=1}^M h_{j,IBC} / M$  and  $F_{SIGACT} = \sum_{j=1}^M h_{j,SIGACT} / M$ . If the value of

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$F_{IBC}$  (or  $F_{SIGACT}$ ) is close to 0 we can be certain that the distributions of inter-event times  $\hat{\tau}_{i,IBC}$  (or  $\hat{\tau}_{i,SIGACT}$ ) are different from an exponential distribution—independently of particular values of the “noise” terms  $\xi_{i,IBC}$  (or  $\xi_{i,SIGACT}$  respectively). This also implies that the real inter-event times  $\tilde{\tau}_{i,IBC}$  (or  $\tilde{\tau}_{i,SIGACT}$ ) exhibit non-trivial clustering. Similarly, a value of  $F$  close to 1 suggests that for most of the cases we can not reject the null hypothesis for the proxy values  $\hat{\tau}_i$ . This, in turn, implies that we will most likely not reject the null hypothesis at the same significance level for the real (unobserved) values  $\tilde{\tau}_i$ .<sup>10</sup> Effectively the fraction  $F$  may thus be referred to as “likelihood” of the time series to have been generated by a Poisson process.

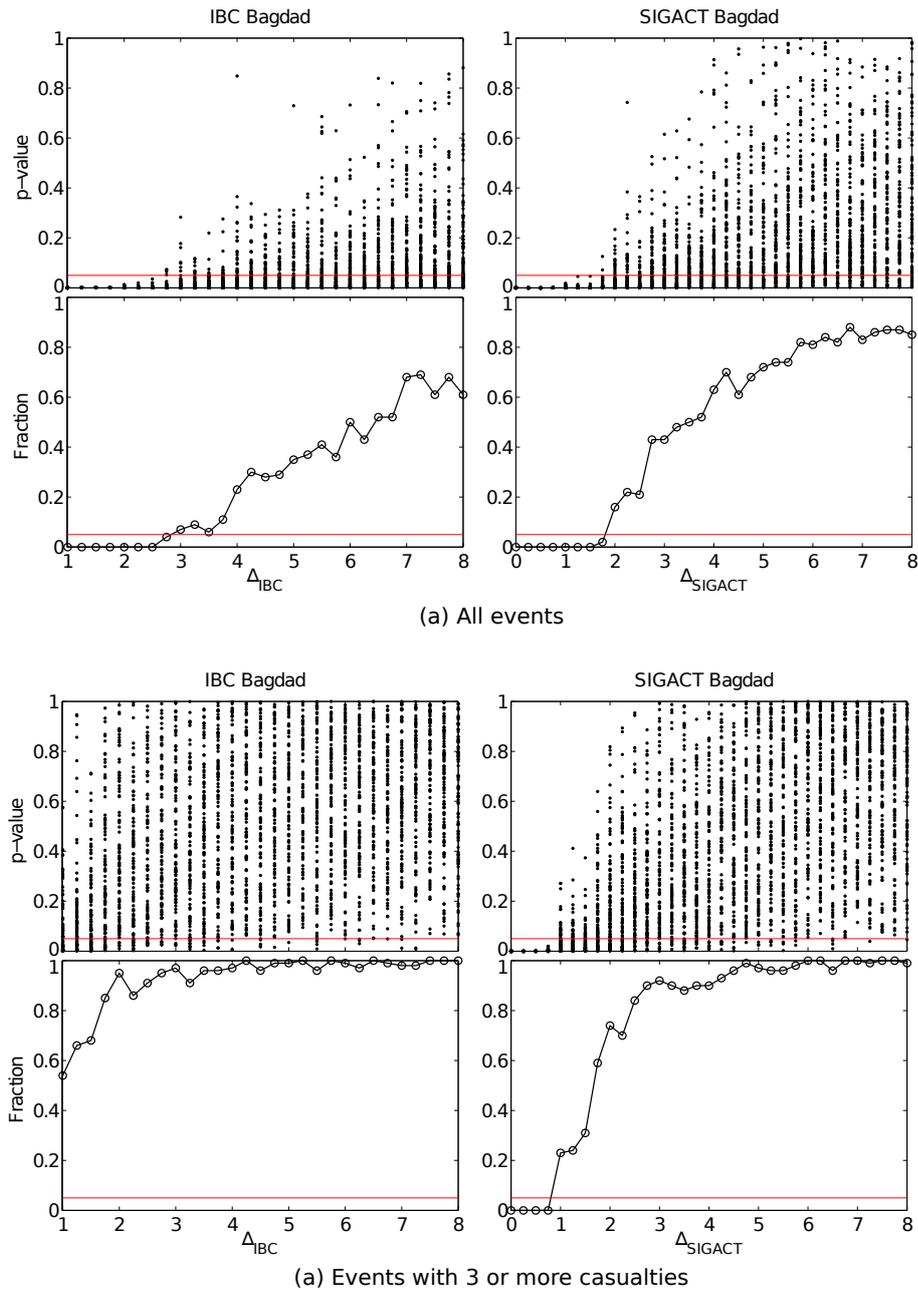
From a conceptual point of views, the random time shifts  $\hat{\tau}_i = t_i - \xi_i$  simply introduce bias to the time-series: the larger  $\Delta$ , the larger the “randomness” in our proxy time-series  $\hat{\tau}_i$ . Note that the more robust the timing signatures in the data, the larger the uncertainty  $\Delta$  at which  $\hat{\tau}_{i,IBC}$  and  $\hat{\tau}_{i,SIGACT}$  start to only represent *iid* random samples from the exponential probability distribution. The functional dependence of  $F$  on  $\Delta$  is thus a quantitative measure for the robustness of the timing signatures. In particular, we will identify the critical value of  $\Delta_c$  for which we can be more than 95% certain, i.e.,  $F < 0.05$ , that uncertainties in timestamps do not destroy the non-trivial signature in  $\hat{\tau}_{i,IBC}$  and  $\hat{\tau}_{i,SIGACT}$ .

Figure 5.9 shows the p-values of the KS-test and the fraction  $F$  as a function of the value of  $\Delta$  for the time window October 15, 2006 to February 15, 2007—a period specifically chosen to reflect a situation where both full datasets show non-trivial timing signatures but where for larger thresholds this signature breaks down in IBC. For both IBC and SIGACT the figure clearly demonstrates that the non-trivial timing distributions in the full datasets are quite robust to uncertainties in timestamps with  $\Delta_{c,IBC} \approx 3$  days and  $\Delta_{c,SIGACT} \approx 2$  days respectively (Figure 5.9a). Notice, too, that the transition to Poissonian dynamics for increasing  $\Delta$  is continuous and relatively slow. At uncertainties of about 5 days (IBC) and 4 days (SIGACT) 50% of the reshuffled datasets are indistinguishable from featureless data. Note that we also analyzed events with 3 or more casualties (Figure 5.9b). Here IBC clearly does not feature robust non-trivial timing signature since already at the minimal uncertainty of one day  $F$  is close to 1. For SIGACT we do observe a non-trivial signature and  $\Delta_{c,SIGACT} \approx 2$  suggests that this signature is similarly robust as that observed for the full dataset.

Our analysis thus suggests that—where they exist—the non-trivial timing signatures for the full IBC and SIGACT data are indeed quite robust against uncertainty of timestamps. In fact, the signatures are robust enough that even if event timing may have been miscoded by up to 2 days, we could still expect to see non-trivial timing dynamics. Note that this does, of course, not imply that timestamp uncertainties of up to 2 days would not affect the inferences we draw from day-by-day and even distributional comparison—it only suggests that some timing information will be preserved.

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<sup>10</sup>As a consequence of the nature of the statistical test used here we reject the correct null hypothesis in 5% of the cases by chance and we thus effectively expect to obtain  $F_{max} = 0.95$  even if the dataset is completely featureless.



**Figure 5.9:** Robustness of timestamps. We test whether the inter-event timing distribution of “IBC Bagdad” (left) and “SIGACT Bagdad” (right) in the time window October 15, 2006 to February 15, 2007 exhibit non-trivial timing signatures for different timestamp uncertainty  $\Delta$ . (a) shows the results for the full datasets and (b) for threshold equal to 3 casualties per event. The top panels illustrate how for 100 different redistributions (see text for details) the p-values for the test for exponential distribution of the inter-event times changes as a function of  $\Delta_{IBC}$  and  $\Delta_{SIGACT}$ . The horizontal red line corresponds to the significance level of 0.05, below which the null hypothesis of exponential distribution can be rejected. The bottom panels show the fraction  $F$  of realizations (out of 100) for which the exponential distribution can not be rejected.

## **5.4 Discussion and Conclusion**

In this study we systematically identified a number of key quantitative differences between the event reporting in media-based IBC data and field report-based SIGACT military data. In fact, we find significant differences in reporting at all levels of analysis: aggregate, monthly, distributional and day-by-day comparisons. These relative biases are consistent with a number of structural differences of the reporting in IBC and SIGACT. We further showed that even for subsets of events where both datasets were found to be most consistent at an aggregate level, the daily time series of events were significantly different. Overall this suggests that at any level of analysis the specific choice of dataset may have a critical impact on the quantitative inferences we draw—at the extreme using IBC or SIGACT data might, in fact, lead to substantially different results.

In an individual analysis of each dataset we further showed that SIGACT and IBC differ markedly with regard to their usefulness for event timing analyses—a key application for both datasets. In fact, IBC was found to have only trivial timing signatures, i.e., signatures indistinguishable from an *iid* random process, for much of the time period analyzed. In comparison SIGACT codes much more non-trivial timing dynamics and is thus generally more suitable for the analysis of event timing. In the low intensity conflict phases prior to 2006 and after mid-2008, however, even SIGACT generally does not feature non-trivial timing dynamics. This strongly suggests that any analysis of event timing and causal relationships between events using SIGACT should best be restricted to the period 2006 to 2008. Our analysis, however, also confirmed that where non-trivial timing signatures for the full datasets exist these signatures are quite robust against uncertainties in timestamps of events. In order not to be systematically affected by geographically biased coverage, our quantitative analysis focused exclusively on the case of Baghdad. We contend, however, that the relative as well as absolute differences in reporting of IBC and SIGACT extend beyond this “best case” scenario to all of Iraq. In other words, for the full Iraq datasets reporting differences are at best what we found here but are likely even more pronounced due to fundamentally more limited event coverage outside of the greater Baghdad area.

Our findings have a number of concrete implications for recent studies analyzing the conflict in Iraq. First, we would like to re-emphasize that the substantial disagreement between the two datasets suggests that using one or the other will likely yield substantively different results. This applies to studies using IBC data at a distributional (Bohorquez et al., 2009) or aggregate level (Condra & Shapiro, 2012), but most notably to studies using IBC (Johnson et al., 2011; Lewis et al., 2012; Lewis & Mohler, 2011) or SIGACT (Braithwaite & Johnson, 2012; Linke et al., 2012) data at a daily resolution where the differences are most substantial. The lack of simultaneous agreement with regard to number of events and casualty counts per months implies in particular that time series analysis with models that describe both event occurrence and casualties—for instance, models of marked point processes (Daley & Vere-Jones, 2008)—may lead to substantially different results depending on which dataset is used, even if focusing on subsets of events of certain minimal sizes.

Second, the absence of non-trivial timing signatures for significant parts of both datasets may

pose a substantial problem if data is used for detailed timing (or causal) analysis. In fact, none of the above mentioned studies using either IBC or SIGACT data at a daily resolution confirmed whether they actually feature robust timing signatures. The analyses in Lewis et al. (2012) and Lewis & Mohler (2011), for example, employ a Hawkes point process model (Hawkes, 1971a,b) to study event timing dynamics. However, our analysis suggests that the IBC data used is almost featureless at short time-scales, having only long-term non-stationary trends for long periods in 2005, 2006 and 2008. It is therefore clearly not suitable for this kind of analysis. Moreover, given the daily resolution of timestamps in IBC and the corresponding clustering of events on a given day, we strongly caution against the direct calibration of a Hawkes model even where robust timing signatures exist, simply because the resulting model fits will be (falsely) rejected by standard goodness-of-fit methods. Instead, it is better to rely on randomization techniques such as those proposed in Filimonov & Sornette (2012) and used for the timestamp analysis in our study. Note also that the absence of non-trivial timing signatures in SIGACT prior to 2006 and after mid-2008 may affect the inferences regarding causal relationship between events in Braithwaite & Johnson (2012); Linke et al. (2012)—this applies particularly for Braithwaite & Johnson (2012) which analyzes event dynamics exclusively in the first six months of 2005.

The growing number of recent contributions addressing issues of bias in conflict event data (Eck, 2012; Chojnacki et al., 2012; Raleigh, 2012; Weidmann, 2013) points to an increased awareness for data related issues in conflict research. Our study contributes to this literature by systematically analyzing relative biases in conflict event data and relating them to structural differences in reporting. The sources of systematic bias discussed here are, however, clearly not restricted to conflict data. For researchers using data on other social processes that may be subject to similar biases our analysis suggests two important “lessons learned”. First, the often very substantial differences between the two datasets analyzed here should raise awareness that data bias is not an afterthought but a critical issue worthy of our fullest attention. In particular, if analyses are meant to provide concrete policy advice we must be especially wary that substantive findings do not arise from biased inference. Second, we demonstrated how structural differences in reporting directly translate into relative biases. This suggests, that a careful *a priori* understanding of the strength and limitations of a given dataset allows to anticipate possible biases in subsequent analyses—even if there is only one dataset that covers the case in question. If more than one comparable dataset exists one can either directly analyze their relative bias or, at least, perform the same analysis for all datasets to verify that the substantial conclusions drawn are robust and consistent. We also showed that statistical tests may help identify datasets that are more suitable than others for the analysis at hand.

To date most studies using these data unfortunately neither address potential biases nor systematically test the robustness of their findings. There is certainly not one comprehensive strategy to mitigate bias in empirical data but the present study suggests that researchers can at least actively address it. Especially with the growing availability of large and highly-resolved datasets it will be more important than ever that issues of data quality are taken seriously. As the case of the conflict in Iraq shows, if unaccounted for, we otherwise face the risk that the “views to a war” will indeed be driving our substantial findings.



# 6 Severity matters: Analyzing the spatiotemporal relationship of small- and large-scale violence in Iraq<sup>†</sup>

## Abstract

A central question in the quantitative literature on civil conflict concerns the impact of the severity or scale of events on subsequent conflict dynamics. In classifying events—for example as indiscriminate compared to selective violence—studies typically rely on detailed information about the type of event. In many cases, however, such information is absent or incomplete. Alternatively, one can directly rely on casualty figures to categorize events by severity. While this is often quite problematic due to the considerable uncertainty associated with casualty counts, we show here that this problem can be mitigated using a robust statistical classification into only two broad casualty categories: small- and large-scale violence. Spatiotemporal clustering analysis then reveals systematic differences between small- and large-scale violence in Iraq that vary with conflict phases and geographical region. In particular, we find that large- compared to small-scale violence much more strongly affects subsequent cascades of events. Our findings thus underscore the necessity to consider event severity in the analysis of civil conflict dynamics. We further outline concrete implications for policy makers and practitioners.

## 6.1 Introduction

In recent years quantitative research on civil conflict has increasingly focused on dynamics and mechanisms at detailed, sub-national units of analysis—a paradigm shift accompanied and facilitated by a corresponding increase in the collection of highly disaggregated conflict (event) data. The disaggregate approaches to the study of civil conflict respond to a key criticism leveled at country-level analyses, the observation that there exists “[...] a fundamental mismatch between many civil war theories and their empirical applications” (Eck, 2012, 124). Consequently,

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research at smaller, sub-national units of analysis aims to study causal mechanisms at the level at which they are theorized to operate, closing the apparent gap between concepts and data (Kalyvas, 2008).<sup>1</sup>

Recent studies using spatially disaggregated data have, for example, analyzed (endogenous) conflict diffusion processes (Schutte & Weidmann, 2011), demonstrated how demographic and geographical factors shape the way in which the exclusion of ethnic groups from power affects the probability of ethno-nationalist civil war (Cederman et al., 2009) or investigated the link between violence and ethnic segregation (Weidmann, 2011; Weidmann & Salehyan, 2013). Other studies have concentrated specifically on cities studying origins and consequences of urban violence (Bhavnani et al., 2014; Urdal & Hoeschler, 2012; Vargas, 2009). Disaggregating by politically relevant ethnic groups and accounting for their access to executive-level state power, Wimmer et al. (2009) and Cederman et al. (2010, 2011b) show that not ethnic diversity as such but ethnic power relations are decisive in understanding the onset of armed (ethnic) conflict.

We here focus on a central question in the quantitative literature on civil conflict that specifically concerns the impact of the severity or scale of events: *How does the scale of violence affect subsequent conflict dynamics?* Prior research has, in particular, emphasized the role of selective as compare to indiscriminate violence in civil conflicts. Using data that is both disaggregated in space and by actor groups Kalyvas (2006, 2008, 2012) and Bhavnani et al. (2011) analyze how territorial control shapes the use of selective vs indiscriminate violence. The study of Lyall (2009) investigates whether indiscriminate violence incites insurgent attacks using spatially disaggregated data on Russian artillery strikes and insurgent violence in Chechnya. Condra & Shapiro (2012) show that in Iraq insurgent attacks increased following coalition attacks with civilian casualties, while in turn civilian casualties caused by insurgent attacks decreased insurgent violence. Further testing the effect of civilian agency, Schutte & Donnay (2014) demonstrate that indiscriminate insurgent violence increased civilian support for the U.S.-led coalition troops in Iraq.

In classifying the scale of events these studies typically rely on detailed information about the type of event. It is important to note though that this is usually inherently linked to event severity. For example, the classification of artillery or air strikes as instances of indiscriminate violence (Bhavnani et al., 2011; Lyall, 2009) entails that such events tend to lead to more casualties as they are generally launched without regard for the fate of innocent bystanders. In contrast, the selective targeting of groups or individuals (Bhavnani et al., 2011; Kalyvas, 2012) implies that the use of force is generally more restrained and leads to fewer casualties.<sup>2</sup> However, given the substantial uncertainty in the reporting of casualty figures (Chojnacki et al., 2012; Eck, 2012; Weidmann, 2013) and the fact that casualty counts do not necessarily provide an adequate

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<sup>1</sup>For a more detailed discussion of micro-level approaches in contrast to country-level studies, please refer to Cederman & Gleditsch (2009), Donnay et al. (2014) or Kalyvas (2008).

<sup>2</sup>Note that this distinction also has a strong normative component: extensive shelling can simply not be justified as selective violence and thus there exists a strong incentives to use the utmost restraint when selectively targeting groups or individuals.

representation of the intended scale of an attack (Rogers, 2010b), the literature on civil conflict has generally been very reluctant to explicitly use casualty counts to disaggregate by severity.

We fully acknowledge the problems associated with classifications based on severity but show here how these can be effectively mitigated by using a robust statistical classification into two broad casualty categories: small- and large-scale violence. This categorization is in principle applicable to a wide range of empirical data complementing the categorization by event type in cases where the reporting of the kind of incident itself is biased or incomplete.<sup>3</sup> The large geo-referenced event dataset on Iraq for the period 2004–2009 used in our study offers a unique perspective on the conflict dynamics but its event reporting is in many cases too unspecific to allow for a reliable coding of events by type. Disaggregating events by severity then makes it possible to nonetheless systematically test a number of hypotheses regarding the spatiotemporal dynamics of small- and large-scale violence and their mutual interdependence.

This paper proceeds as follows: After discussing existing research, we outline our theoretical argument regarding disaggregation of events by severity. Turning to our empirical case, we derive hypothesis for the dynamics of small- and large-scale violence in Iraq, introduce the data and statistically classify the incidents into the two broad categories. We then systematically analyze their spatiotemporal dynamics for different phases of the war and for different geographical regions in Iraq. We conclude by discussing concrete implications of our analysis for counterinsurgency practices.

## 6.2 Disaggregating severity

Disaggregating events by type (or character) has been a core interest of both the theoretical and empirical literature studying sub-national conflict dynamics (see for example Kalyvas, 2006; Lyall, 2009). The intuition is that just knowing where and when incidents occur is often not sufficient to gauge their effect on conflict dynamics but that their severity and strategic value critically matter—a massive car bomb in a strategic location is usually thought to have a very different impact on subsequent conflict dynamics than a single person shot on a street.<sup>4</sup>

The existing literature on civil conflict has most prominently considered such distinctions in the context of the use of selective compared to indiscriminate violence in counterinsurgency campaigns. The importance of “winning hearts and minds” of the civilian population was first widely discussed in the context of the Vietnam war and the concept of so-called “population centric” warfare has since become a key part of contemporary counterinsurgency doctrines (DoS, 2009). In the context of the wars in Afghanistan and Iraq, General McChrystal has also famously

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<sup>3</sup>A classification into broad severity categories may in some cases actually also be preferable from a theoretical and methodological point of view, in particular if the impact of events is explicitly linked to the number of casualties. Condra & Shapiro (2012), for example, explicitly consider casualty figures in their analysis of the strategic effect of (collateral) civilian casualties in Iraq.

<sup>4</sup>This is, of course, not necessarily true if the individual targeted is highly relevant to a conflict party and the attack thus has a strong strategic and symbolic character.

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referred to this as “insurgent math” (Hastings, 2010).

The argument is that perpetrators face negative consequences from the use of indiscriminate as compared to selective violence. The deaths of civilian bystanders—often accompanied by the destruction of houses and civilian property—but also excessive violence against combatants are typically thought to sway civilian support towards the insurgents (Kalyvas, 2006; Kalyvas & Kocher, 2007). In fact, insurgents may even strategically provoke such attacks to shift public support in their favor (Ellsberg, 1970). On the other hand, insurgents may also engage in large-scale violence targeting security forces or use highly visible attacks to undermine the state’s ability to guarantee security (DoS, 2009, 9).

Practitioners additionally stress the increasing importance of information in shaping the impact of counterinsurgency campaigns (Petraeus, 2006; Sepp, 2005). In other words, just as important as the severity of incidents is if and how incidents are covered in the media and whether the coverage reaches a broad audience or not. Using cross-national data on mass media accessibility in the post-World War II period, Warren (2014) recently showed that information superiority is indeed a key factor in countering insurgencies.<sup>5</sup>

In the tradition of Richardson’s seminal work on the size of interstate wars (Richardson, 1948), quantitative research on the dynamics of insurgent conflict has statistically characterized the occurrence of events with different severity. Clauset et al. (2007) showed that the severity of terrorist events follows a robust power law, suggesting a striking regularity in how events with different casualties occur. This research also points to a close link of an insurgent organization’s size (or capacity) and the severity of violence: the larger the group or its capacity, the more severe the events it can carry out (Bohorquez et al., 2009; Clauset & Gleditsch, 2012).

Before turning to our classification of the “scale” of violence we would like to emphasize that the dividing line between different types of violence becomes increasingly blurry in situations of weak state control and deteriorating security (van Creveld, 1991). In particular, large-scale organized and strategic violence often co-exists with small-scale opportunistic criminal or intergroup violence (Green & Ward, 2009, 611). In our classification we therefore follow the typology of collective violence outlined in Tilly (2003). Tilly’s key distinctions for the categorization of the “scale” of violence are the *salience* of damage and the degree of *coordination* among actors (Tilly, 2003, 13). In other words, even if different types of violence—criminal and strategic violence, for example—start to significantly overlap we can still categorize violence by the degree to which violence is used and by how coordinated or strategic it is.<sup>6</sup>

In developing our statistical classification of events we build on the distinct empirical regularity in the size distribution of terrorist events identified by Clauset et al. (2007). The power law signature in the tail of the severity distribution suggests, in particular, that the dynamics of terrorist events

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<sup>5</sup>Note that media reporting is, at least partly, endogenous to the scale of violence since large incidents are much more likely to be covered in the media (McCarthy et al., 1996).

<sup>6</sup>In Tilly (2003) damage encompasses deadly violence but also more generally loss of property, injury etc.. We here use it more restrictively as we are only concerned with casualties.

are—above a certain minimum size—scale invariant. This implies that, at least from a statistical point of view, events of very different sizes are consistent with one violence mechanism (Clauset et al., 2007, 59). In fact, the sort of strategic large-scale attacks intended for maximal destruction and visibility analyzed in Clauset et al. (2007) are empirically the only kind of general violence mechanism that can account for a very wide range of severities. Recall also that the ability to stage such coordinated large-scale attacks is closely related to the size or capacity of the militant group (Bohorquez et al., 2009; Clauset & Gleditsch, 2012). This kind of highly strategic and coordinated *large-scale violence* thus forms our first broad event category. Note that we here remain intentionally vague about defining more specific violence mechanisms since strategic large-scale attacks may, in fact, arise from a variety of mechanisms specific to the empirical case.<sup>7</sup>

In contrast, *small-scale violence* then corresponds to all events whose sizes do not follow the same power law regularity in the distribution of event sizes suggesting that it encompasses attacks that result from multiple different and independent processes. In Tilly’s typology this is consistent with event categories in the medium to low range of both salience and coordination where the overlap of different event types is by far the largest (Tilly, 2003, 15). Small-scale violence thus likely also includes a significant fraction of individual level violence.

The classification into small- and large-scale violence is first of all empirically driven: we chose it to separate events that are clearly identifiable based on their statistical signature from those that are not. It is, however, also driven by the more subtle characteristics discussed here that are implicit in the definition and that closely correspond to the theoretical and empirical perspectives on insurgent conflict we reviewed before. Note, in particular, that the distinction into only two broad severity categories follows naturally from the statistical classification and is not an *ad hoc* definition.

Formally, we then simply define large-scale attacks as all events in the tail of the severity size distribution, i.e., events with minimal severity  $\lambda$  such that their severity size distribution follows a power law. Correspondingly, small-scale attacks are all events with less than  $\lambda$  casualties per event. In classifying small- and large-scale violence from empirical data we rely on the TP statistic (Pisarenko & Sornette, 2006), a robust, non-parametric test statistic designed to identify at which cutoff  $\lambda$  the power law tail of a distribution begins. It has previously been introduced as a powerful technique for the analysis of severity statistics by Cederman et al. (2011a) in the context of interstate war sizes. The TP statistic is defined such that it approaches 0—within its confidence bounds—if the tail is statistically indistinguishable from power law. In practice, the statistic is calculated for a large range of possible cutoffs and thus provides a continuous estimation of where the tail begins.

It is important to note that there are two well-known systematic limitations associated with the

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<sup>7</sup>In the typology of Tilly, for example, this kind of large-scale strategic violence would primarily encompass “violent rituals” and “coordinated destruction” (Tilly, 2003, 15). Note that large salience of damage does not strictly always imply many casualties—in this sense our typology is more restrictive than that of Tilly (2003).

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reporting of casualty figures that may, in principle, affect our event classification. If the scale of an attack itself is misrepresented because of reporting bias (Chojnacki et al., 2012; Eck, 2012; Weidmann, 2013) this directly affects classification of an incident. Furthermore, the number of inflicted casualties may not accurately represent the intended scale of the attack (Rogers, 2010b)—a car bomb gone off early or in the wrong location, for example—potentially making it difficult to infer intention from the event severities we observe.

The classification we use, however, is designed to be as insensitive as possible to these kinds of issues. First, we rely on large samples both in our statistical classification and in the subsequent analysis. Our analysis is thus not very vulnerable to miscoding of individual incidents. Second, we only aim to classify events into broad casualty categories, which implies that we only require the order of magnitude of an event to be correctly represented in the dataset, i.e., did an attack lead to 2 or 20 casualties. We then systematically control for the robustness of the classification by re-analyzing our data for a range of  $\lambda$ -values, demonstrating that our results are insensitive to the exact choice of classification cutoff. Note that these prescriptions can, of course, not account for systematic *one-sided* reporting bias or misrepresentation of the intended scale of attacks. In such cases the violence categories we derive would likely simply be shifted compared to the “true” value. However, as long as biased reporting both over- and under-estimates true casualty counts, the classification can be expected to be robust.

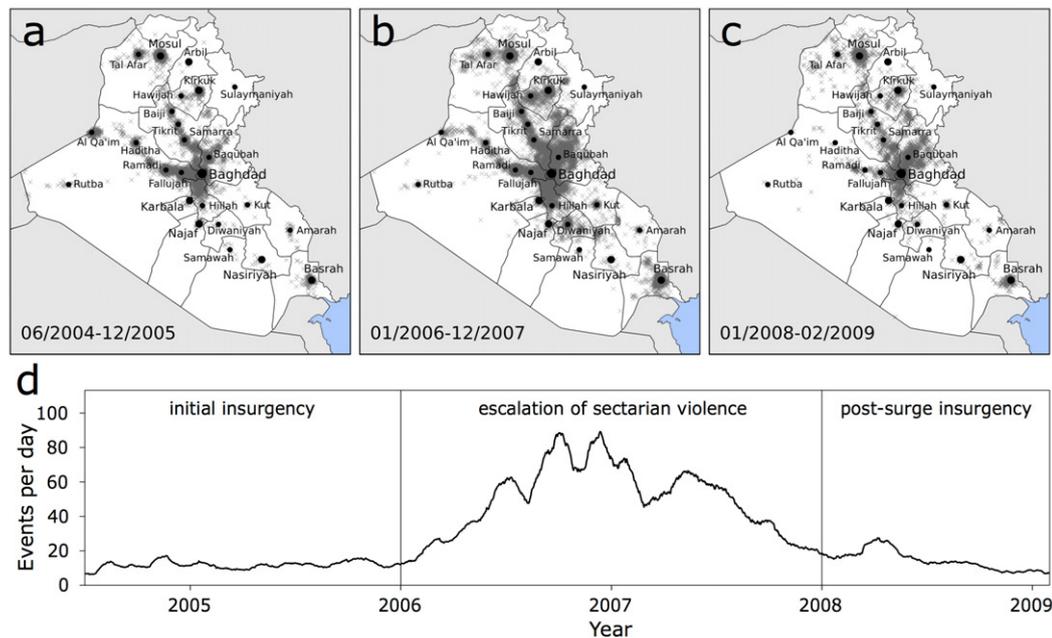
### 6.3 The case of Iraq

The conflict in Iraq ranks among the most violent insurgent conflicts of the early 21<sup>st</sup> century with estimates of civilian fatalities exceeding 130,000 by mid-2014 (IBC, 2014).<sup>8</sup> Following the U.S. invasion in mid-2003 the conflict went through a number of distinct phases (Figure 6.1). Initially, the conflict started out as an insurgency directed at the U.S.-led coalition troops and the Iraqi central government and was driven by forces loyal to Saddam Hussein. However, by early 2004 groups of radical religious militants—some of them formed or supported by foreign jihadists—and Iraqis opposed to the foreign occupation carried out the majority of attacks. This initial insurgency intensified and expanded throughout 2004 and 2005, first affecting mainly central and southern Iraq but later also Al Anbar, Salah ad-Din and Ninawa (Figure 6.1a and 6.2).

In 2006 and 2007 sectarian violence between the Shia majority and Sunni minority rapidly escalated in addition to the ongoing insurgency, embroiling the majority of populated areas in Iraq in often excessive violence (Figure 6.1b). The U.S.-led troop ‘surge’ in 2007—a substantial increase of U.S. military personnel on the ground accompanied by a major shift in counterinsurgency tactics (Kagan, 2009; Petraeus, 2006, 2010)—eventually led to a significant deescalation of the conflict throughout 2008 and 2009 (Figure 6.1c), largely by curbing sectarian violence but also by more effectively countering the insurgency. After the US withdrawal from Iraq in 2011 the country continues to experience often massive violence on an almost daily basis. The conflict

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<sup>8</sup>The estimates for the number of civilian fatalities in Iraq differ substantially, see for example <http://www.iraqbodycount.org/analysis/beyond/exaggerated-orb/fordetails>.



**Figure 6.1:** Conflict phases in Iraq. Panels a-c show the spatial distributions of violence in the three main periods covered by our data; intensities are normalized to period length and directly comparable. d shows the time series of the number of events per day (one month moving average).

noticeably intensified again in 2014, the first six months being the most violent period since the 2007 troop surge (IBC, 2014). This reflects a dramatic escalation of the insurgency and a resurgence of sectarian violence—even before the effective take-over of the north-western (Sunni) provinces by the Islamic State of Iraq and the Levant (ISIL), an Al-Qaeda affiliate, in June that now threatens the very existence of a multi-ethnic Iraq.

In our analysis we draw on detailed conflict event data available through *The Guardian* (Rogers, 2010a). The dataset covers 48,734 episodes of deadly violence in Iraq in the period June 1, 2004 to February 28, 2009.<sup>9</sup> This period encompasses the first three main phases of the conflict and thus allows us to study variation across distinctly different conflict periods. Entries in the dataset carry minute resolved timestamps, are geo-located and provide detailed casualty counts. While each entry also contains general context information regarding the type and category of the incident and a short description, these categories are in many cases too unspecific to allow for a reliable coding of events by type.

It is also important to emphasize that we here explicitly do not disaggregate by perpetrator identities. Prior work has relied on information regarding the “type” of events and the “affiliation” of perpetrators to distinguish events initiated by coalition and insurgent forces (Linke et al., 2012). Note, however, that these categorizations in many cases do not reliably identify the

<sup>9</sup>For details please refer to Section D.1 of the supplementary information. The data and their limitations are also discussed in detail in Donnay & Filimonov (2014).

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**Figure 6.2:** Provinces of Iraq.

actual perpetrator of an attack. Note, for example, that “Friendly Actions”—the perpetrator affiliations reported here are exclusively “FRIEND”, i.e., coalition or Iraqi forces—are not limited to incidents actually perpetrated by coalition or Iraqi forces. They also contain reports about shootings among civilians or cover episodes of violence where casualties can not be clearly ascribed to enemy action. Moreover, events are routinely tagged as “Friendly Actions” that were actually not initiated by coalition or Iraq forces but in which then only insurgents suffered casualties.

### 6.3.1 Conflict dynamics in Iraq

The conflict in Iraq has not only attracted substantial scholarly attention,<sup>10</sup> it is also highly relevant to policy makers as well as practitioners. In particular, it has had a marked effect on contemporary U.S. counterinsurgency doctrines (DoS, 2009; Petraeus, 2006, 2010). In the public perception especially the massive loss of human life stands out and large-scale attacks regularly make headlines around the globe. Averting such attacks may thus not only improve the security situation but also positively affect public perception and the legitimacy of the government. It is also extremely relevant from a more general policy point of view since large-scale violence in insurgent conflict typically accounts for only a fraction of total incidents but a very substantial part of all casualties. They are therefore typically a natural priority of counterinsurgency policies.

Prior research on Iraq has already recognized the importance of the scale of attacks on subsequent conflict dynamics. The work of Condra & Shapiro (2012), for example, explicitly considers casualty figures when analyzing the strategic effect of (collateral) civilian casualties in Iraq. Schutte

<sup>10</sup>See, for example, Braithwaite & Johnson (2012); Condra & Shapiro (2012); Linke et al. (2012); Schutte & Donnay (2014); Weidmann & Salehyan (2013).

& Donnay (2014) take into account detailed information about the scale of attacks and demonstrate that indiscriminate insurgent violence increases civilian support for the U.S.-led coalition troops in Iraq. Our study, however, goes significantly beyond this: we separately analyze the spatiotemporal dynamics of small- and large-scale violence and their relationship, which allows us to cleanly disentangle their effect.

Before turning to our theoretical expectations for the effect of small- and large-scale attacks on subsequent levels of violence, we would like to emphasize that the violence dynamics in Iraq—whether in phases dominated by insurgent or by sectarian violence—generally have a strong “tit-for-tat” character (Linke et al., 2012).<sup>11</sup> We can therefore expect that if attacks have substantive effects on subsequent levels of violence these effects are on average positive, i.e., lead to an increase in levels of violence.

In addition to the ongoing insurgency, Iraq experienced significant sectarian violence, especially in 2006–2007 and to a lesser extent post-2007. While this also led to strategic large-scale attacks, much of the sectarian conflict played out on a much smaller scale, often at the neighborhood level (Weidmann & Salehyan, 2013). The generally deteriorating security situation throughout the conflict at the same time resulted in a noticeable rise of criminal violence (Green & Ward, 2009). Further, small-scale violence likely also encompasses opportunistic violence—revenge killings, for example—or incidents arising from individual aggression (Tilly, 2003, 15). In comparison, large-scale violence in Iraq empirically largely corresponds to strategic and coordinated attacks, most often using IEDs—both targeting the military and civilians. Insurgents and the different sectarian factions also stage coordinated shooting attacks and ambushes.

With small-scale violence arising from a variety of different mechanisms and given its typically less strategic character, it can be thought of as a measure for the overall level of violence at a given location and a given point in time. Together with the general “tit-for-tat” character of violence in Iraq we can therefore expect that: *(I) Small-scale attacks typically have a systematic positive effect on subsequent levels of small-scale violence.* Given their difference in strategic character we would then also generally expect that: *(II) Small-scale attacks typically have no or only a weak effect on large-scale violence.* Note though that this does not always hold true. Consider for example a situation in which small-scale violence slowly escalates until it reaches a tipping point after which the fighting becomes of strategic relevance—may it be through increased visibility or the fact that violence has escalated so far that the conflict parties have significant stakes in the conflict. In such a situation we should, on the contrary, see a significant increase in the number of large-scale attacks following small-scale violence. We would, however, expect this situation to arise only very rarely and the corresponding effect to be very weak.

The strategic nature of large-scale violence and its comparably greater visibility would, in turn, suggest that: *(III) Large-scale attacks typically have a substantial positive effect on subsequent levels of both small- and large-scale violence.* But we would also expect that: *(IV) The effect*

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<sup>11</sup> Similar “tit-for-tat” dynamics have, for example, also been shown to exist in the Israeli-Palestinian conflict (Haushofer et al., 2010).

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*of large-scale attacks is systematically larger on the occurrence of subsequent large-scale than on small-scale attacks.* The latter derives from the fact that we can expect “tit-for-tat” dynamics to be most substantial for the most noticeable and clearly recognizable attacks. Based on the observation that reciprocity tends to be strongest in small spatiotemporal distances (Linke et al., 2012) we can further expect that: (V) *The effect of both small-scale and large-scale attacks on subsequent levels of violence are typically strongest in close spatial proximity and right after an attack.*

In principle both small- and large-scale violence could have an effect primarily on subsequent violence in the same location as an attack—a violence *hot spot*—but also an immediate effect on levels of violence in adjacent locations—we will refer to this as a *hot phase*. Whether the effect of attacks on subsequent events carries a *hot spot* or *hot phase* signature (or both) is primarily a question how local or not the conflict dynamics are. This likely varies significantly both by geographical region and by conflict phase. We can, however, in general expect that: (VI) *Small-scale violence tends to have a stronger hot spot than hot phase signature.* Given the lower visibility of small-scale violence it is simply generally more likely that it affects levels of violence in its direct vicinity.

It is important to emphasize that while we have used arguments of reciprocity to motivate our theoretical expectations we can not explicitly test reciprocal violence—for example, between insurgents and security forces. In fact, given the absence of perpetrator information, our empirical tests will generally not be able to distinguish the effect of reactive violence—cascades of attacks and counter-attacks, for example—from the clustering of events as a result of coordinated attacks by only one side of the conflict.

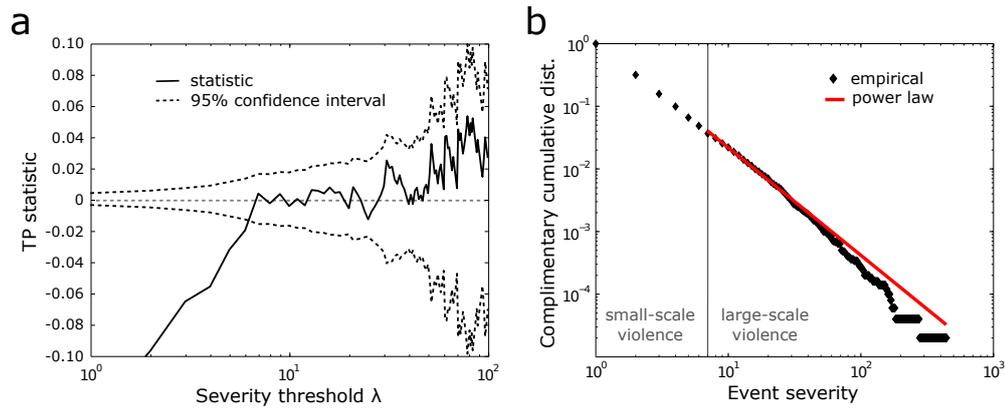
### 6.3.2 Spatiotemporal dynamics of small- and large-scale violence in Iraq

The starting point of our empirical analysis is the statistical classification of all events in our dataset following the methodology outlined before. Note that we use the full period 2004–2009 for classification in order to guarantee that it is as robust as possible and consistent across the whole dataset.<sup>12</sup> Figure 6.3a shows the change of the TP statistic with increasing threshold value  $\lambda$ . The full distribution clearly does not follow a power law. Subsequently excluding incidents with few casualties rapidly improves the statistic. It first converges towards zero and stays within the confidence bounds—indicating a power law tail—at a threshold of  $\lambda = 7$  and then oscillates around zero for a range of larger threshold values. We therefore categorize all events with 6 or less casualties as small-scale and all events with 7 or more casualties in the tail of the severity distribution as large-scale violence (Figure 6.3b).

According to our classification small-scale violence corresponds to over 96% of all events in our dataset accounting for 71% of all casualties, in other words less than 4% are large-scale attacks

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<sup>12</sup>In Section D.2 of the supplementary information we show that the classification for the three main periods (2004–2005, 2006–2007 and 2008–2009) and for individual provinces is substantially identical.



**Figure 6.3:** Statistical classification of events into small- and large-scale violence for the full period 2004–2009. **a** shows the change of the TP statistic as a function of the threshold  $\lambda$ . **b** depicts the complementary cumulative distribution of event sizes.

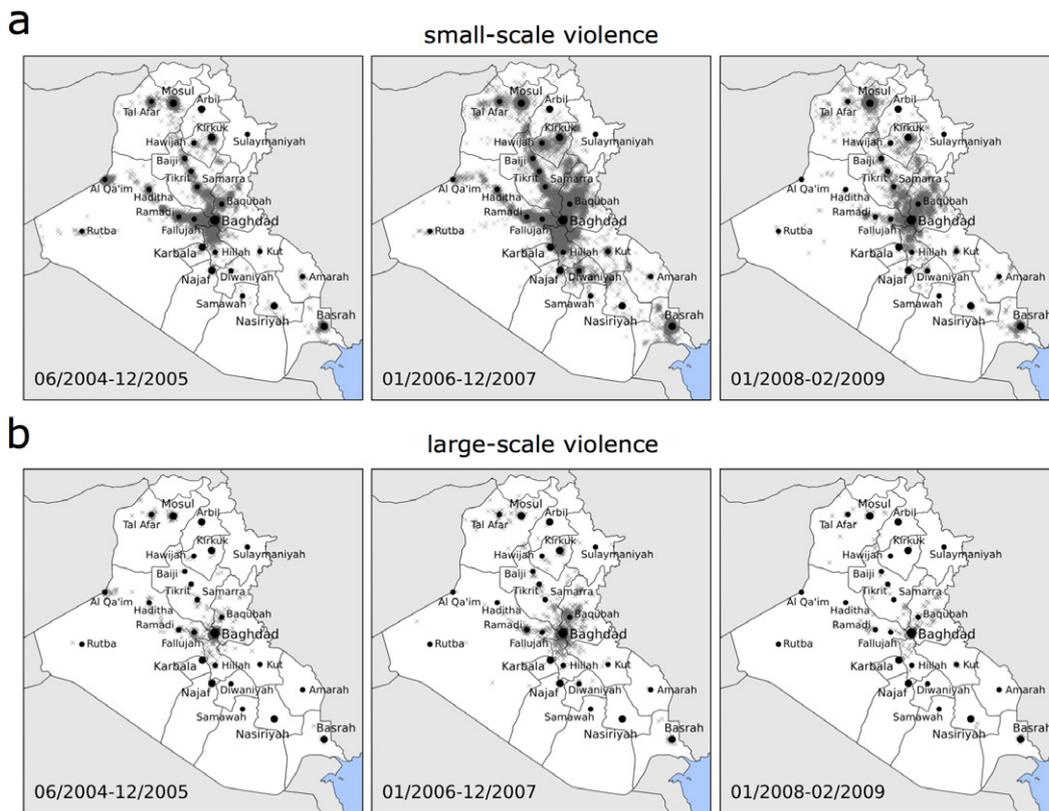
but they account for 29% of all casualties. The most substantial impact of large-scale violence on the death toll in Iraq was clearly during the initial insurgency in 2004–2005 where they account for 43% of all deaths. In the two subsequent periods covered by our dataset this fraction is substantially smaller (2006–2007: 25%, 2008–2009: 26%). We provide the full descriptive statistics for small- and large-scale attacks and corresponding casualties disaggregated by time periods and provinces in Table 6.1.

Figure 6.4 illustrates the spatial patterns of events disaggregated by severity. In the following, we systematically analyze the relationships of small- and large-scale violence using techniques for spatiotemporal cluster analysis. Specifically, these techniques systematically test for significant correlations between the timing and location of any given attack and all subsequent incidents across a given sample. They also provide an estimate of the direction and strength of this relationship. We here favor the use of these measures of systematic co-occurrence over a spatiotemporal

| Location     | 2004–2005   |              |            |             | 2006–2007    |              |             |              | 2008–2009   |             |            |             |
|--------------|-------------|--------------|------------|-------------|--------------|--------------|-------------|--------------|-------------|-------------|------------|-------------|
|              | small       |              | large      |             | small        |              | large       |              | small       |             | large      |             |
|              | #           | cas.         | #          | cas.        | #            | cas.         | #           | cas.         | #           | cas.        | #          | cas.        |
| Al Anbar     | 1636        | 2942         | 120        | 1868        | 3368         | 5294         | 138         | 1733         | 293         | 483         | 19         | 250         |
| Al Basrah    | 156         | 238          | 7          | 98          | 1751         | 2155         | 26          | 293          | 246         | 323         | 14         | 234         |
| Babil        | 441         | 798          | 38         | 700         | 1860         | 2816         | 74          | 1202         | 222         | 338         | 11         | 216         |
| Baghdad      | 1454        | 2598         | 122        | 1839        | 16653        | 23486        | 432         | 6870         | 1738        | 2546        | 52         | 937         |
| Diyala       | 506         | 962          | 46         | 848         | 4576         | 7824         | 239         | 3015         | 746         | 1297        | 40         | 551         |
| <b>Iraq</b>  | <b>6186</b> | <b>10970</b> | <b>469</b> | <b>8272</b> | <b>35044</b> | <b>52142</b> | <b>1158</b> | <b>17385</b> | <b>5681</b> | <b>8612</b> | <b>196</b> | <b>3064</b> |
| Kirkuk       | 219         | 343          | 10         | 166         | 830          | 1273         | 36          | 444          | 217         | 341         | 2          | 74          |
| Ninawa       | 975         | 1718         | 63         | 1169        | 2551         | 3777         | 67          | 1181         | 1509        | 2162        | 28         | 349         |
| Salah ad-Din | 616         | 1055         | 37         | 591         | 2078         | 3540         | 79          | 1215         | 496         | 816         | 17         | 220         |

**Table 6.1:** Violence in all of Iraq and the eight most violent provinces in 2004–2009; # denotes number of incidents, cas. number of casualties. *Note: Al-Muthannia, Al-Qadisiyah, An-Najaf, Arbil, As-Sulaymaniyah, Dhi-Qar, Dihok, Karbala', Maysan and Wasit have too few events for our subsequent statistical analysis and are therefore not shown here.*

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**Figure 6.4:** Spatial distribution of small-scale violence (a) and large-scale violence (b) for the three main conflict phases analyzed.

causal inference design. The latter typically requires low spatial and temporal density of events. This is usually achieved by focusing on relatively specific types of events (Schutte & Donnay, 2014) and therefore not applicable for our broad event categories. Other common techniques require the aggregation of observation in artificial spatial bins (Linke et al., 2012) or district-level time series (Condra & Shapiro, 2012)—a limitation we would strictly like to avoid since spatial aggregation would limit our ability to detect distance scales at which events cluster.

In order to be able to gauge the effect of a variety of confounding factors on the systematic co-occurrences of events, we perform our analysis for different time periods and separately for all of Iraq and individual provinces. This gives us substantial variation on a number of dimensions—predominantly urban vs. rural, low-intensity conflict vs. high-intensity conflict, predominantly Sunni vs. Shia, insurgent conflict vs. sectarian violence—and thus sheds light on how the relationships of small- and large-scale violence we find are influenced by a variety of factors.

In our analysis we rely on the *Knox* test, an elegant non-parametric clustering test that has previously been introduced by Braithwaite & Johnson (2012) for the analysis of conflict event data. It was originally developed in the context of infectious disease spreading for situations

where it is not possible to specify an expected baseline of events when testing for significant clustering—this is generally also the case in conflict event data (Schutte & Donnay, 2014). The Knox methodology relies on a simple permutation test that randomly swaps the location of events while preserving the temporal sequence exactly as in the empirical data.<sup>13</sup>

In line with Braithwaite & Johnson (2012) we perform the test for moving spatiotemporal windows thus overcoming the limitation of choosing arbitrary spatiotemporal bins. Specifically, for all events in the dataset we count the number of subsequent events that lie within a given spatial and temporal window—this is the Knox metric. We then determine the Knox metric for  $n = 1000$  simulated baselines where the locations of events have been randomly swapped. The Knox ratio,  $K$ —the factor by which the number of empirical events deviates from our null expectation—is then simply the empirical Knox metric divided by the average simulated Knox metric. The significance of the Knox ratio estimate is given by  $p = (r + 1)/(n + 1)$ . It is usually calculated for a significant increase of event counts compared to the baseline ( $K > 1$ ) but can equivalently be calculated for a significant decrease ( $K < 1$ );  $r$  then is the number of cases where a simulated Knox metric is larger or equal to or smaller or equal to the empirical Knox metric respectively (Braithwaite & Johnson, 2012).

The Knox test methodology is typically used to detect significant clustering of one class of events (univariate). It can, however, also be used to test for “directed” clustering of events (bivariate), i.e., do events of type A tend to cluster following events of type B. In this case one simply considers temporally ordered pairs of events where events of type B precede events of type A (Braithwaite & Johnson, 2012). In the present analysis we rely on this variation to test whether large events tend to cluster in space and time following small events and vice versa.

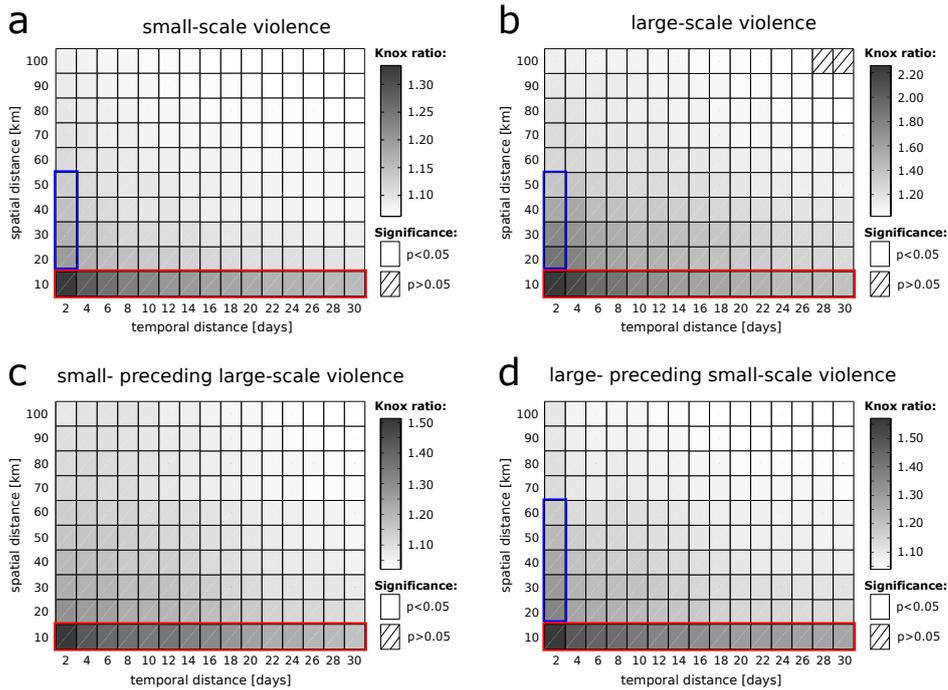
We begin by analyzing the spatiotemporal dynamics for all of Iraq in the period 2004–2005 dominated by insurgent conflict. Figure 6.5 illustrates the results of the Knox analysis graphically as a set of contour plots: The darker the color the larger the estimated clustering (expressed as the Knox ratio  $K$ ), corresponding standard errors are indicated by shading out non-significant estimates. The cells indicating effect size and significance level are arranged in a table where each field corresponds to one specific combination of spatial and temporal sizes used for the Knox test. Additionally, we highlighted the strongest clustering in the direct vicinity of attacks (*hot spots*) in red and clustering of events in space directly following an attack (*hot phases*) in blue.

The analysis clearly reveals systematic clustering of events in space and time with visible differences between the effect of small- and large-scale attacks on subsequent levels of violence. Specifically, we observe predominantly strong *hot spot* signatures for both small- and large-scale violence that reflect the more localized nature of the initial insurgency where fighting generally focused on urban centers and strategic locations (Figure 6.5a-d). Hot phase signatures, however, are only clearly noticeable for large-scale violence (Figure 6.5b and d). In fact, our analysis

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<sup>13</sup>To avoid spurious signatures, we ensure that only locations that were empirically affected in a given period can be part of the corresponding random baseline. Please refer to Section D.3 of the supplementary information for details.

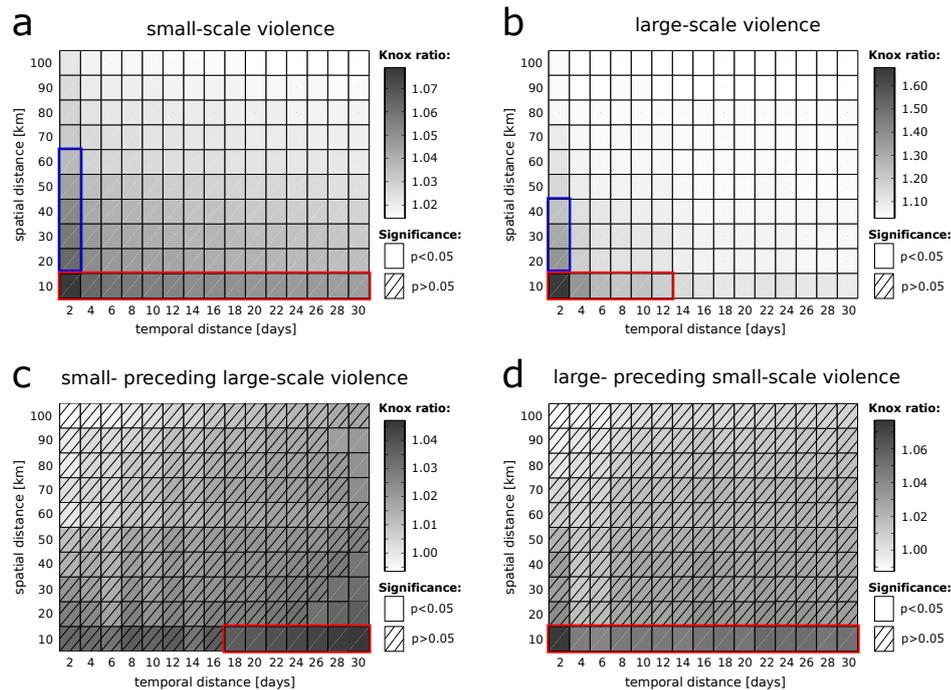
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**Figure 6.5:** Knox test results for the period 2004–2005. Colors indicate the value of the Knox ratio  $K$ , corresponding standard errors are indicated by shading out non-significant estimates. Note that significance levels  $p$  indicate significant spatiotemporal correlation if  $K > 1$  and anti-correlation if  $K < 1$ . *Hot spot* and *hot phase* signatures are highlighted in red and blue respectively.

suggests that following a large-scale attack on average both small- and large-scale events are substantially more likely to occur within the next two days in distances of up to 50 km from an attack. Note that empirically such *hot phase* signatures not only arise if violence escalates following previous attacks but also if coordinated attacks target different locations in quick succession.

In 2006–2007 violence severely escalated and was increasingly driven by sectarian conflict. In this period, we observe only weak clustering of small-scale violence (<10% increase, Figure 6.6a), and in particular a relatively weak spatiotemporal correlation between small- and large-scale violence (Figure 6.6c and d). Interestingly, instances of small-scale violence only lead to a small but significant clustering of large-scale attacks in the same location but with delay of 2 weeks or more. The weak effect of large-scale attacks on subsequent small-scale attacks in the same location similarly increases with the time since the preceding incident. Compared to the previous period, large-scale attacks only have both a substantial *hot spot* and *hot phase* effect on other large-scale violence (Figure 6.6b). Note though that the strongest effect (up to 60% increase) only occurs directly after and in the direct vicinity of previous large-scale attacks. The fact that only the timing and location of large-scale strategic violence appear to be substantially correlated is consistent with the often less coordinate, civil war-like violence dynamics during the escalation



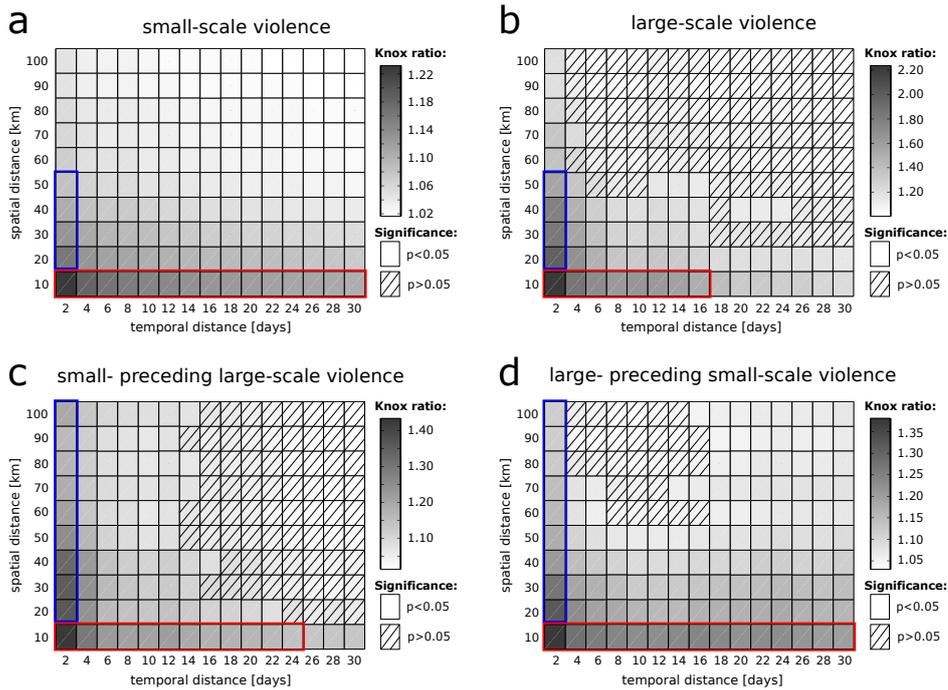
**Figure 6.6:** Knox test results for the period 2006–2007. Colors indicate the value of the Knox ratio  $K$ , corresponding standard errors are indicated by shading out non-significant estimates. Significance levels  $p$  indicate significant spatiotemporal correlation if  $K > 1$  and anti-correlation if  $K < 1$ . *Hot spot* and *hot phase* signatures are highlighted in red and blue respectively.

of sectarian conflict in this period.

The clustering dynamics in the period 2008–2009 again more closely resemble those of 2004–2005 albeit with a few notable differences (Figure 6.7). Generally, events cluster less than in the first period with the exception of large-scale violence, which clusters as strongly as in the first period (Figure 6.7b). Large-scale attacks also again have a substantial effect on the subsequent occurrence of small-scale violence in the direct vicinity of the attack and for locations up to 50 km away within the next 2 days (Figure 6.7d). The signatures of small- preceding large-scale violence suggest that other than in 2004–2005 small-scale violence not only increases the chance of large attacks in the same location but also noticeably for locations up to 100 km away (Figure 6.7c). This suggests that the effects of small-scale attacks on subsequent levels of violence bear closer resemblance to that of large-scale attacks—this is consistent with the increased visibility of smaller-scale attacks in the overall much less violent 2008–2009 period.

The spatiotemporal clustering analysis overall thus largely confirms our theoretical expectations: we observe a significant effect of small-scale attacks on subsequent small-scale violence (I), which generally generally cluster stronger in space than in time (VI). The effect of small-scale attacks on large-scale violence is typically relatively weak compared to the effect of large-scale attacks on large-scale violence (II); it is almost completely absent in the period 2006–2007. We also find a

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**Figure 6.7:** Knox test results for the period 2008–2009. Colors indicate the value of the Knox ration  $K$ , corresponding standard errors are indicated by shading out non-significant estimates. Significance levels  $p$  indicate significant spatiotemporal correlation if  $K > 1$  and anti-correlation if  $K < 1$ . *Hot spot* and *hot phase* signatures are highlighted in red and blue respectively.

strong effect of large-scale attacks on subsequent levels of violence (III), which is typically much more substantial for subsequent large-scale attacks (IV). All effects are also usually strongest in the direct spatial vicinity and directly following attacks (V). The only exception is the weak time-delayed coupling of small- and large-scale violence in 2006–2007.<sup>14</sup>

Overall, the patterns are generally quite consistent across periods suggesting that the general relationships we find are not critically depend on the particular conflict phase. The most noticeable difference concerns the coupling of large- and small-scale violence. While they are clearly coupled in the in 2004–2005 and 2008–2009, the location and timing of large-scale attacks seems to be almost completely decoupled from small-scale violence in 2006–2007 and with that from the general conflict situation. This could suggest that the insurgency against the central government proceeded relatively independent of the sectarian conflict dynamics but also simply that large-scale attacks with sectarian background were targeting the opposing group at times and locations not systematically related to the general conflict situation.

<sup>14</sup>We confirmed that these findings are robust to the exact classification of events into small- and large-scale violence. Please refer to Section D.4 of the supplementary information for details.

### 6.3.3 Disaggregating dynamics of small- and large-scale violence by province

The analysis so far has ignored any variations across different regions or provinces. In fact, given the narrative of the conflict one would expect noticeable differences between focal centers of violence such as Baghdad and provinces less affected by the conflict such as Al Basrah or Kirkuk. We here analyze those differences for the period 2008–2009, the most recent phase of the conflict covered by our data. Figure 6.4 illustrates the geographical distribution of small- and large-scale violence in this period.

In our comparison of clustering across provinces we focus only on the most substantial effects, i.e., *hot spot* and *hot phase* signatures. The results for the eight most violent provinces (see also Table 6.1) are shown in Figures 6.8 and 6.9; we also always included the estimates for all of Iraq discussed above for comparison. In all eight provinces small events show significant *hot spot* signatures (Figure 6.8a) albeit with very different intensity. Small events cluster most strongly in Babil, Salah ad-Din, Baghdad, Diyala and Al Anbar. In all cases clustering is strongest directly after preceding events. Note, however, that clustering strength in Baghdad falls off far more slowly than in any other province: even 30 days after a small-scale attack we observe substantially more small-scale violence in the direct vicinity of locations where small-scale attacks took place than we would expect if locations of violence were uncorrelated.

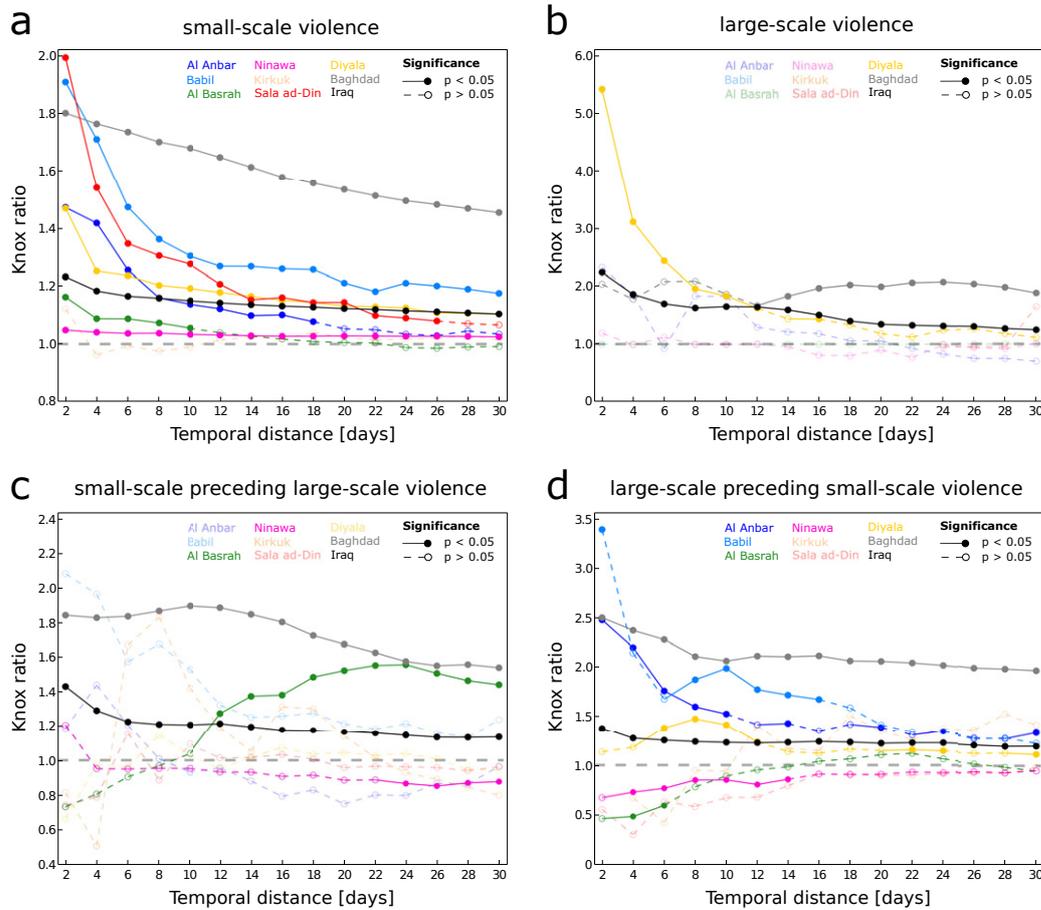
With the exception of Diyala the same provinces also exhibit the strongest *hot phase* signatures (Figure 6.9a), i.e., small-scale attacks also strongly cluster in time in distances up to about 50 km (or 12 km for Baghdad). Interestingly, in Ninawa—the province with the second most small-scale attacks in 2008–2009—small-scale violence does cluster in space and time but the effect is very weak. Note that both *hot spot* and *hot phase* effects we observed for all of Iraq therefore really only represent an average effect while the variation among individual provinces is relatively large.

Figure 6.8b and 6.9b suggest that the timing and location of large-scale attacks is only significantly correlated in Diyala and Baghdad.<sup>15</sup> In fact, Diyala—after Baghdad the province with the most large-scale violence in this period—visibly stands out in that large-scale violence very strongly clusters after previous attacks, both in the direct vicinity up to 2 weeks later and up to 30 km away within the next two days. In contrast, large-scale attacks in Baghdad only clusters significantly in the same location with a 2-week delay but within 2 days substantially increase the frequency of large-scale violence in a radius of 6 km from an attack. Note that these observations clearly show that the strong significant effects we observed for all of Iraq (Figure 6.7) arise as the average effect of the spatiotemporal clustering in Baghdad and Diyala—the two provinces mostly affected by large-scale violence in this period.

Disaggregating by province also reveals that the effect of small- on large-scale violence throughout Iraq is far from uniform. In Baghdad small-scale attacks have a substantial effect on subsequent large-scale violence (Figure 6.8c and 6.9c)—this is true both in the same location up to 30 days

<sup>15</sup>This is probably attributable to a lack of statistical power of the analysis in all other provinces with comparably fewer large-scale attacks (see Table 6.1).

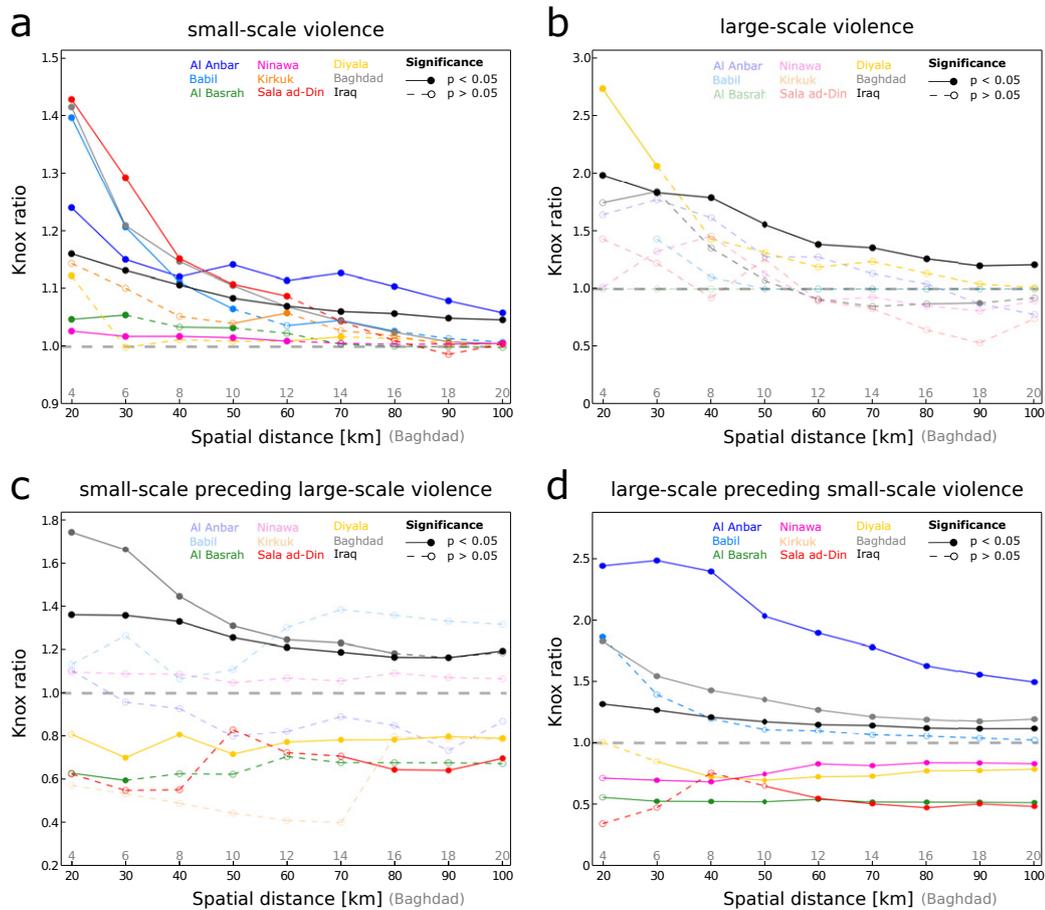
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**Figure 6.8:** *Hot spot* signatures across provinces for the period 2008–2009; provinces without any significant estimates are shown semi-transparent. Significance levels  $p$  indicate significant spatiotemporal correlation if  $K > 1$  and anti-correlation if  $K < 1$ .

later and for distances up to 16 km within then next two days. In Al Basrah we see a substantial *hot spot* effect but with a delay of about two weeks (Figure 6.8c). The positive effect of small-scale attacks on large-scale violence observed in 2008–2009 for all of Iraq is thus actually strictly limited to two urban centers where violence to some degree “naturally” clusters. In contrast, we even observed significantly less large-scale violence in the same location as previous small-scale attacks in Ninawa (Figure 6.8c). This anti-correlation of small- and large-scale violence also significantly affects the location of large-scale violence directly following small-scale attacks in Al-Basrah, Salah ad-Din and Diyala (Figure 6.9c).

The increase of small-scale violence following large-scale attacks we observe for Iraq is mainly driven by the dynamics in Al-Anbar, Baghdad and Babil (Figure 6.8d and 6.9d); in Diyala we only observe a time-delayed *hot spot* effect. These effects are, in fact, noticeably larger than the average effect observed for Iraq because in some provinces the effect of large- and small-scale violence is anti-correlated: there is significantly less small-scale violence following



**Figure 6.9:** *Hot phase* signatures across provinces for the period 2008–2009; provinces without any significant estimates are shown semi-transparent. Significance levels  $p$  indicate significant spatiotemporal correlation if  $K > 1$  and anti-correlation if  $K < 1$ .

large-scale attacks in Ninawa and Al Basrah, both in the same location and directly after an attack in distances up to 100 km away (Figure 6.8d and 6.9d). For Diyala and Salah ad-Din we also observe significantly less small-scale violence in locations up to 100 km directly following large-scale attacks.

The case of Diyala—the province with the third most casualties in this period—is particularly interesting: while the timing and location of each large-scale and small-scale attacks are significantly positively correlated in space and time, attacks of different scale are uncorrelated in space and mutually exclusive in time. This explicit anti-correlation suggests, in particular, that there is a strong strategic element to both the timing of small- and large-scale violence.

Overall, our spatially disaggregate analysis thus suggests that the average effects we observe for all of Iraq, in fact, in many cases arise from quite diverse relationships at the level of provinces. We can confirm across all provinces that small-scale attacks have a significant positive effect on subsequent levels of small-scale violence (I) and the effect is stronger in space than in time (VI).

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Note, however, that there is substantial variation with respect to the strength of the effect. The effect of small-scale attacks on large-scale violence is largely driven by the dynamics Baghdad. Contrary to our expectation (II) it is almost just as strong as the effect of large-scale attacks on large-scale violence. Note though that for most provinces there is no significant relationship or small- and large-scale violence are, in fact, anti-correlated.<sup>16</sup>

In provinces with substantial numbers of large-scale attacks, the disaggregate analysis confirms the strong effect of large-scale attacks on subsequent violence (III), however, it is only in Diyala systematically larger for subsequent large-scale attacks (IV). In Baghdad especially, the effect sizes are relatively similar. While we find that also at the level of provinces the effects are strongest in the direct spatial vicinity and directly following attacks (V), there are a few notable exceptions where significant and substantial effects are time-delayed—for example in Baghdad, Al Basrah, and Al Anbar.

We can generally not find significant correspondence between the character of each province—predominantly urban vs. rural, low-intensity conflict vs. high-intensity conflict, predominantly Sunni vs. Shia—and the violence dynamics we observe. For example, while small-scale violence is highly correlated in Baghdad, the most violent province, it is only weakly correlated in Ninawa, the second most violent province. On the other hand, Babil ranks among the less violent provinces but exhibits a strong clustering of small-scale violence. Note that large-scale violence only significantly clusters in the most violent provinces—this is, however, probably a consequence of a lack of statistical power in our estimates for the provinces with only few large events.

Among the four most violent provinces in 2008–2009, two are ethnically and religiously mixed (Baghdad, Diyala) and the other two are predominantly Sunni (Ninawa, Salah ad-Din). While the effect of large-scale attacks on large-scale violence is relatively consistent in Baghdad and Diyala, there is substantial variation with respect to the relationship of small-scale violence and the mutual interdependence of small- and large-scale violence. Overall, the four provinces exhibit significant substantial variation in the spatiotemporal clustering we observe. Note that the predominantly Shia province Al Basrah and Babil both rank among the least violent provinces—the relationship of small- and large-scale violence, however, is quite different in each province.<sup>17</sup>

The dynamics in Iraq's largest urban center, Baghdad, clearly dominates the aggregate violence patterns. Note, however, that in most other provinces—especially in Al Basrah, Salah ad-Din, Al Anbar, Diyala or Ninawa—much of the violence also clusters around population centers (Figure 6.4). Across these provinces and Baghdad we see large differences with respect to the relationship of small- and large-scale violence. Note, too, that in Diyala and Ninawa but also in Kerkuk and Al Anbar violence also clusters in more rural areas. Again, across these provinces the relationships we find are very diverse.

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<sup>16</sup>We confirmed that these findings are robust to the exact classification of events into small- and large-scale violence. Please refer to Section D.4 of the supplementary information for details.

<sup>17</sup>Northern Babil has a significant Sunni population and it is thus not strictly a majority Shia province.

### 6.4 Discussion and Conclusion

In this paper, we have theoretically and empirically demonstrated the need to disaggregate conflict event data by severity. In this we build on prior research on violent civil conflict that accounts for severity. Classification of events by type commonly used, however, typically only covers a fraction of empirical events. We therefore here introduce a statistical classification that “naturally” and robustly categorizes all events in our dataset into two broad categories—small- and large-scale violence—and thus enables us to disaggregate the effect of severity on the relationship between subsequent events across the whole data.

The classification reflects different theoretical expectations regarding the “nature” of small- compared to large-scale violence. While the former arises from a variety of different mechanisms, the latter largely reflects strategic, large-scale attacks. Explicitly testing hypotheses derived from these structural differences revealed important variations in how events of different severity impact subsequent violence dynamics but also serves as a qualitative validation of our classification and underlying theoretical argument.

While our spatially disaggregated analysis revealed significant differences across provinces, we showed that the dynamics of small- and large-scale violence we observe are not systematically driven by differences in their characteristics—Sunni vs Shia, urban vs rural or high intensity vs low intensity conflict. Analyzing data for three separate periods characterized to mixed degree by insurgent conflict and sectarian violence, we also confirmed that they hold across different conflict dynamics. Note that all analysis of spatiotemporal clustering in this study were performed using a custom *R* package, which will be released to the public.

From a theoretical and empirical point of view our study underscores the necessity to explicitly consider the scale of violence when analyzing dynamics of civil conflict. Consider for example our findings for the years 2004–2005. Given the much greater number of small-scale violence in that period—only 7% are large-scale attacks—we would, without disaggregating by scale, observe a signature similar to that of small-scale violence. This would, however, significantly understate the correlation between the timing and location of large-scale attacks, which account for over 43% of all casualties in this period.

We would also like to emphasize a number of implications that stand out from a policy point of view. The most robust pattern we find is that large-scale attacks on average tend to have a much larger systematic effect on the location and timing of subsequent violence than small-scale attacks. Given that these large-scale attacks overall amount to only about 4% of all events but over 30% of all casualties, this places a large strategic emphasis on stopping large-scale attacks. Our analysis can here help provide systematic guidance about *when* and *where* large-scale attacks are most likely to occur. In Diyala, for example, large-scale violence in 2008–2009 clustered extremely strongly within the next week in the same location and up to two days after attacks within a radius of 30 km. These kinds of patterns provide empirical leverage to efficiently identify “at-risk” regions following large-scale attacks and may thus help to effectively guide policy decisions.

## **Chapter 6. Severity matters: Analyzing the spatiotemporal relationship of small- and large-scale violence in Iraq**

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Our findings, however, also suggests that whatever inferences we can draw from past patterns of attacks heavily depend on individual provinces and are to some degree influenced by the specific regional dynamics of the conflict. We would also like to caution that a too limited emphasis on stopping organized, large-scale violence would likely not lead to the desired results. Our findings show that the location of small- and large-scale violence—especially in the insurgent conflict phases we analyzed—are significantly related. Only simultaneously improving the general security situation thus curbs the risk of small-scale violence translating into large-scale attacks. The fact that small-scale violence as such is strongly correlated in space and time puts further emphasis on the fact that—for a sustained reduction of violence—we also must promote measures that reduce general levels of violence.

In recent years detailed conflict event data on a variety of conflicts has increasingly become available. Our methodology of disaggregating events into broad severity categories is, in principle, applicable to any kind of conflict event dataset as long as casualty counts are reported as a measure of severity. It thus helps to broaden the empirical basis for quantitative studies of civil conflict, complementing the categorization by event type in cases where the reporting of the kind of incident itself is biased or incomplete.

## 7 Conclusion

This dissertation highlights the need for disaggregate analysis of conflict dynamics in order to reach a better, more nuanced understanding of civil conflict. The substantive studies place a particular emphasis on endogenous drivers of conflict, highlighting the fact that we must not only consider the conditions under which civil conflict emerges but also how prior events shape current conflict trajectories. Specifically they address three central questions: First, why does intergroup contact in some circumstances exacerbate but in others mitigate violence? Second, what is the role of civilians in conflict dynamics? Are they merely bystanders or actually help shape the conflict dynamics we observe? And third, how does the scale of violence affect subsequent conflict dynamics?

The studies not only confirm that contact, civilian agency and the scale of violent attacks all affect the trajectory of civil conflicts, but also clarify the *conditions* under which they deter or incite future violence and reveal the *strength* of these effects: contact tends to lead to violence if intergroup tensions are high; civilian collaboration with security forces increases when civilians themselves become targets of violence; and large-scale attacks more strongly incite subsequent violence than small-scale attacks.

These substantive findings have a number of concrete policy implications. In the context of Jerusalem, we investigate four realistic, counterfactual scenarios for the future of the city (Chapter 3). The analysis suggests that—given the current levels of intergroup tensions—arrangements conducive to reducing the extent of intergroup interactions may, in fact, dampen current levels of violence. The study, however, also demonstrates that similar improvements for the levels of violence can be achieved through comprehensive measures that improve group relations.

The finding that civilians in Iraq actively and strategically respond to insurgent attacks highlights the importance of civilian agency in civil conflicts (Chapter 4). Complementing empirical research that highlights the importance of population centric warfare in countering insurgencies, our study suggests that insurgents, in fact, operate under similar constraints. This has concrete implications for both insurgent and counterinsurgent tactics underscoring the critical importance of avoiding “collateral” civilian casualties.

## Chapter 7. Conclusion

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The study on the relationship of small- and large-scale violence in Iraq emphasizes the necessity to disaggregate conflict dynamics by event severity (Chapter 6). In fact, the most robust pattern that we found is that large-scale attacks on average had a much larger systematic effect on the location and timing of subsequent violence than small-scale attacks. While large-scale violence amounts to only about 4% of attacks, it accounts for over 30% of all casualties thus placing a particular strategic emphasis on stopping these attacks. Our analysis can help provide guidance about when and where they are most likely to occur. It also cautions, however, that any inferences we draw from past patterns of attacks are to some degree influenced by specific regional conflict dynamics.

Beside the theoretical focus on endogenous conflict processes, this dissertation has a second, explicitly methodological focus. It develops new or refines existing techniques for the analysis of disaggregate conflict data. It further draws attention to and systematically analyzes the effect of biases in these data (Chapter 5). These studies, in fact, demonstrate that this kind of methodological contributions are a critical prerequisite for inferences in the disaggregate settings we consider. We explicitly highlight potential pitfalls in the analysis of these data and present a number of conceptual and methodological approaches that improve upon prior work. This dissertation also aims to explicitly facilitate the dissemination of such methodology. The technique for causal inference in spatiotemporal event data developed together with Sebastian Schutte (Chapter 4), for example, has been released as an *R* package.

The dual focus of this dissertation on substantial questions and the development of suitable methods naturally arises from both the potential of disaggregate research on civil conflict and its possible limitations. The conceptual and empirical focus on smaller units of analysis allows to more explicitly study mechanisms of civil conflict and reach a better and more nuanced understanding of these dynamics. At the same time, studies at disaggregate levels of analysis must address specific methodological and conceptual issues that explicitly arise in the context of disaggregation.

This dualism, of course, extends beyond the context of the present work. Tackling questions at detailed levels of analysis, studies must pay particular attention to empirical model validation, clean causal inference and data quality. Only if studies follow “best practices” for the analysis of detailed, disaggregate conflict dynamics, we can be sure that substantial findings are unbiased and reliable. This is a critical prerequisite for formulating any concrete policy advice and therefore deserves our utmost attention.



# APPENDIX



# A Supporting Information (SI): “Group Segregation and Urban Violence”<sup>†</sup>

## A.1 Empirical Data

### A.1.1 Data Sources

The dataset used in this study covers acts of violence involving Secular/Moderate Orthodox Jews, Ultra-Orthodox Jews, Palestinians and (Israeli) security forces from January 2001 to December 2009 within the municipal boundaries of Jerusalem. It also includes data on several permanent checkpoints in the outskirts of the city, which are used to control population flows between the West Bank and the city. The data has daily resolution and events are geo-coded by statistical districts of Jerusalem. In addition to the geographical location and the type of event, the dataset contains detailed information on the identities of both perpetrators and victims.

Raw data was collected from various sources: The Israeli Police, particularly the Statistics and Mapping division operating within the police’s Planning and Organization Branch;<sup>1</sup> *B’Tselem*, the Israeli Information Center for Human Rights in the Occupied Territories, an organization whose activities include the documentation of assorted human rights violations, including the restriction of movement, expropriation of land, discrimination in planning and building, administrative detention, and fatalities;<sup>2</sup> OCHA oPT, the UN Office for the Coordination of Humanitarian Affairs, an office established to monitor the humanitarian situation in the Occupied Territories (East Jerusalem notwithstanding), to enhance inter-agency coordination, and affect policy making through the collection and dissemination of information and facts;<sup>3</sup> *AIC*, Alternative Information Center, an Israeli-Palestinian organization devoted, among other things, to the collection, analysis,

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<sup>†</sup>This chapter is an edited version of the supporting information for the following article: Ravi Bhavnani, Karsten Donnay, Dan Miodownik, Maayan Mor and Dirk Helbing. (2014). “Group Segregation and Urban Violence.” *American Journal of Political Science* 58(1): 226–245. It can be downloaded at <http://dx.doi.org/10.1111/ajps.12045>.

<sup>1</sup>Israel Police, Planning and Organization Division: [http://www.police.gov.il/mehozot/agafTichnon/Pages/mipoy\\_geografi.aspx](http://www.police.gov.il/mehozot/agafTichnon/Pages/mipoy_geografi.aspx).

<sup>2</sup>B’Tselem, the Israeli Information Center for Human Rights in the Occupied Territories, Statistics: <http://www.btselem.org/statistics>.

<sup>3</sup>OCHA oPT, United Nations Office for the Coordination of Humanitarian Affairs, online databases: <http://www.ochaopt.org/onlinedatabases.aspx>.

## Appendix A. Supporting Information (SI): “Group Segregation and Urban Violence”

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and dissemination of information pertaining to human rights violations in the Israeli-Palestinian context;<sup>4</sup> and lastly data was collected through a thorough content analysis of all the daily issues of *Yediot Aharonot*, Israel’s highest circulation newspaper.<sup>5</sup>

### A.1.2 Data Reliability and Availability

These data sources were used with several goals in mind: (1) to assemble a wide universe of events of deadly and non-deadly violence in Jerusalem; (2) to cross-check and validate the coding of events across various sources; and (3) to compensate for biases in the data that may have been introduced by relying on only one or a limited set of sources.<sup>6</sup> In the context of Jerusalem, Palestinians for example are less likely to use the police to file complaints on violence but more likely to express grievances in front of a representative of a human rights organization. Using multiple sources we cross-validate and account for potential biases wherever possible—nonetheless we are conscious that there may still be systematic biases remaining in the data.

Police records available for the entire period of research include events with information on the perpetrators and victims, and particular information on the type of violent act committed; *B’Tselem*’s data includes high quality reports on deadly violence occurring in the city of Jerusalem for the entire period; OCHA’s data includes weekly reports that cover information on deadly violence (mostly obtained from *B’Tselem*), and on non-deadly violence for the period between October 2003 to November 2009; *Yediot Aharonot* provided information on murders, attempted murders, minor assaults and riots for the entire period; and, finally, the *AIC* holds some information on minor assaults and mobilization events between Jews and Palestinians.

### A.1.3 Coding Violence

Data were coded into three types of events: murders, which includes cases of deadly stabbing, gunfire, and suicide bombing; attempted murders, including events with injuries incurred as a result of stabbing, gunfire, or suicide bombings; and minor assaults, or cases that involved beating, and either stone or Molotov cocktail throwing. Overall the dataset includes 286 cases of minor assaults, 173 events of attempted murders (116 of the reported attempted murders and violent assault events occurred during riots or collective clashes), and 85 deadly events incurring 253 casualties. Descriptions of typical events in our data include:

- On January 26, 2008, a Palestinian working in the *Atarot* industrial zone in the northern part of the city stabbed a Jewish fellow worker and then was fired at and killed.

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<sup>4</sup>*AIC*, Alternative Information Center, <http://www.alternativenews.org/english/>.

<sup>5</sup>Access to the *Yediot Aharonot* archive was provided by the Jewish National & University Library in Jerusalem <http://jnul.huji.ac.il/eng/>.

<sup>6</sup>Implicit selection bias in the collection of empirical data is a known issue (see, for example, Davenport & Ball, 2002).

- On October 26, 2009, a Palestinian woman stabbed an Israeli security guard at *Qalandiya* checkpoint, injuring him. A few weeks later on November 19, 2009, an Israeli settler stabbed and moderately injured a Palestinian man while he was standing at a bus station.
- On February 12, 2007, Palestinians stoned Israeli policemen who responded with tear gas canisters and rubber-coated metal bullets.

For purposes of comparison, we treat events from all three categories as “incidents of violence.”

### A.1.4 Population and Settlement Data

The geography and the initial population setup of the simulations are based on data from Israel’s Central Bureau of Statistics<sup>7</sup> and include: (1) detailed information on the geography of all of Jerusalem’s neighborhoods, including the locations of residential areas; and (2) population statistics and information on natural population growth (births, deaths, immigration) on a neighborhood basis for the years 2001–2009 (note that for the Ultra-Orthodox population only estimates for the year 2005 are available—the other years are extrapolated from those estimates). The geography and the initial population setup of the model are based on polygons that were made available through Israel’s Central Bureau of Statistics, the Jerusalem Institute for Israel Studies<sup>8</sup> and the HUGIS (The Hebrew University GIS Center),<sup>9</sup> and Shaul Arieli, a retired colonel, publicist and member of the Geneva Initiative, who has been collecting geo-spatial data on proposed peace initiatives and settlements.<sup>10</sup>

## A.2 The Agent-Based Model

The computational model is implemented in JAVA using the GIS functionality of the REPAST Symphony multi-agent simulation toolbox.<sup>11</sup> The model dynamics, the statistical analysis of the simulation results, and output and input functionality are implemented in custom JAVA code. All simulation results are fully reproducible knowing the exact parameter configuration of the respective scenario and the random seed used. Every simulation run returns detailed statistics on the simulated events (location, time, size, assailant/victim identity) and the population distribution in the simulated neighborhoods. Note that each simulated violent event is marked by an event ID—incidents that occur during the same simulation time step and involve individuals from the same pairing of perpetrator and victim groups are treated as part of the same event.<sup>12</sup>

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<sup>7</sup>The Central Bureau of Statistics (Israel): <http://www.cbs.gov.il/>.

<sup>8</sup>The Jerusalem Institute for Israel Studies, Data & Statistics: <http://www.jiis.org/?cmd=datast>.

<sup>9</sup>The Hebrew University GIS Center: <http://hugis.huji.ac.il/>.

<sup>10</sup>Col (Ret.) Shaul Arieli: <http://www.shaularieli.com/>.

<sup>11</sup>Repast Organization for Architecture and Design, “Repast,” available at <http://repast.sourceforge.net/> (Version 1.2.0, 2008).

<sup>12</sup>In a typical simulation run most episodes of violence correspond to single incidents while episodes consisting of several related incidents account for only ~ 20% of the simulated events.

**Appendix A. Supporting Information (SI): “Group Segregation and Urban Violence”**

| <i>ID</i> | <i>Name</i>                         | <i>ID</i> | <i>Name</i>                 | <i>ID</i> | <i>Name</i>             | <i>ID</i> | <i>Name</i>                   |
|-----------|-------------------------------------|-----------|-----------------------------|-----------|-------------------------|-----------|-------------------------------|
| 1         | Atarot, Kafr 'Aqab, Industrial Zone | 21        | Givat Shaul industrial zone | 41        | Armenian Quarter        | 61        | Ramat Sharett and Ramat Denya |
| 2         | Beit Hanina                         | 22        | Givat Shaul                 | 42        | Talbiya                 | 62        | Teddy Stadium, Zoo            |
| 3         | Neve Yaaqov                         | 23        | Har Nof                     | 43        | Rehavia                 | 63        | Malha                         |
| 4         | Pisgat Ze'ev                        | 24        | Kiryat Moshe                | 44        | German Colony           | 64        | Sharafat                      |
| 5         | Shuafat                             | 25        | Beit Hakerem                | 45        | Katamonim               | 65        | Gilo                          |
| 6         | Ramot Haredi                        | 26        | Givat Ram                   | 46        | Katamon                 | 66        | Beit Safafa                   |
| 7         | Ramot Alon                          | 27        | Keryat Hale'om              | 47        | Givat Mordechai         | 67        | Givat Hamatos                 |
| 8         | Ramat Shlomo                        | 28        | Nachlaot                    | 48        | Nayot                   | 68        | Har Homa                      |
| 9         | Motza                               | 29        | Mekor Baruch                | 49        | Bayit VeGan             | 69        | Sur Baher, Um Tuba            |
| 10        | Har Hotzvim                         | 30        | Mea Sharim                  | 50        | Yefeh Nof               | 70        | Um Lisun                      |
| 11        | Sanhedria                           | 31        | City Center                 | 51        | Herzl Mount             | 71        | Talpiot                       |
| 12        | Ramat Eshkol                        | 32        | Musrara                     | 52        | Ein Karem               | 72        | East Talpiot                  |
| 13        | French's Hill                       | 33        | Bab A zahara                | 53        | Ein Karem Hospital      | 73        | Talpiot Industrial Zone       |
| 14        | Mount Scopus                        | 34        | Sheikh Jarrah               | 54        | Rama Hadassah (unbuilt) | 74        | Baka                          |
| 15        | Issawiya                            | 35        | Wadi Joz                    | 55        | Ein Lavan               | 75        | Abu Tor                       |
| 16        | Ma'alot Dafna                       | 36        | A-Tur                       | 56        | Kiryat Hajovel          | 76        | Jabel Mukaber                 |
| 17        | Shmuel Hanavi                       | 37        | Silwan                      | 57        | Kiryat Mennahem         | 77        | Ras al-amid                   |
| 18        | Geula                               | 38        | Muslim Quarter              | 58        | Givat Massuah           | 5555      | (unbuilt)                     |
| 19        | Romema                              | 39        | Christian Quarter           | 59        | Lavan Ridge             | 7777      | (unbuilt)                     |
| 20        | Lifta                               | 40        | Jewish Quarter              | 60        | Shalmon Mount           |           |                               |

**Table A.1: Neighborhoods of Jerusalem**

### A.2.1 Model Dynamics

To define time progression from repeated pair-wise interactions, we assume a time step to be the period after which 10 percent of the agent population has been updated. We compensate for the arbitrariness in this definition by exclusively considering time-aggregated simulation results for our analysis and rescaling the total number of simulated events to the total number of empirical events. Effectively, this amounts to comparing relative frequencies of simulated events to the corresponding empirical data. In order to improve computational performance, the agent population in the simulation was scaled down from the empirical population size,<sup>13</sup> as long as the relative population sizes and characteristics of the population distributions are maintained, the rescaling can be absorbed in the time step definition without altering the simulation outcomes. The (empirical) population growth rates for each neighborhood are explicitly time-dependent and have been rescaled to reflect the difference in time progression between model and empirical time. In the simulations reported here the time scaling is such that 30 simulation steps correspond to one year; this represents model dynamics that lead to sufficiently large (representative) simulated event samples.

In every time step, an agent migrates and interacts—the order of migration and interaction does not have a systematic influence on the model dynamics.<sup>14</sup> Agents relocate with probability  $m_G$  (the group specific mobility) if the local level of violence  $\nu_R$  exceeds the average level of violence in neighborhood  $N$ . If they relocate, they only migrate to neighborhoods where they are not in the minority; if they cannot find such a location, they leave the city.

The logistic function used in the definition of the event probability has a finite value on both sides of the transition point where social distance equals the threshold. We believe such a smooth, graduated transition from non-violence to violence represents a more plausible escalation dynamic than a step function with a sharp transition from non-violence to violence at a certain point. When the value of  $\lambda$  is small (large), the curve is steep (shallow) and  $p_{i,j}(t)$  goes to zero and 1 on the respective sides of the transition point at a faster (slower) pace. As part of the formal model estimation, we evaluate the dependence of the model results on the value of  $\lambda$ .

Interaction dynamics in the simulation model are residence-based, i.e., only members of the three population groups (Secular/Moderate Orthodox Jews, Ultra-Orthodox Jews, Palestinians) may perpetrate violence;<sup>15</sup> comparing the model to empirical data we therefore exclude violent events perpetrated by security forces. In the 2001–2004 (2005–2009) period, security forces can be held accountable for 17 (67) violent incidents, yet the overall pattern of violence is very similar with and without those events (Figure A.1), mainly because they are concentrated in quarters of the city with the highest levels of violence. Note that security force violence may both occur in response to violence but also incite further violence. We do account for this potentially adverse effect of

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<sup>13</sup>In the simulation runs reported here, the empirical population was scaled down by a factor of 100.

<sup>14</sup>Changes in simulated model dynamics arising from inverting this order are negligible since they are no larger than those arising from a different random simulation seed.

<sup>15</sup>Security forces may nonetheless be the targets of violence.

## Appendix A. Supporting Information (SI): “Group Segregation and Urban Violence”

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policing and police violence in our modeling framework (see also Section A.4): high levels of policing correspond to more contact with security forces thus increasing the risks of violence when tension is high. In line with the empirical observations but without explicitly matching incidents perpetrated by security forces, our model then exhibits high levels of (potentially violent) policing in violent city quarters—both as a result of and a reason for high levels of violence.

### A.2.2 Model Geography

We seed the geography of Jerusalem and its neighborhoods using shape files such that agents interact on a virtual landscape that mirrors the actual physical geography of the city. In order to reduce the computational complexity of the simulations, agent locations are defined on an underlying regular grid that is dynamically generated using actual settlement locations and their associated densities. This corresponds to a (fine-grained) discretization of geographical space, i.e., the model rests on a finite number of geographical sites or settlement locations.

The grid size of 100 m used in this study is roughly equivalent to the size of settlement or housing blocks. In order to account for both low and high neighborhood population densities, we use a grid of medium granularity to specify agent locations on the model topology and “stack” agents, i.e., locate more than one agent at a given grid point, to capture denser population distributions. The immediate surrounding  $R$  of an agent is operationalized in terms of a Moore neighborhood, a concept drawn from the theory of cellular automata: it consists of all agents located on positions on the regular grid within range  $r$  from the agent. The stacking of agents ensures that within the same range  $r$  an agent has more direct neighbors in a densely compared to a more sparsely populated area. For every neighborhood the largest (empirical) population in the time period considered defines the maximal number of possible agent locations.<sup>16</sup>

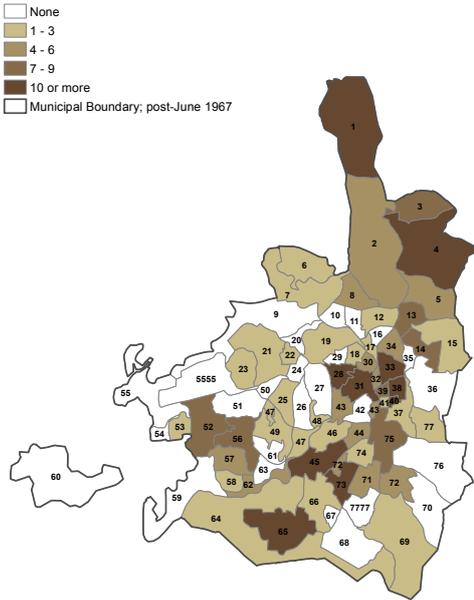
As noted in the previous section, only agents who belong to the civilian groups are explicitly represented on the model landscape while state authorities are assigned to each neighborhood  $N$  in numbers proportional to the level of policing  $s_N$ . Assigning fixed positions to security forces is not realistic: they are typically deployed to a neighborhood and within a short time span may reach any point in the locality. The interaction partners for any agent  $i$  are then randomly drawn from: (1) all civilian agents within local surroundings  $R$ , and (2) the security forces. If violence against a civilian agent  $j$  ensues, the violence memory of all affected neighbors in the victim’s immediate surroundings increases; for violence against security forces all neighbors in the perpetrator’s surroundings are affected. An agent’s individual violence memory ranges from 0 (no memory of violence) to 1 (very high exposure to violence); for every experience of violence it increases to 1 and then decays exponentially on a characteristic time scale  $t$ . We further assume that memory is not private information but shared by neighbors subjected to violence. The number of security forces interaction partners is calculated as the square-root of

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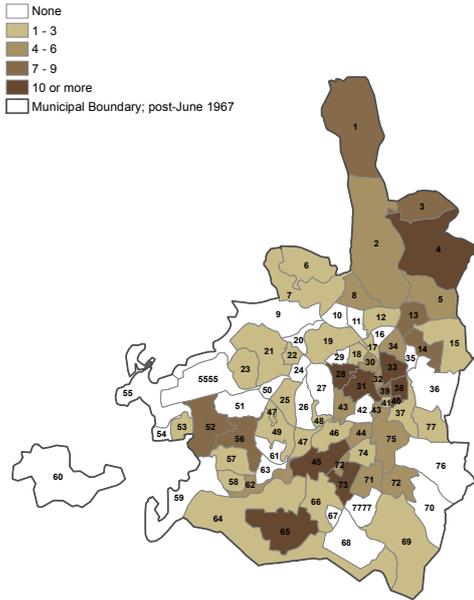
<sup>16</sup>In the case of new housing developments (only relevant for the counterfactual analysis), the projected housing capacities are also considered for the housing capacity.

A.2. The Agent-Based Model

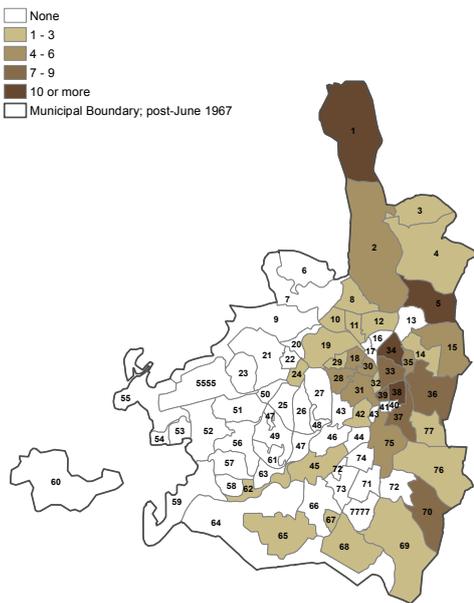
(a) Full data, 2001-04



(b) Data excluding security force violence, 2001-04



(c) Full data, 2005-09



(d) Data excluding security force violence, 2005-09

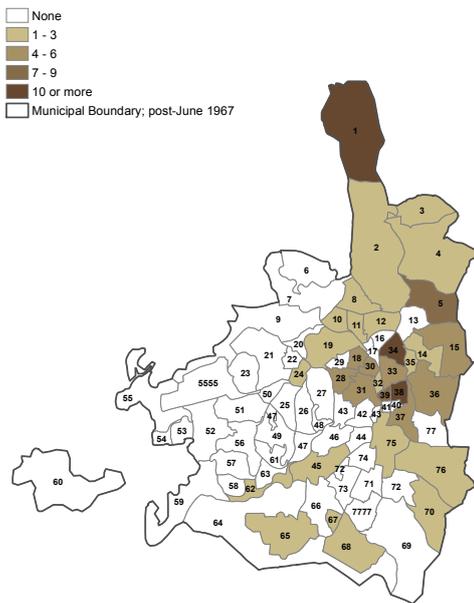


Figure A.1: Empirical Number of Violent Events by Neighborhood

## Appendix A. Supporting Information (SI): “Group Segregation and Urban Violence”

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the number of grid locations in  $R$  multiplied by  $s_N$ , thus the number of police interaction partners scales with the interaction range.<sup>17</sup>

### A.2.3 Empirical Parameters

The values for the mobility parameters  $m_G$  are developed based on results from the Israel Social Survey (2002–2007) conducted by the Central Bureau of Statistics, according to which about 10 percent of the Ultra-Orthodox Jewish population, 20 percent of the Secular Jewish population and 30 percent of the Palestinian population are not satisfied with their current residential location. We take these figures as a measure of the motivation to migrate, then factor in that at any given point only a fraction of the population—assumed to be 10 percent—considers or is capable of moving, before translating the values to a unit interval scale. The agent population, the corresponding natural population growth, and the housing capacity of each neighborhood are given as empirical inputs for both the 2001–2004 and 2005–2009 period; in the counterfactual analysis they are based on the specific provisions of each scenario.

## A.3 Model Estimation

The simple heuristic methodology we employ enumerates the model’s parameters and identifies parameter combinations for which the model best approximates the empirical data along the specified dimensions of agreement. First, the full parameter space is covered in a coarse-grained sweep; the parameter region of interest where the model exhibits the best agreement in all dimensions is then subjected to a fine-grained analysis. In the first sweep, the minimal conditions for qualifying a parameter vector as having “good agreement” are set for the 2001–2004 (2005–2009) period as 0.75 (0.8) location of violence match, 0.3 (0.5) Pearson’s correlation for the number of violent events per neighborhood, and 0.9 (0.9) Pearson’s correlation for the attack targets by group. In the fine-grained parameter sweep, the latter condition is increased to a 0.95 Pearson’s correlation allowing only for very good (city-wide) representations of the violence dynamics. Note that the coarse-grained sweep already reliably identifies the parameter ranges leading to good agreement with data. The subsequent fine-grained analysis simply yields even better quantitative agreement—it more precisely identifies the parameter ranges of parameter combinations with the best agreement to data (see also Figure 3.5 and A.4). In order to define a reference scenario for each period, we then identify within this subset of “best agreement” the parameter combination for which the simulation model most reliably exhibits the maximal agreement (see Table A.2 for an overview).<sup>18</sup>

In order to guarantee that our specific choice of  $\lambda$ ,  $r$  and  $t$ —the scale of the logistic threshold function, the size of the local surroundings  $R$ , and the time scale for memory decay—does not

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<sup>17</sup>The probability of civilian violence directed at security forces is then calculated exactly as for interactions with (other) civilian groups since both are driven by local conflict drivers and contact in a given location.

<sup>18</sup>The run that “most reliably” agrees with the data is the one with the highest average quantitative agreement in all three dimensions of agreement for 100 simulation runs.

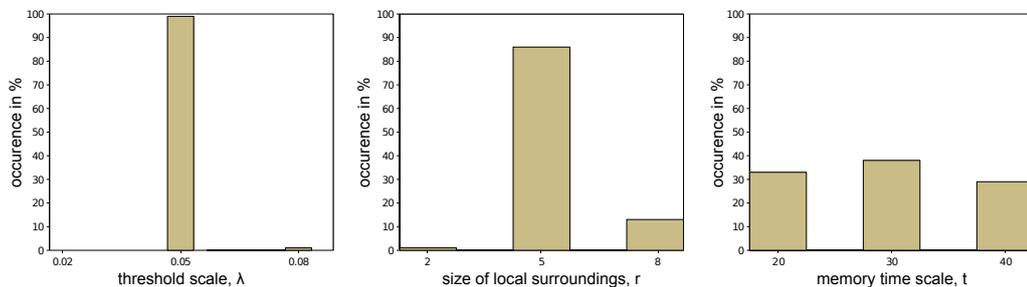
### A.3. Model Estimation

| period    | social distance |        |        |        |        |        |        |        |        | discrimination |      |     | agreement to data |       |       |
|-----------|-----------------|--------|--------|--------|--------|--------|--------|--------|--------|----------------|------|-----|-------------------|-------|-------|
|           | S to U          | S to P | S to F | U to S | U to P | U to F | P to S | P to U | P to F | S              | U    | P   | L                 | D     | T     |
| 2001-2004 | 0.1             | 0.5    | 0      | 0      | 0.3    | 0.2    | 0.6    | 0.3    | 0.3    | 0.1            | 0.1  | 0.6 | 0.77              | 0.33* | 0.99* |
| 2005-2009 | 0               | 0.4    | 0      | 0.15   | 0.3    | 0.4    | 0.25   | 0.2    | 0.4    | 0              | 0.35 | 0.4 | 0.83              | 0.65* | 0.97* |

L: location of violence; D: number of violent events per neighborhood; T: attack targets by group \* $p < 0.005$  (Pearson's correlation)  
 S: Secular/Moderate Orthodox Jews, U: Ultra-Orthodox Jews, P: Palestinians, F: Security Forces

**Table A.2: Reference Scenarios**

impact the estimation of the social distance and discrimination parameters, we simultaneously also vary these parameters in three discrete steps each ( $\lambda \in \{0.02, 0.05, 0.08\}$ ;  $r \in \{2, 5, 8\}$ ;  $t \in \{20, 30, 40\}$ ). Figure A.2 shows the occurrence of these parameter values in the subset of “best agreement.” The majority of parameter combinations with excellent agreement to data assumes  $\lambda = 0.05$ ,  $r = 5$  and  $t = 30$ , though the model’s agreement to data is more strongly dependent on the choice of  $\lambda$  and  $r$  than on  $t$ . Note that in contrast to social distance and discrimination these parameters do not have clear empirical referents, however, we may nonetheless check their face validity. For  $r = 5$  the local surroundings in the model have a radius of 500 m, this is larger than the immediate neighborhood but smaller than the size of an average residential quarter—in this sense it captures well the geographical unit at which local contact takes place. The violence memory time scale of  $t = 30$  simulation steps corresponds in our scaling to one year; in other words, after one year, given no further exposure to violence, the memory of a violent incident will have mainly faded. While for major incidents this may appear too short it nonetheless adequately captures the notion that the memory of exposure to violence lingers for a considerable time. The threshold scale value of  $\lambda = 0.05$  simply implies that the transition from non-violent to violent behavior is not too abrupt (for  $\lambda = 0.02$  the probability,  $p$ , takes a form much closer to a step function); at the same time the specification ensures that it is very unlikely that for low social distances violence ensues (this is much more likely at  $\lambda = 0.08$  or larger; see also Figure 3.1 in the manuscript). In that sense, the value of  $\lambda$  we find in the estimation yields plausible threshold dynamics.



**Figure A.2: Interaction Parameters**

In the model estimation procedure the number of scenarios to be simulated increases with the

## **Appendix A. Supporting Information (SI): “Group Segregation and Urban Violence”**

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number of parameters varied; it also grows as the step size of the parameter variations decrease. Therefore, even for a model with a small number of parameters scanning the whole parameter space at a reasonable resolution is computationally very intensive. The multi-step procedure we employ here helps to mitigate this issue: a coarser initial parameter resolution allows enumerating the full parameter space at an acceptable computational cost, whereas the subsequent fine-grained analysis of the parameter subset that leads to good agreement with data guarantees that the parameter values leading to the best agreement with data are precisely identified.

The model enumeration procedure has a few limitations: (1) it only reliably detects regions of good agreement with data that are larger than the resolution of the first coarse-grained parameter sweep; however, making the steps in the coarse-grained analysis sufficiently small mitigates this problem; (2) using a finite step size for the parameter variations the procedure implicitly requires that there are no extreme changes in the fit measures for small parameter variations—this limitation is inherent to procedures using finite step sizes; (3) among the parameter regions with good agreement to empirical data, only the largest region is reliably identified. We are confident, however, that these limitations do not affect the optimization results in the present case: the coarse-grained enumeration is using relatively small step sizes and an analysis of the fit measures as a function of the model parameters in this coarse-grained parameter sweep indicates that only one general parameter region leads to a good fit along all three fit dimensions; there also do not appear to be sudden changes in the fit measures for small parameter variations.

### **A.3.1 Measures for Quantitative Agreement**

The quantitative model optimization requires clear criteria for identifying if a simulated scenario is consistent with the empirical observations or not and to what degree. We compare the simulation results to the empirical data along three dimensions:

- a neighborhood is violent or non-violent (location of violence)
- the exact number of violent events in each neighborhood
- the attack targets by group, i.e., which population group is responsible for which fraction of attacks on which other population group(s)

It is possible to have good agreement in the location of violence while at the same time the quantitative agreement in the number of events per neighborhood is quite poor and vice versa. The attack targets by group are considered on the city-level, guaranteeing that the violence dynamics are (globally) representative of the empirical violence dynamics.

The comparison between simulated and empirical data in each of the three dimensions is formalized using standard measures: the agreement for the location of violence may be compactly expressed as the percentage of neighborhoods for which “violence” or “no violence” is correctly

predicted in the simulated data. In order to test if two patterns of violent/non-violent neighborhoods are significantly different we use a non-parametric McNemar test (McNemar, 1947). The number of violent events per neighborhood statistics may be cast in the form of a data series where each entry corresponds to the number of attacks in a specific neighborhood. The degree of agreement between the empirical and simulated series may then be quantified using different measures: we use standard Pearson's correlation and several root mean square deviation (RMSD) measures. Used for example in bioinformatics, RMSD measures are well suited to quantify how precisely a predicted data series corresponds to an empirical reference series. The common definition of the root mean square deviation is:

$$\text{RMSD} = \sqrt{\frac{1}{n} \sum_{i=1}^n (x_i - x_j)^2} \quad (\text{A.1})$$

where  $\{x_i\}$  and  $\{y_i\}$  for  $i = 1 \dots n$  are two data series representing the empirical and predicted distributions of violence respectively;  $n$  is the number of neighborhoods. The measure returns the number of attacks by which the simulation and the empirical data are (on average) not agreeing. It may also be normalized to the average number of attacks per neighborhood and is then referred to as the coefficient of variation of the root mean square deviation (CVRMSD); normalizing with the maximal range of values in the data series yields the normalized root mean square deviation (NRMSD). Note that the measures are generally quite consistent in estimating the per neighborhood agreement; in the optimization procedure we relied on Pearson's correlation as the quantitative criterion.

The attack targets by group statistics may also be formalized as a data series, the entries corresponding here to the nine interaction pairings between actor groups that may lead to violence: Secular/Moderate Orthodox Jews attacking Palestinians, Ultra-Orthodox Jews or security forces; Palestinians attacking Secular/Moderate Orthodox Jews, Ultra-Orthodox Jews or security forces; and Ultra-Orthodox Jews attacking Secular/Moderate Orthodox Jews, Palestinians or security forces. The quantitative agreement between the simulated and empirical distribution of violence targets may then again be estimated using a simple Pearson's correlation, analogous to the measure for the per neighborhood agreement in the distribution of violence.

## A.4 Validation

The validation procedures detailed below indicate that the simulation model has a high degree of internal validity and can thus serve as a reliable basis for the counterfactual analysis conducted. To this end we first analyze the model's predictive power, in-sample and compared to a base line model; the analysis focuses on the 2005–2009 period since the counterfactual analysis is based on the dynamics of this period. We then verify that parameter values for social distance and discrimination obtained through formal optimization are consistent and reflect observed levels of intergroup tension and discrimination in Jerusalem—a strong indication of the internal validity of the individual (micro) level model mechanisms.

### A.4.1 In-Sample Validation

The in-sample validation performed here replaces an out-of-sample prediction test: we train the model on geographically and temporally sliced subsets in the 2005–2009 period and then test its predictive power on the remaining subsets. Splitting the data geographically, the neighborhoods constituting the “training set” are selected by randomly drawing half of the non-violent and half of the violent neighborhoods; the “test set” covers the remaining neighborhoods.<sup>19</sup> The corresponding training and test datasets then cover all incidents in the 2005–2009 period for their respective subset of neighborhoods (we generated training and test datasets for 20 different random geographical slices). Slicing temporally, the following five splits of the data set (training vs. test dataset) were analyzed: (1) 2005–2006 vs. 2007–2009; (2) 2005–2007 vs. 2008–2009; (3) 2005–2008 vs. 2009; (4) 2005–2006, 2009 vs. 2006–2007; (5) 2005, 2009 vs. 2006–2008. In the first validation step, the simulation model is optimized for the training set with the same procedure used to obtain the reference scenarios.<sup>20</sup> Testing its predictive power, the optimized model is run on the test dataset. This step is repeated 100 times with different random simulation seeds to obtain confidence intervals for the quantitative agreement.

The results of the in-sample predictions are summarized in Figure A.3. In the case of spatial slicing (Figure A.3a), the model optimized for the training set predicts violence in the test dataset with on average 0.75 location match, 0.45 correlation for the number of violent events per neighborhood and 0.75 for the attack targets by group. This quantitative agreement is in the range of the optimized scenarios for the training sets. Note, however, that a number of splits deviate substantially for one of the measures—this can be attributed to a substantial difference between training and test datasets: if the two sets are too different it is not possible that the parameter vector optimized for the training set matches the test set with high precision. This is most noticeable for the attack targets by group for which a few of the splits (split 4, 6, 9 and 13) show comparably lower agreement. For the temporal slices of the dataset (Figure A.3b) the model optimized for the training datasets predicts violence in the test datasets on average with 0.72 location match, 0.36 correlation for the number of violent events per neighborhood and 0.7 for attack targets by group. The degree of agreement for the attack targets by group again varies, which may be attributed to the fact that training and test set when splitting temporally may in fact be quite different; in particular, the distribution of attack targets by group changes substantially over time. Overall, however, the generally strong quantitative agreement with the test datasets is a strong indication that the simulation model has substantial in-sample predictive power. Note further that optimized parameter vectors for both spatial and temporal subsets are very similar to the parameter vector of the reference scenario; this points to a high consistency of the model mechanisms in predicting the empirical violence patterns (see also Section A.4.3).

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<sup>19</sup>Independently drawing from the violent and non-violent neighborhoods ensures that the training and test set have similar numbers of violent and non-violent neighborhoods.

<sup>20</sup>For the spatial slices, the matching in both the training and in the testing stage is done only for the neighborhoods that are part of the respective subset; the spatially sliced datasets by definition do not contain data on the neighborhoods outside of the sample.

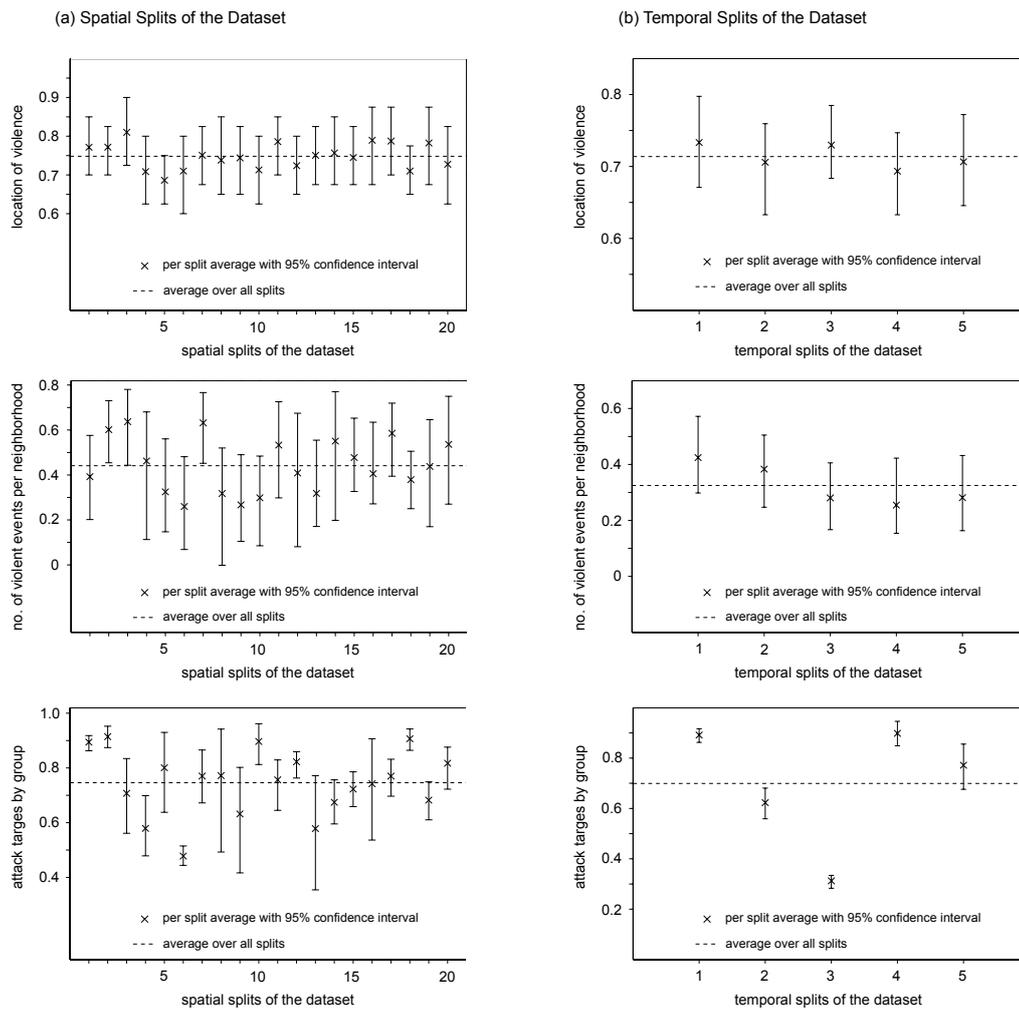


Figure A.3: In-Sample Validation

### A.4.2 Comparison to Baseline Model

We further verified that the simulation model has added explanatory value compared to a simple statistical baseline model. Our data indicates a strong regularity in the location and intensity of violence in the 2005–2009 period; consequently, we expect past violence to be an excellent predictor for future violence within that period. We construct the model’s prediction for the number of violent events in a neighborhood  $N$  by assuming that  $\text{violence}_N(\text{year}) = \text{violence}_N(\text{year} - 1)$ . The predictive power of our simulation relative to the statistical model is estimated by regressing the empirical number of violent events per neighborhood against the predictions of the two models. We find that the predictions of both models are significant (Table A.3)—in the combined model, the simulated results significantly increase the explanatory value compared to the baseline model.<sup>21</sup> Note that the statistical baseline model is intentionally kept simple.

<sup>21</sup>F-test:  $F = 34.93$ ,  $p < 0.001$ .

## Appendix A. Supporting Information (SI): “Group Segregation and Urban Violence”

| Variables  | <i>Past Violence Model</i> | <i>Simulation Model</i> | <i>Past Violence &amp; Simulation Model</i> |
|--|----------------------------|-------------------------|---|
|  | Coefficient                | Coefficient             | Coefficient                                 |
| Events per neighborhood constructed from past violence | 1.084***<br>(0.117)        | -                       | 0.795***<br>(0.109)                         |
| Simulated events per neighborhood                      | -                          | 0.924***<br>(0.116)     | 0.595***<br>(0.101)                         |
| Constant   | 0.948**<br>(0.230)         | 0.112<br>(0.354)        | 0.099<br>(0.292)                            |
| Number of Observations                                 | 79                         | 79                      | 79  |
| R <sup>2</sup>   | 0.526                      | 0.443                   | 0.667                                       |

Note: Linear regressions, dependent variable: empirical number of violent events per neighborhood for the 2005-2009 period. Standard errors are given in parantheses. Two-tailed significance test: \*p<0.05 \*\*p<0.01 \*\*\*p<0.001

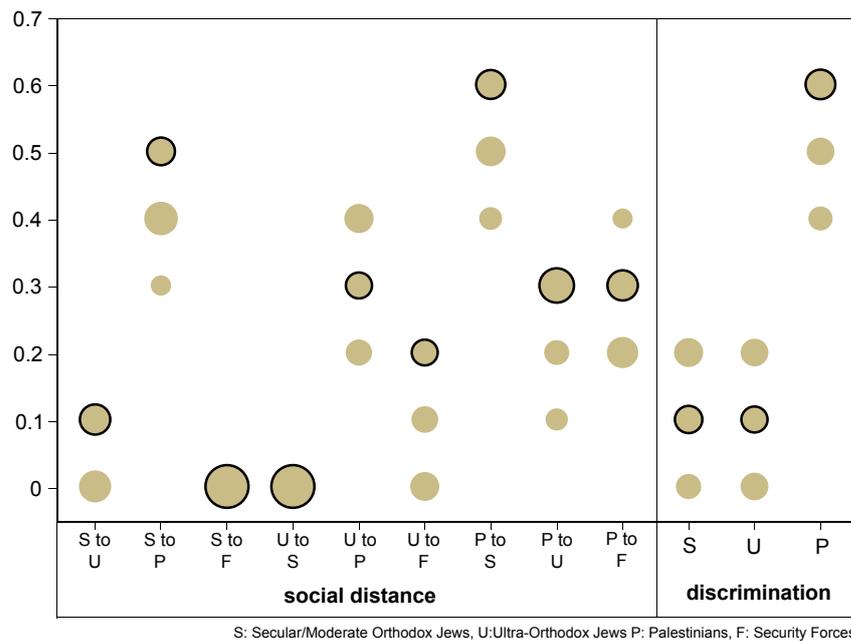
**Table A.3: Regression Analysis—Events per Neighborhood**

### A.4.3 Consistency of Parameter Values in the Subset of Good Fits

The distribution of the social distance and discrimination parameter values in the subset of parameter combinations that generate the best agreement with empirical data for the 2001–2004 period is shown in Figure A.4 (the corresponding figure for the 2005–2009 period is Figure 3.5 of the manuscript). The distribution of parameter values for each parameter is an important indicator of model fit and parsimony—in this analysis we follow Weidmann & Salehyan (2013). The more similar the parameter combinations in the subset, the more reliably the model approximates empirical data. In particular, a low spread in values for each parameter is a strong indicator that each parameter is necessary for generating model fit. In Figure A.4 a circle represents the occurrence of a given parameter value for the 2001–2004 period; the larger the circle the more often this parameter value is assumed in the subset. The parameter values of the reference scenario are marked with a black circle. As in the 2005–2009 period, all parameters in the subset of good fits are very consistent, featuring a low spread around the values assumed in the reference scenario.

### A.4.4 Empirical Plausibility of Parameter Values

In this section, we assess the plausibility of parameter values generated by the calibration exercise, specifically inter-group tension and discrimination, to the empirical situation. In the 2001–2004 period, the only recorded incidents of violence perpetrated by Secular/Moderate Orthodox Jews are carried out against Palestinians; in the reference scenario, this is correctly represented by a vanishing (or close to vanishing) social distance of Secular/Moderate Orthodox



**Figure A.4: Distribution of Parameter Values in the Subset of Good Fits (2001–2004)**

Jews towards all groups but Palestinians. Similarly, Ultra-Orthodox Jews in that time period only engage in violence towards Palestinians reflected in the large social distance towards this group. Palestinians perpetrate the majority of events, which in the reference scenario translates to non-zero social distances to the other population groups; in particular, the relationship with Secular/Moderate Orthodox Jews is very strained during the second *Intifada*. Note that both Secular/Moderate Orthodox and Ultra-Orthodox Jews in that time period do not perceive to be strongly discriminated by the state security forces, whereas Palestinians are subject to strict security measures and certainly perceive the resulting limitations in everyday life as (state) discrimination. The situation is consistently reflected in the discrimination variables of the reference scenario.

In the 2005–2009 period there is close to no recorded violence of Secular/Moderate Orthodox Jews towards Ultra-Orthodox Jews and no violence towards security forces. Compared to the 2001–2004 period there is also less Secular/Moderate Orthodox violence towards Palestinians. This again is reflected in the social distance as the tension proxy of the model, in particular a smaller social distance represents the decrease in Secular/Moderate Orthodox violence against Palestinians. In contrast to the 2001–2004 interaction dynamics, Ultra-Orthodox Jews primarily engage in violence with security forces; this is representative of the increasing estrangement from the Israeli state and radical opposition towards government involvement in Ultra-Orthodox affairs. In the reference scenario, this development is both reflected in the increased social distance towards security forces and the higher perception of (state) discrimination. Similar to the 2001–2004 period, Palestinians carry out most recorded attacks; the observable shift in violence targets towards security forces is represented by a corresponding shift in the social distance

## Appendix A. Supporting Information (SI): “Group Segregation and Urban Violence”

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parameters in the best fit vector.

Consistent with the model mechanisms, the limiting case of no violence in the simulated data corresponds to parameter vectors with vanishing social distance. The location of violence match for the 2001–2004 (2005–2009) period in this case is 0.32 (0.57) and the Pearson’s correlation for the number of violent events per neighborhood and the attack targets by group simply vanishes.<sup>22</sup> The non-zero value for the location of violence measure arises from the fact that in the 2001–2004 (2005–2009) period 24 (44) of 77 neighborhoods are empirically non-violent. Note that the 2001–2004 reference scenario has a poor quantitative agreement with the 2005–2009 empirical data, verifying that parameter vectors representing different interaction dynamics lead to poor agreement in the fit measures.<sup>23</sup>

### A.4.5 Adverse Effect of Policing

The effect of policing is conditional on the level of inter-group tension. It tends to mitigate violence for small social distances towards security forces but may incite violence if tensions are high (see also Section A.2.1). We validate that this aspect is correctly represented in our framework analyzing a stylized scenario identical to the 2005–2009 reference scenario but with social distances between civilian population groups set to zero, i.e., we exclusively model violence directed at the police. The simulations demonstrate that if social distance between a population group (in this case Ultra-Orthodox Jews and Palestinians) and the police is high, violence is self-perpetuating with levels of violence directed at the police nearly as high as in the reference scenario.<sup>24</sup> We then confirm that this adverse effect vanishes for social distances towards the police below a critical value of around 0.4.

## A.5 Counterfactual Scenarios

The potential “futures” of Jerusalem are characterized by specific provisions concerning the city’s population structure and migration dynamics. The inter-group relations underlying the violence mechanism are those of the reference scenario—any deviations in violence patterns from those of the reference scenario may therefore be fully attributed to the provisions of the “futures”. We report average statistics for the trends in the number of affected neighborhoods and total number of events generated from 100 runs that only differ in their random seed—this accounts for the influence of the probabilistic nature with which violence ensues. The futures are then illustrated using representative (or ideal typical) runs, i.e., simulation runs that have the most similar number of total events and violent neighborhoods compared to the average values of the scenario they represent. In order to draw conclusions regarding a relative increase/decrease

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<sup>22</sup>The no violence case then also sets the natural base line for the fit measures.

<sup>23</sup>The location of violence match in this case is below 0.61 and it has a Pearson’s correlation of 0.28 for the distribution of violence and of 0.57 for the distribution of violence targets.

<sup>24</sup>We initialize the model with different levels of policing and let the scenario develop endogenously afterwards; as long as this initial level is sufficiently high ( $> 0.2$ ), the resulting violence dynamics are comparable.

in violence, the event distributions are rescaled relative to the total number of events in the reference scenario.<sup>25</sup> Correspondingly, the violence categories used to visualize the results of the counterfactual analysis are comparable to those of the reference scenario; the use of qualitative categories emphasizes that the figures demonstrate forecasts of general trends and not precise predictions for expected numbers of violent incidents per neighborhood. The detailed provisions of the four “futures” discussed in the manuscript are given below. In order to develop an intuition for the degree to which trends in the scenarios are contingent on changes in inter-group relations, we explored a “worst” and “best” case realization of each of the four futures (see Table A.6 for an overview). These worst and best case scenarios are of course highly stylized but may still serve to illustrate how specific (political) developments would affect the simulated violence dynamics. The specific assumptions we make with regard to potential changes in social distance between population groups and perceptions of discrimination are informed by a number of public opinion polls.<sup>26</sup> Additionally, we introduce two stylized scenarios—Uniform Mixing and Localized Segregation—that illustrate the effect of neighborhood composition.

| <i>Return to 1967</i>  | <i>Clinton Parameters</i>   | <i>Palestinian Proposal</i>   |
|--|---|---|
| 1, 2, 3, 4, 5, 6, 7, 8, 12, 13, 14, 15, 16, 33, 34, 35, 36, 37, 38, 39, 40, 41, 64, 65, 66, 67, 68, 69, 70, 72, 75, 76, 77 | 1, 2, 5, 15, 33, 34, 35, 36, 37, 38, 39, 41, 64, 66, 69, 70, 75, 76, 77 | 1, 2, 5, 15, 33, 34, 35, 36, 37, 38, 39, 64, 66, 68, 69, 70, 75, 76, 77 |

numbering according to Table A.1

**Table A.4: East Jerusalem Neighborhoods under Palestinian Authority**

**A.5.1 Business as Usual**

The most similar to the reference scenario, this “future” reflects trends in the population dynamics already visible at present and believed to impact the dynamics in the city in the near future. The initial population in the scenario is based on the empirical distribution of the year 2008. Palestinians are assumed to have a strong preference to reside in East Jerusalem and only consider

<sup>25</sup>The event distributions become comparable to the reference scenario by rescaling with the same scaling factor: the number of events in the reference scenario divided by the total number of empirical events.

<sup>26</sup> List of sources:

Palestinian Center for Policy and Survey Research. (1994). <http://www.pcpsr.org/survey/cprspolls/94/poll10b.html> (accessed August 8, 2012).  
 IPCRI, Israel Palestine Center for Research and Information. (1996). <http://www.ipcri.org/files/future-jerusalem.html> (accessed August 8, 2012).  
 Palestinian Center for Policy and Survey Research. (2000). <http://www.pcpsr.org/survey/polls/2000/p1a.html> (accessed August 8, 2012).  
 Yediot Ahronot. (2005). Available at [http://www.angus-reid.com/polls/13440/israelis\\_split\\_over\\_future\\_of\\_jerusalem](http://www.angus-reid.com/polls/13440/israelis_split_over_future_of_jerusalem) (accessed August 8, 2012).  
 Palestinian Center for Policy and Survey Research. (2003). <http://www.pcpsr.org/survey/polls/2003/p10a.html> (accessed August 8, 2012).  
 BESA, Begin-Sadat Center for Strategic Studies. (2008). <http://www.biu.ac.il/SOC/besa/docs/JerusalemPollApril08-ENG.pdf> (accessed August 8, 2012).  
 Alvin Richman. (2010). “Attitude Factors in the Search for Israeli-Palestinian Peace: A Comprehensive Review of Recent Polls.” [http://www.worldpublicopinion.org/pipa/pdf/sep10/IsPal\\_Sep10\\_rpt.pdf](http://www.worldpublicopinion.org/pipa/pdf/sep10/IsPal_Sep10_rpt.pdf) (accessed: August 8, 2012).

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moving to neighborhoods in the East.<sup>27</sup> The Ultra-Orthodox population growth is set to 4 percent annually, reflecting the empirical trend of an increased population growth compared to the remainder of the population. Capturing the fact that Ultra-Orthodox tend to move to neighboring Secular/Moderate Orthodox quarters, those areas are considered to have a 50 percent probability for Ultra-Orthodox in-migration. The scenario further reflects the substantial Ultra-Orthodox migration to the three neighborhoods *Ramat Shlomo*, *Kiryat Hayovel* and *Har Homa* by introducing a (small) bias in every Ultra-Orthodox migration decision towards a move to those neighborhoods. The *Muslim*, *Christian* and *Armenian Quarter* see additional Jewish population growth as a consequence of right-wing Jewish groups pushing to obtain property and establish settlements in the holy basin.

As the “worst” case situation, we assume that Israel would continue to expand settlements in East Jerusalem, partly in currently unsettled locations (e.g., *Givat Hamatos* in the south, *Ramat Shlomo* in the north), and partly by claiming/re-claiming property arguably owned by Jews in the past (e.g., *Sheikh Jarrah* north of the *Old City*, *Silwan* south of the *Old City*, *Ras el-Amud* east of the *Old City*). This would create more points of friction, undoubtedly increase feelings of discrimination among Palestinians (in general and specifically in these locations) and contribute to increased social distance towards the Jewish population groups. In the model, these developments are reflected by an increase in the Palestinian perception of discrimination (+0.05 compared to the reference scenario) and an increase of social distance of Palestinians toward the two Jewish population groups (+0.05 towards each faction).

In a “best” case scenario, Israel might stop expansion toward the East and invest heavily in improving Palestinian infrastructure (roads, building permits, educational system, employment/business centers in the East, etc.). Overall, this should significantly reduce feelings of discrimination and Palestinian social distance towards Israelis and security forces—we represent this by a decrease in the Palestinian perception of discrimination (−0.1) and a significant decrease in social distance towards Secular Jews and security forces (−0.1 towards each faction). We assume that civic relations with Ultra-Orthodox Jews will remain as strained as before, with tension continuing to arise from conflict over access to holy sites in the old city and in East Jerusalem.

### A.5.2 Return to 1967

The return to the boundaries of 1967 implies major changes to the population structure of the city;<sup>28</sup> in particular, it assumes that the Jewish population in former Jewish East Jerusalem neighborhoods would be evicted and relocate to newly constructed dwellings in the West or, in the case of Ultra-Orthodox Jews, largely migrate to neighboring cities. The new Jewish dwellings are expected to be constructed in the neighborhoods of *Ein Kareem*, *Ein Lavan* and in the area north of *Ein Kareem* and west of *Har Nof*. The projected size of the new dwelling units is

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<sup>27</sup>The definition of East Jerusalem is in accordance with the boundaries of 1967 (Table A.4).

<sup>28</sup>The empirical distribution of the year 2008 is taken as the base to which changes of the population structure are applied.

estimated from information on planned construction in those neighborhoods (Table A.5). The scenario assumes that half of the Palestinian population in (former) East Jerusalem relocates to the vacated neighborhoods; within three years the neighborhoods are then again fully settled due to a substantial inflow of Palestinians from the West Bank. In this process the total population balance in the city shifts in favor of Palestinians. In particular, the Ultra-Orthodox population fraction declines significantly due to their out-migration, while their annual population growth assumed at 4 percent remains the largest in the city. The clear division into Israeli West and Palestinian East Jerusalem has two main implications: (1) The east of the city comes under Palestinian authority—analogueous to the interaction of Secular/Moderate Orthodox Jews with the Israeli security forces in the West, there is no confrontation between the (almost exclusively Palestinian) population in the East and the Palestinian security forces. In the simulations this is captured by a vanishing social distance (tension) between population and security forces.<sup>29</sup> (2) There are strict restrictions on mobility between the two parts, both for relocation within the city as well as for daily movement around the city: (a) Jewish citizens will only consider moving to neighborhoods in the West, Palestinian citizens only to neighborhoods in the East; (b) mobility across the East/West divide is reduced such that a person from the East (West) only has a reduced probability to interact with a person from the West (East).<sup>30</sup> The scenario further reflects the special case of the Palestinian neighborhoods *Sharafat* and *Beit Safafa* where inhabitants are either Israeli citizens or Israeli residents but the identity of both groups is Palestinian. The two neighborhoods are among the richest in the East and their inhabitants are very unlikely to move to other Palestinian neighborhoods in East Jerusalem. At the same time, the scenario assumes no considerable in-migration of other Palestinians since there are very attractive (former Jewish) neighborhoods closer to the Palestinian city center. The simulation accounts for this special case by disallowing migration to and from the two referred neighborhoods. The “future” further assumes a special international regime for the holy basin and *Mount Scopus*: maintaining the status quo in particular implies no major shift in demographic balance. In the simulation, migration to and from neighborhoods in the holy basin is therefore subjected to the condition that the population fractions may not change by more than 5 percent compared to the status quo where the 2008 population in each quarter defines this status quo.

Dividing the city entails a massive relocation of the Jewish population from East Jerusalem—while it is safe to assume that they would be nicely compensated, a repartitioning of the city will in the “worst case” lead to strong resentments toward Palestinians across all segments of the Jewish population and also potentially increase distance between the Jewish population and the Israeli security forces (demonstrations, sabotage, violent attacks, etc.). Note that in particular ceasing control of the old city would contribute to the social distance of Jews towards Palestinians. The “worst case” scenario reflects these developments through an increase of social distance towards Palestinians (+0.05 for Ultra-Orthodox and Secular) and Israeli security forces (+0.05 Ultra-Orthodox, +0.1 Secular). In addition, dividing the city and re-settling Palestinians in the

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<sup>29</sup> Adjusting for the different relationship of the population to the security forces in the East is the minimal generalization of the violence mechanism to the case where security forces represent two different state actors.

<sup>30</sup> The value of the mobility restriction is chosen based on trial runs; at the selected value of the mobility restriction the observed dynamics are first noticeably affected.

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former Jewish enclaves in East Jerusalem might create intra-Palestinian frictions (Jerusalemites vs. newcomers) and increase social distance between Palestinians and Palestinian security forces (+0.1). If the agreement does not include specific provisions allowing Palestinians access to centers of employment in the West this would likely negatively affect Palestinian perceptions of discrimination (+0.05).

The effects of partition, however, could be moderated if specific provisions were to be introduced into the agreement. In such a “best case” scenario relatively free travel across the East/West divide would be possible allowing Palestinians easy access to employment centers (in the West), and Jews access to the *Old City* and other holy and symbolic locations in the East. Overall this might lead to slight but visible improvements in civic relations both on the side of the Palestinians (−0.05 towards Secular Jews and −0.1 towards Israeli security forces) as well as on the side of the Ultra-Orthodox Jews (−0.05 towards Palestinians); moreover, it should positively affect perceptions of discrimination among Palestinians (−0.1).

| <i>Neighborhood ID</i> | <i>Neighborhood Name</i> | <i>Expected Residents</i> |
|------------------------|--------------------------|---------------------------|
| 52                     | Ein Karem                | 9900                      |
| 55                     | Ein Lavan                | 45000                     |
| 5555                   |                          | 6750                      |

**Table A.5: Housing Projects Relevant to the “Futures”**

### A.5.3 Clinton Parameters

In comparison to *Return to 1967*, the changes to the population structure assumed here are less fundamental: the city remains integrated without exchange of territories or similar measures, however, the transfer of authority and the responsibility for maintaining security in East Jerusalem to the Palestinians again has a substantial impact on the projected violence dynamics.<sup>31</sup> The city remains integrated but relocation is assumed to be strictly divided along ethnic lines: Jewish (Secular/Moderate Orthodox and Ultra-Orthodox) inhabitants’ migration is limited to Jewish neighborhoods and Palestinian migration to Palestinian neighborhoods. This also implies a slow out-migration of Jews from mixed neighborhoods in the East and of Palestinians from mixed neighborhoods in the West. The inhabitants of *Sharafat* and *Beit Safafa* are subject to the same migration restriction as in the previous scenario and the Ultra-Orthodox population growth rate is again set to 4 percent annually. There is no clear division into Israeli West and Palestinian East Jerusalem, but as a consequence of security concerns Palestinian access to Jewish neighborhoods is restricted; this is implemented analogous to the (general) East/West mobility restriction in the *Return to 1967* scenario. The “future” is further subject to a special international regime for the holy basin and *Mount Scopus* as detailed in *Return to 1967*.

In a “worst case” scenario this future might be perceived as a compromise that no side is actually content with—together with new points of frictions between Palestinians and Israelis we would

<sup>31</sup>The definition of neighborhoods constituting East Jerusalem in this scenario may be found in Table A.4.

thus generally anticipate additional strains on all inter-group relations. In the case of the Ultra-Orthodox faction the loss of control over symbolic sites like Mt. Olive and the *Old City* would also contribute to the rising social distance towards Palestinians (+0.05). The general strain of civic relations would also be reflected in Palestinian relations with Secular and Ultra-Orthodox Jews (+0.05 towards both factions). The general dissatisfaction of Palestinians—due to substantial losses of areas in East Jerusalem as a consequence of the agreement, for example—might lead to increased tensions with Palestinian security forces (+0.05) and rising perceptions of discrimination (+0.05). Analogous to the *Return to 1967* scenario, these negative effects could be moderated by introducing additional provisions to the agreement. The consequences of such a “best case” scenario with regard to social distances and perceptions of discrimination would be analogous to those detailed for *Return to 1967*.

### A.5.4 Palestinian Proposal

This scenario reflects a number of concessions Palestinian negotiators are purported to have brought forward in May 2008 and that were first reported by *The Guardian* in January 2011:<sup>32</sup> the sovereignty in the city would be largely divided along the lines specified by the *Clinton parameters* with a few marked exceptions. The Palestinian side would accept Israeli authority over the Jewish settlements in East Jerusalem with the exception of *Har Homa* (the neighborhood has a critical strategic importance as it provides Palestinians with direct access to Bethlehem and would be put under Palestinian authority). The scenario further assumes that the Israeli inhabitants of *Har Homa* relocate to new housing developments in *Ein Karem* and in the area north of *Ein Karem* and west of *Har Nof*; the neighborhood would initially be half settled by Palestinian citizens of East Jerusalem with the remainder of the dwellings occupied by Palestinians moving in from the West Bank over the course of the next three years. As a concession to Israeli interests, the Palestinians would in turn agree to give Israel control over two controversial areas: the Israeli settlement of *Shimo?n Hatzadik* in the Palestinian neighborhood of *Sheikh Jarrah*, including the nearby sacred graves, and the *Armenian quarter* in the *Old City*. The Palestinian proposal also includes provisions regarding the other key points of conflict such as the status of the old city, operationalized here as the special international regime detailed in the *Return to 1967* scenario. The “future” further assumes a clear division into Israeli West and Palestinian East Jerusalem with the same restrictions on mobility as outlined in the *Return to 1967* scenario. Analogous to the previous scenarios, the Ultra-Orthodox population growth rate is set to 4 percent annually and the inhabitants of *Sharafat* and *Beit Safafa* are again subject to the same migration restrictions.

This scenario is very similar to the *Clinton Parameters* in its key structural changes and we assume its “worst case” and “best case” developments to be analogous, both with regard to expected developments and their operationalization via changes in social distance and perceptions of discrimination.

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<sup>32</sup>*The Guardian* (2011); the newspaper provides access to leaked internal documents and reports documenting the content of the talks at <http://www.guardian.co.uk/world/palestine-papers-documents/browse> (accessed August 8, 2012).

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| <i>Counterfactual Scenario</i> |             | <i>Total number of violent events**</i> |             | <i>Number of violent neighborhoods**</i> |             |
|--------------------------------|-------------|---|-------------|--|-------------|
|                                |             | <i>mean</i>                             | <i>std.</i> | <i>Mean</i>                              | <i>std.</i> |
| Business as Usual              | Best Case   | - 48%                                   | 9%          | - 32%                                    | 9%          |
|                                | Status Quo* | + 6%                                    | 8%          | + 3%                                     | 8%          |
|                                | Worst Case  | + 39%                                   | 10%         | + 14%                                    | 8%          |
| Clinton Parameters             | Best Case   | - 60%                                   | 8%          | - 39%                                    | 9%          |
|                                | Status Quo* | - 33%                                   | 8%          | - 10%                                    | 9%          |
|                                | Worst Case  | + 5%                                    | 9%          | + 7%                                     | 9%          |
| Palestinian Proposal           | Best Case   | - 67%                                   | 8%          | - 47%                                    | 11%         |
|                                | Status Quo* | - 42%                                   | 8%          | -19%                                     | 9%          |
|                                | Worst Case  | - 25%                                   | 11%         | - 9%                                     | 9%          |
| Return to 1967                 | Best Case   | - 75%                                   | 8%          | - 57%                                    | 11%         |
|                                | Status Quo* | - 52%                                   | 8%          | - 32%                                    | 9%          |
|                                | Worst Case  | - 26%                                   | 8%          | - 19%                                    | 7%          |

\*Status Quo counterfactual results based on 2005-2009 parameter values without additional changes to social distance and discrimination

\*\*relative increase/decrease compared to the total number of violent events (neighborhoods) in the reference scenario

**Table A.6: Best and Worst Case Realizations of the “Futures”**

### A.5.5 Uniform Mixing

In this stylized scenario the population of each neighborhood is recast to reflect the citywide ratio of the social groups in 2005 (41% Secular/Moderate Orthodox Jews, 25% Ultra- Orthodox Jews, 34% Palestinians). Uniform mixing is further achieved by randomizing the position of all agents within each neighborhood. The outcome of the simulation (Figure A.5a) is a diffusion of violence to West Jerusalem, with a substantial increase in frequency in several neighborhoods, which contrasts with the 2005–2009 reference scenario where the bulk of violence occurs in East Jerusalem. Consistent heterogeneity did not, however, result in the diffusion of violence to all parts of the city, and several neighborhoods remained unaffected, largely due to their small population sizes. Of note, the correlation between the frequencies of violence in the representative run of the complete mixing counterfactual and the reference scenario is high (0.44). However, the counterfactual yields the same prediction for the onset of violence as the reference scenario in only 50 of 77 neighborhoods (64.9%) and as the empirical data in 41 of 77 neighborhoods (53.2%).

### A.5.6 Localized Segregation

This second stylized scenario represents the opposite case to Uniform Mixing: Locally segregated populations are designated by changing the demography of Jerusalem to create entirely homogenous neighborhoods. For this purpose, in each neighborhood we maintain the total population from 2005 but only seed the majority group. A comparison of the representative run to the 2005–2009 reference scenario indicates a reduction in violence in 13 East Jerusalem neighborhoods and 11 West Jerusalem neighborhoods (Figure A.5b). The correlation between the frequencies of violence is high (0.65). Also, the simulation matches the reference scenario

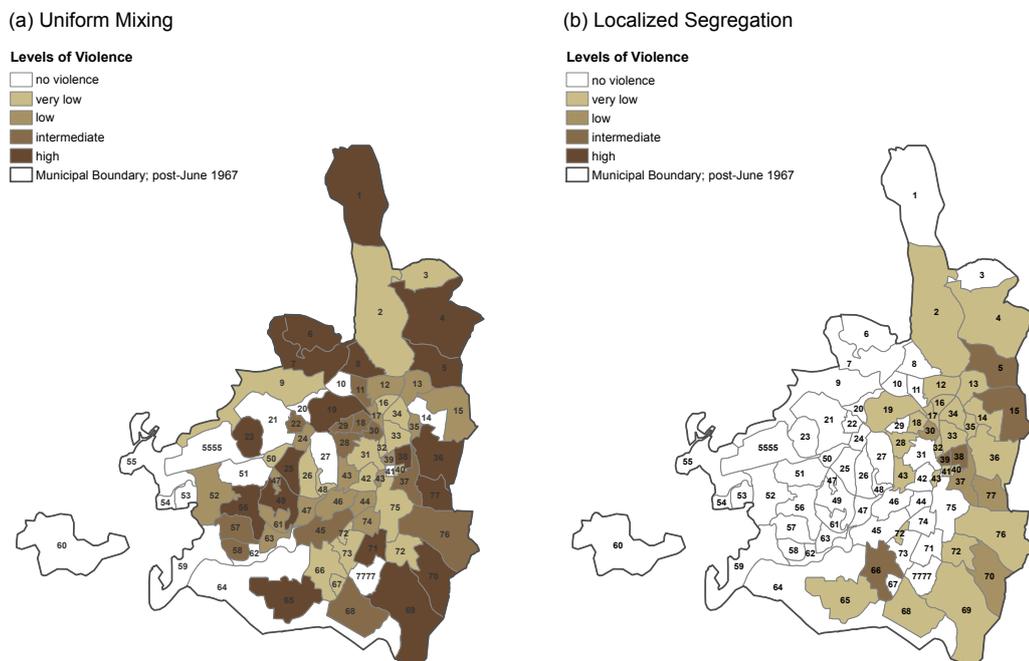
## A.5. Counterfactual Scenarios

|                                 | Uniform Mixing                                       | Localized Segregation    |
|---------------------------------|--|--------------------------|
| Idealized Changes               | mixed neighborhoods<br>(according to citywide ratio) | segregated neighborhoods |
| Number of Violent Neighborhoods | + 65%<br>(std. 3%)                                   | - 15%<br>(std. 11%)      |
| Number of Violent Events        | +240 %<br>(std. 13%)                                 | - 23%<br>(std. 13%)      |

**Table A.7: Summary of Additional Counterfactual Scenarios**

with respect to the occurrence of violence in 65 of 77 (84.4%) and the empirical data in 56 of 77 neighborhoods (72.7%).

The Uniform Mixing counterfactual represents the limiting case of maximum intergroup contact and produced a sharp increase in both the number of violent neighborhoods (+65%, relative to the reference scenario) and the frequency of violence (+240%). Localized Segregation on the other hand features minimal intergroup contact within each neighborhood and consequently sees a significant reduction in violence (-15% violent neighborhoods, -23% violent events). The two scenarios thus demonstrate the maximum extent to which neighborhood composition within each neighborhood influences levels of violence.



**Figure A.5: Additional Counterfactual Results**



# B Supplementary Information (SI): “Matched wake analysis”<sup>†</sup>

## B.1 Empirical details

In this section, we provide detailed replication instructions for the published results. For legal reasons, we cannot share the actual replication data as some of it is still classified. However, all data is in the public domain and can therefore be used for replication purposes.

### B.1.1 Data acquisition

The SIGACT data that we analyzed were made available to the general public through wikileaks.org on October 22, 2010. A substantive subset of the data data has also been made available by the Guardian Newspaper (see <http://www.theguardian.com/news/datablog/2010/oct/23/wikileaks-iraq-data-journalism#data>). We loaded the full SIGACT file into a PostgreSQL database with the PostGIS extension installed. Matching variables were generated by superimposing spatial covariates such as nightlight emissions (NGDC, 2012), spatially referenced population numbers (CIESIN, 2005), and ethnic settlement regions (Wucherpfennig et al., 2011) with the locations of SIGACT events. Based on nearest neighbor mapping, multivariate information was generated for each SIGACT event.

We used several selection criteria to generate an empirical sample for the analysis. First, we only investigated “IED Explosions” as treatment and control events. We further narrowed down the analysis to events within the greater Baghdad area for the results reported in the article. These events are coded with much higher accuracy than events outside the Iraqi capital: The spatial resolution of these events is approximately one kilometer while the rest of Iraq is only coded with a ten kilometer resolution. The spatial resolution can be easily identified from the length of the MGRS coordinates that are associated with all observations (see [http://en.wikipedia.org/wiki/Military\\_grid\\_reference\\_system](http://en.wikipedia.org/wiki/Military_grid_reference_system)). We used this criterion to select all

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<sup>†</sup>This chapter is an edited version of the supplementary information for the following article: Sebastian Schutte and Karsten Donnay. (2014). “Matched wake analysis: Finding causal relationships in spatiotemporal event data.” *Political Geography* 41: 1–10. It can be downloaded at <http://dx.doi.org/10.1016/j.polgeo.2014.03.001>.

## Appendix B. Supplementary Information (SI): “Matched wake analysis”

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observations from the greater Baghdad area, the complimentary analyses for data covering Iraq without Baghdad are shown in Section B.2.1. In both cases, we narrowed down the analysis further by focusing on events that led to “Significant Military or Civilian Casualties”. This information is provided by a field in the SIGACT data that stands for “Commander’s Critical Information Requirements” (CCIR). IED explosions that led to civilian casualties were coded as treatment events and those that did not lead to any civilian casualties were coded as control events. A list of all event IDs used in the analysis can be provided upon request.

### B.1.2 Summary statistics for the matching variables

For the analysis in the article and the supplementary analysis in Section B.2.1, we used a series of matching variables to account for confounding factors. As data on the ethnic composition of the civilian population in the vicinity of attacks could not be acquired for the analysis of Baghdad, we only used population numbers for the year 2000, nightlight emissions for the year 2008, and distance from the heavily secured “Green Zone” in the city center as matching variables.<sup>1</sup> For the analysis of the rest of the country, however, we used information on the predominant ethnic group in the vicinity of attacks from the GeoEPR dataset (Wucherpfennig et al., 2011). GeoEPR does not code the ethnic composition of major cities. Table B.1 shows moments for the empirical distributions used in the Baghdad analysis. Table B.2 shows corresponding statistics for the analysis of Iraq in Section B.2.1. Please note that the variables for the predominant ethnic groups in the vicinity of the attack site are binary indicators and that ethnic settlement regions can overlap.

| Variable                   | Mean   | Std. dev. | Min.  | Max.   | Obs. |
|----------------------------|--------|-----------|-------|--------|------|
| Population count (2000)    | 156.49 | 77.32     | 13.84 | 214.67 | 1098 |
| Nightlight emission (2008) | 37.95  | 23.33     | 0.00  | 63.00  | 1098 |
| Distance from “Green Zone” | 20.30  | 13.22     | 0.33  | 53.21  | 1098 |

**Table B.1:** Descriptive statistics for the matching variables in the Baghdad analysis.

| Variable                   | Mean   | Std. dev. | Min.  | Max.   | Obs. |
|----------------------------|--------|-----------|-------|--------|------|
| Population count (2000)    | 32.14  | 31.41     | 3.67  | 214.03 | 2576 |
| Nightlight emission (2008) | 7.84   | 10.46     | 0.00  | 63.00  | 2576 |
| Distance from “Green Zone” | 164.11 | 115.87    | 13.17 | 517.95 | 2576 |
| Sunni area                 | 0.81   | 0.39      | 0.00  | 1.00   | 2576 |
| Shi’a area                 | 0.43   | 0.49      | 0.00  | 1.00   | 2576 |
| Kurdish are                | 0.15   | 0.36      | 0.00  | 1.00   | 2576 |

**Table B.2:** Descriptive statistics for the matching variables in the analysis of Iraq excluding Baghdad. Data on the settlement areas of ethnic groups was obtained from Wucherpfennig et al. (2011). Please note that the groups overlap spatially.

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<sup>1</sup>Weidmann & Salehyan (2013) have coded data for the city of Baghdad, but information on the greater Baghdad area is not available.

## **B.2 MWA analysis for Iraq**

### **B.2.1 Civilian collaboration in Iraq in the period 2008–2009**

As discussed in the article, SIGACT data for Iraq are recorded in different spatial resolutions for different parts of the country. Events in the greater Baghdad are coded with a spatial resolutions of about 1km. For the rest of the country, they are coded at a more coarse resolution of about 10km. Combining these data in a single analysis can lead to spurious effects. We therefore decided to focus on Baghdad in the main analysis.

To supplement our insights from Baghdad, we tested whether the proposed mechanism generalizes to the entire country. Excluding events from the Baghdad area, we focused on the remaining incidents from all other parts of Iraq. Again, we used IED attacks that were classified as severe in the “friendly force information requirements” associated with many SIGACT observations. Those events that injured or killed civilians were classified as “treatments” and those that did not as “controls”. Spatial distances between 5 and 35 kilometers were analyzed and temporal distances of up to 7 days. As Figure B.1 shows, for smaller distances and a temporal offset of two days, a small positive effect can be found.

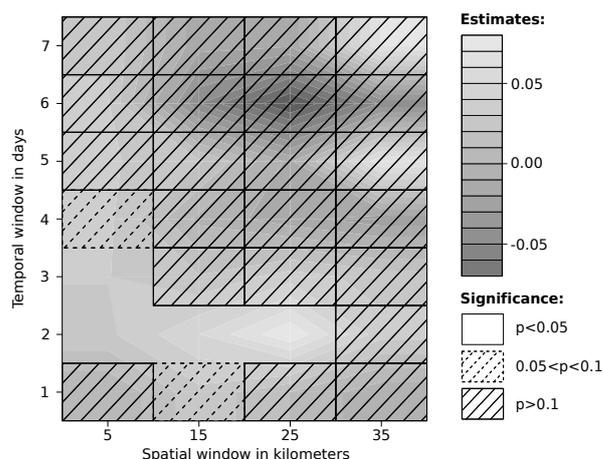
Of course, this analysis draws on coarser data than the Baghdad analysis reported in the article. Table B.3 shows estimates and the fraction of overlapping events for the Iraq analysis. Table B.4 shows summary statistics for the matching procedure. While the effect is comparably smaller than in the case of Baghdad, the low level of overlaps in the data and the good balance for treatment and control groups lend additional support to our conclusion: the observed reactive pattern of civilian assistance to US forces in response to indiscriminate insurgent violence is present in all of Iraq for the period under investigation. In areas of Iraq outside of Baghdad the significant reactive patterns simply occur sooner after instances of indiscriminate insurgent violence.

### **B.2.2 Sensitivity analysis for empirical findings**

The results of the Baghdad analysis are robust across a wide range of specifications (Figure B.2, details below). This includes the exact choice of temporal and spatial window sizes, weighted vs. unweighted regression and additionally including our matching variables in the DD estimation. Figure B.3 shows that there are no additional significant signatures for additional time spans.

The Baghdad data has a maximum resolution of 1km and is mapped on mgrs coordinates. In this situation, concentric circles around any point do not necessarily include all neighboring grid points. A circle of 1 km radius, for example, would not include the closest points to the North-West, North-East, South-West and South-East. For this reason we also tested larger spatial window sizes chosen to include the full (first order Moore) neighborhood on the grid in the first step. In the article we estimated effects at the level of days, here we perform the same analysis for 12h and 48h windows. As an additional robustness test, we employed unweighted regressions in our DD estimation and also tested the effect of including our matching variables in the DD

**Appendix B. Supplementary Information (SI): “Matched wake analysis”**



**Figure B.1:** Empirical results of the MWA analysis of civilian collaboration in Iraq for the 2008–2009 period. The underlying contour plot shows the estimated effect of insurgent violence against civilians on civilian collaboration with the incumbent.

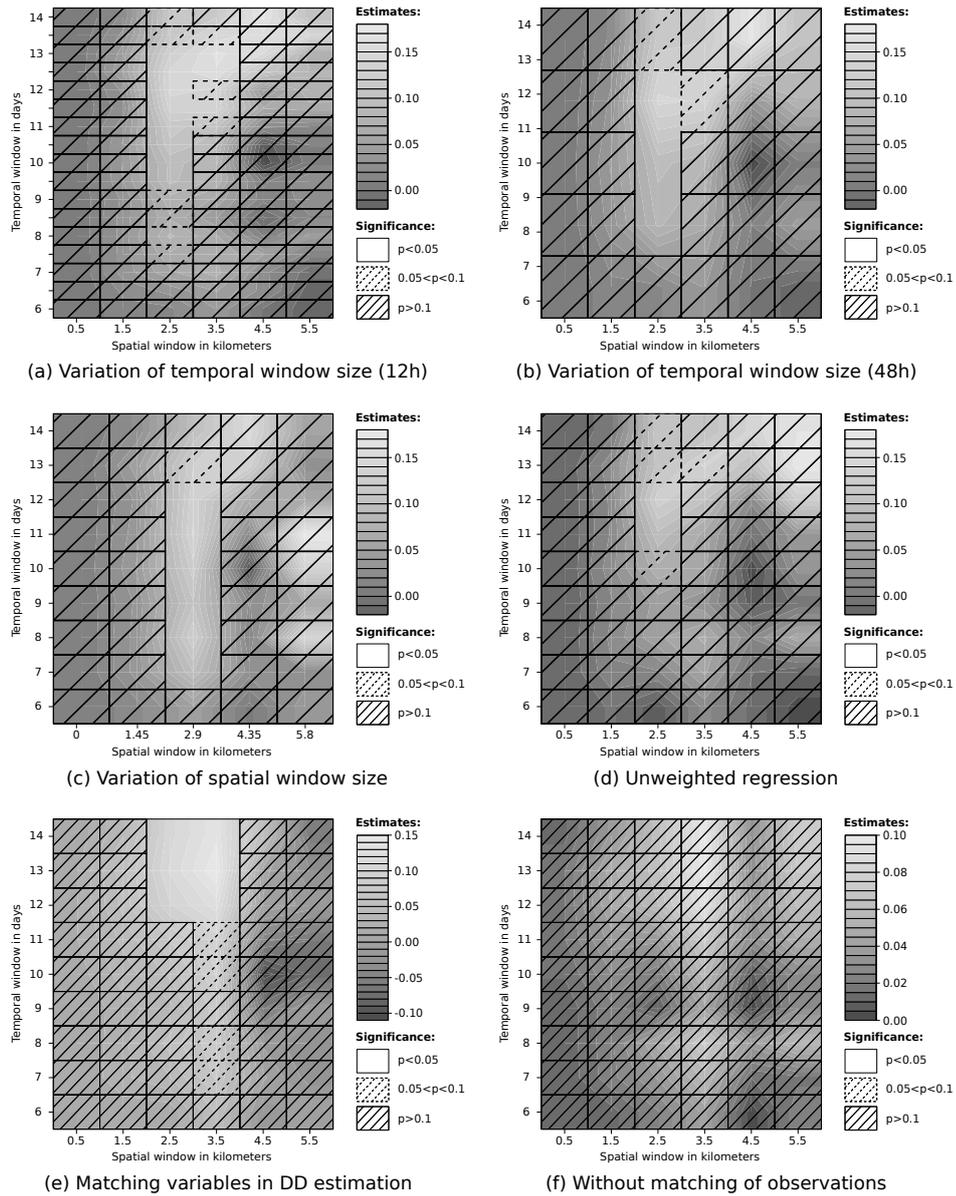
| Time (days) | Space (km) | Treatment effect | P-value | SO   | MO   |
|-------------|------------|------------------|---------|------|------|
| 2           | 5          | 0.03             | 0.02    | 0.08 | 0.07 |
| 2           | 15         | 0.05             | 0.03    | 0.16 | 0.14 |
| 2           | 25         | 0.08             | 0.01    | 0.19 | 0.17 |
| 3           | 5          | 0.03             | 0.04    | 0.10 | 0.09 |

**Table B.3:** Summary statistics for the interpretable areas of the contour plot in Figure B.1. SO (“same overlap”) refers to situations where either the cylinders of two or more treatment or two or more control events overlap. MO (“mixed overlap”) refers to situations where treatment and control cylinders overlap.

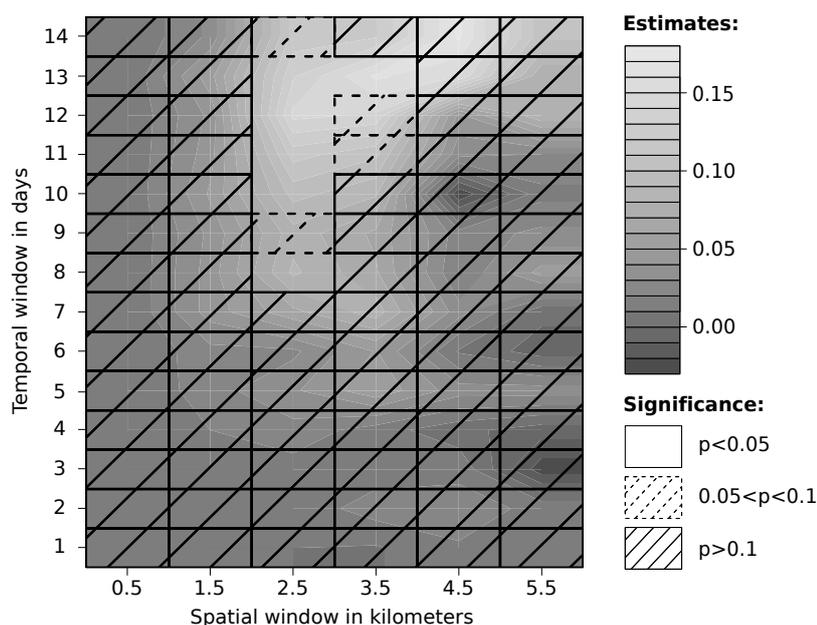
| Time (days) | Space (km) | Controls <sub>pre</sub>  | Treatments <sub>pre</sub>  | L1 <sub>pre</sub>  | %Support <sub>pre</sub>  |
|-------------|------------|--------------------------|----------------------------|--------------------|--------------------------|
| 2           | 5          | 357                      | 371                        | 0.44               | 36.80                    |
| 2           | 15         | 357                      | 371                        | 0.49               | 29.80                    |
| 2           | 25         | 357                      | 371                        | 0.52               | 26.80                    |
| 3           | 5          | 355                      | 371                        | 0.46               | 33.70                    |
| Time (days) | Space (km) | Controls <sub>post</sub> | Treatments <sub>post</sub> | L1 <sub>post</sub> | %Support <sub>post</sub> |
| 2           | 5          | 312                      | 288                        | 0.33               | 70.90                    |
| 2           | 15         | 288                      | 270                        | 0.34               | 68.70                    |
| 2           | 25         | 276                      | 260                        | 0.36               | 65.50                    |
| 3           | 5          | 306                      | 276                        | 0.33               | 70.50                    |

**Table B.4:** Matching statistics for the interpretable areas of the contour plot in Figure B.1.

estimation. We find that for all of these specifications the results are very consistent with those reported in the article (see Figure B.2). Panel (f) on the bottom right shows the results same analysis as reported in Figure 4.7 of the article but without matching observations. The results suggest that without matching selection bias would strongly affect our substantial findings.



**Figure B.2:** Empirical results of the MWA analysis of civilian collaboration in Baghdad for the period 2008–2009 for different specifications.



**Figure B.3:** Empirical results of the MWA analysis of civilian collaboration in Baghdad for the period 2008–2009 for time windows ranging from 1 to 14 days.

### B.3 Additional insights from Monte Carlo simulations

In this section we provide additional details on various aspects of Matched Wake Analysis (MWA). We both give further details on the generating process for our test data and on the effects of overlapping interventions in our sample.

#### B.3.1 Data generation

The procedure to generate artificial test data for our analysis employs two separate steps. First, spatiotemporal patterns of “dependent”, “treatment” and “control” type events are constructed to represent a specific causal effect of treatment versus control on the level of dependent events. All of our simulation tests use the smallest possible increment of 1 additional dependent event per treatment episode and no increase for control episodes.<sup>2</sup>

We specifically chose effect size 1 for two reasons. First, to pose a difficult simulation test for the method to pass. The larger the effect size, the easier it would be to recover the pattern. For overlapping spatiotemporal episodes, where counts in the dependent category additionally vary due to the overlap, significant differences of 1 in the level of dependent events are increasingly

<sup>2</sup>While there is always the same increment of 1 for treatment and no increase in the number of dependent events following control episodes, they do vary with respect to the total number of dependent events. In particular, we guarantee variability in the trend of dependent events in the backward looking window: in all simulations it randomly varies between -1, 0, or +1 for different episodes.

### B.3. Additional insights from Monte Carlo simulations

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difficult to detect. Note that this is similar to the much more noisy data in empirical cases. Second, to emulate the kind of effect sizes we see in empirical data. In fact, empirical effect sizes are often smaller than 1, i.e. on average we see a significant increase or decrease of effect size 1 in the dependent variable only after multiple ‘trigger’ events. Note that we argue in our empirical example that such a seemingly “disproportionate” effect size may be very realistic depending on the kind of dependent and intervention events.

Assigning confounding factors to each intervention event (and thus to every pattern) we use two simple stylized categories: one factor is randomly drawn from a uniform distribution in the interval [0.8, 1.2], the other from a Gaussian distribution (mean 1, std. dev. 0.1). We chose this simple procedure for a number of reasons. First, drawing confounding factors from a reasonably narrow interval ensures that with finite probability every treatment event can be matched to a control event and vice versa thus enabling a reasonably high post-matching balance. Second, by randomly assigning confounding factors we do not introduce a systematic dependence between these factors and the causal effect. In empirical data confounding factors may be driving levels of violence and thus be systematically increasing (or decreasing) the levels of dependent events. In this case, matching on the confounding factors, guarantees that our estimates of the true effect are unbiased. In other words, without loss of generality we can use a simpler data generation process since after matching the influence of confounding factors on the level of dependent events should be accounted for.<sup>3</sup>

In the second data generation step we then distribute our artificial episodes—100 treatment and 200 control type for all our simulation test—in space and time. Specifically, we distribute them over a geographical region within the 1st latitude North and South and the 1st longitude East and West, an area that covers about 220 km by 220 km (see Figure 4.3 of the article), and within a specified time period that varies with the exact test we are running. Geographically, the events are distributed following a simple random scattering algorithm that guarantees a significant clustering in the lower left hand corner of the map. We achieve this simply by randomly distributing half of the episodes of each intervention type over the whole area and the other half only over the lower left quadrant. When distributing the episodes in time, 90% of both intervention types are distributed randomly within the full time period considered. The other 10% are then distributed over a much shorter time interval. We chose to select these 10% from the subset geographically distributed in the lower left hand corner thus guaranteeing that both clustering in space (*exposure*) as well as clustering in time (*momentum*) co-occur in space and time. This simple data distribution algorithm is meant to mimic the clustering of conflict events in space and time generally observed in empirical data (see Figure 4.3 of the article).

The length of different time periods depend on each specific test situation we considered. The results for the simple, non-overlapping dataset discussed in Section 4.4.2 of the article were generated with data distributed over a 20 year period. For the results reported in Figure 4.5 of the

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<sup>3</sup>From a practical point of view, a simpler data generation process further avoids spurious dependencies on confounding factors. Such dependencies may introduce additional unwanted noise as an artifact of our data generation mechanism – something completely undesirable in a clean test dataset.

article, we generated a number of test datasets, in which the treatment and control episodes—in each case again 100 treatment and 200 control episodes—cluster over time intervals ranging from maximally 1 year to minimally 10 days. The incrementally shorter time periods lead to datasets with very low to very high overlaps of the spatiotemporal cylinders respectively. To obtain the confidence intervals, we performed the analysis at each degree of overlap for 100 randomly generated datasets that differ with regard to the exact patterns emerging from the overlaps when randomly distributing them in space and time.

### B.3.2 MWA for increasing degrees of SUTVA violations

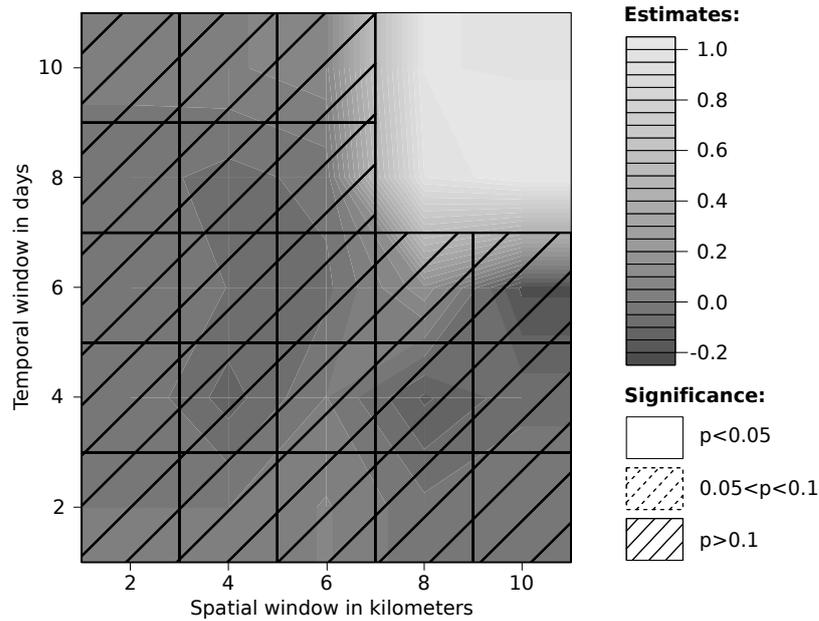
To test the effects of substantial violations of the SUTVA assumption, we first ran a test with one artificial data set, which was constructed to have a degree of overlapping interventions comparable to those in the empirical analyses. This test confirms that substantive insights can still be obtained from mildly clustered empirical samples.

As discussed in the article, we constructed simulated reactive events with a treatment effect of +1 at 8 days and 8 km. Again, we distribute the patterns geographically over our test region (see also Figure 4.3 of the article) but now within a much shorter time period, 2 months instead of one year. In this case the method still clearly recovers the +1 signature in the number of dependent events at 8 days and 8 km, as well as for larger spatiotemporal distances (Figure B.4). Note that we applied MWA with additional matching on previous counts of treatment and control events to counter the effect of SUTVA violations (see also Section 4.4.3 of the article). We further use weighted regression in the estimation as there is some imbalance remaining between treatment and control cases after matching (see also Table B.6 below).

Table B.5 summarizes the significant findings and shows the fraction of cylinders with instances of double treatment and spills (i.e. treatment events in a control cylinder and vice versa). The effect of the overlap in the spatiotemporal cylinders due to the strong clustering of intervention episodes in space and times is clearly visible. Notice that the overlap at 8 days, 8 km is within the range tested in our Monte Carlo analysis in Section 4.4.3 of the article. Table B.6 shows summary statistics for the matching procedure. The percentage of common support increases and the imbalance decreases noticeably through matching. This indicates a improved covariate balance after matching.

In Section 4.4.3 of the article, we discussed the effect of a series of Monte Carlo simulations that systematically explored the dependence of the MWA estimates on the degree of SUTVA violations. The results were presented as a function of % overlaps where we distinguish between “same overlap” (SO) and “mixed overlap” (MO). SO refers to situations where the spatiotemporal cylinders of either two or more treatment or two or more control events overlap whereas MO then refers to situations where treatment and control cylinders overlap. The results in Figure 4.5 of the article were depicted as a function of the % SO but they are substantively identical as a function of % MO (Figure B.5).

### B.3. Additional insights from Monte Carlo simulations



**Figure B.4:** Estimates and significance levels for an increase of +1 in the level of dependent events within 8 days after and 8 km from a treatment event for the case of large overlap.

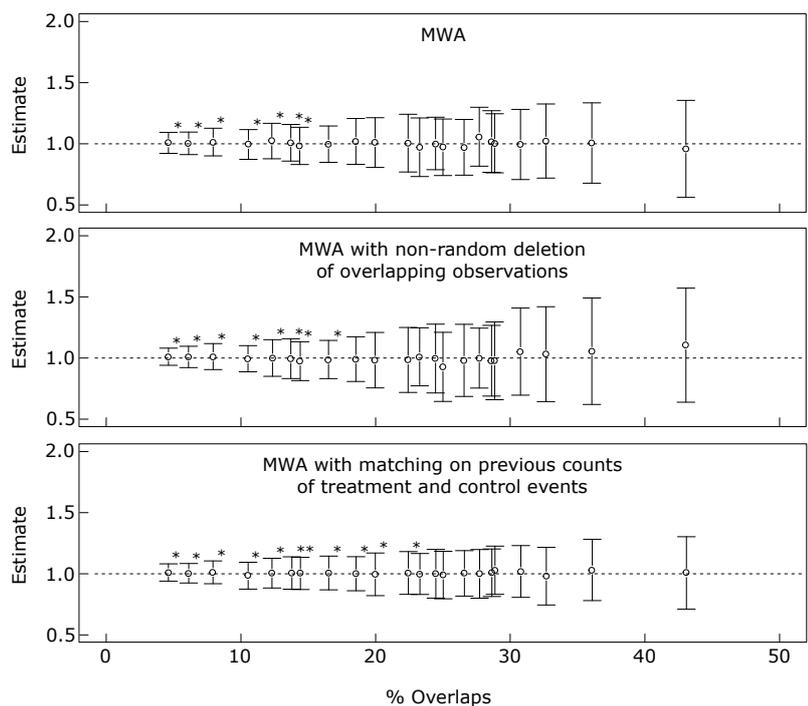
| Time (days) | Space (km) | Treatment effect | P-value | SO   | MO   |
|-------------|------------|------------------|---------|------|------|
| 8           | 8          | 0.95             | 0.02    | 0.28 | 0.20 |
| 8           | 10         | 1.05             | 0.00    | 0.37 | 0.28 |
| 10          | 8          | 1.00             | 0.01    | 0.32 | 0.20 |
| 10          | 10         | 0.99             | 0.01    | 0.43 | 0.29 |

**Table B.5:** Summary statistics for the interpretable areas of the contour plot in Figure B.4. SO (“same overlap”) refers to situations where either the cylinders of two or more treatment or two or more control events overlap. MO (“mixed overlap”) refers to situations where treatment and control cylinders overlap.

| Time (days) | Space (km) | Controls <sub>pre</sub>  | Treatments <sub>pre</sub>  | L1 <sub>pre</sub>  | %Support <sub>pre</sub>  |
|-------------|------------|--------------------------|----------------------------|--------------------|--------------------------|
| 8           | 8          | 200                      | 100                        | 0.80               | 13.30                    |
| 8           | 10         | 200                      | 100                        | 0.83               | 10.40                    |
| 10          | 8          | 200                      | 100                        | 0.80               | 12.70                    |
| 10          | 10         | 200                      | 100                        | 0.85               | 9.00                     |
| Time (days) | Space (km) | Controls <sub>post</sub> | Treatments <sub>post</sub> | L1 <sub>post</sub> | %Support <sub>post</sub> |
| 8           | 8          | 41                       | 30                         | 0.46               | 47.50                    |
| 8           | 10         | 33                       | 27                         | 0.48               | 47.10                    |
| 10          | 8          | 39                       | 31                         | 0.47               | 50.00                    |
| 10          | 10         | 37                       | 29                         | 0.58               | 41.20                    |

**Table B.6:** Matching statistics (top panel: before matching, bottom panel: after matching) for the interpretable areas of the contour plot in Figure B.4.

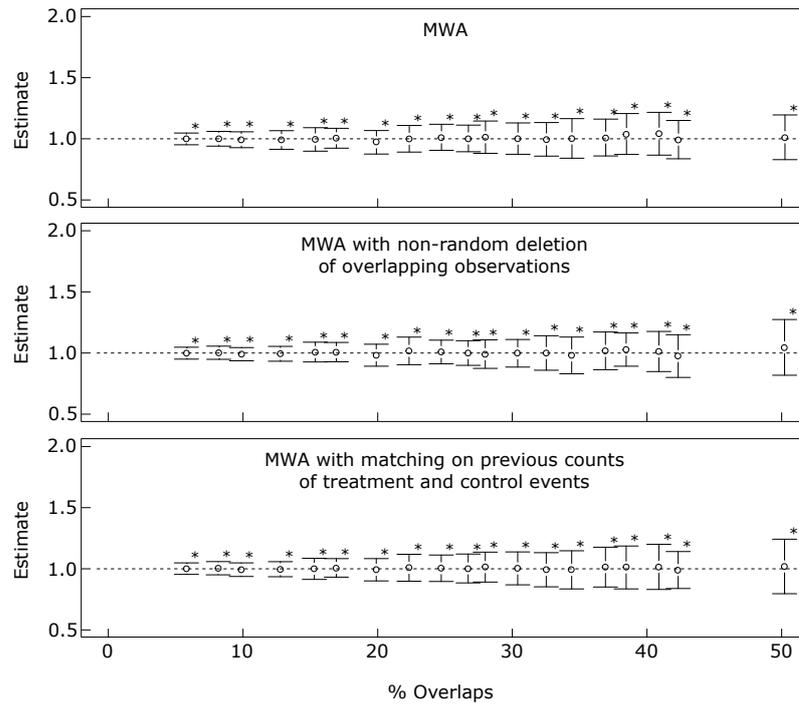
**Appendix B. Supplementary Information (SI): “Matched wake analysis”**



**Figure B.5:** Average estimates with 95% confidence intervals as a function of the overlaps of the spatiotemporal cylinders (overlaps measured in % MO). The graph shows estimates for MWA (top), MWA with non-random deletion of overlapping observations (middle), and MWA with matching on counts of previous treatment and control events (bottom). Asterisks indicate that all estimates for all simulated data sets were significant at the 0.05 level and the dotted line marks the true effect.

We have shown that matching on previous interventions generates robust estimates for the treatment effect even for samples with overlapping interventions. However, it would be premature to conclude that percentages of overlaps alone can be used to assess the reliability of the estimates. Beyond clustering, some data might also yield high numbers of dependent events that occur independently of the proposed reactive effect. In order to explore the effects of unrelated dependent events more systematically, we repeated the Monte Carlo simulations reported in Figure 4.5 of the article with different amounts of dependent events that were distributed independently of treatment and controls. We focused again on distances of 8 km and 8 days from the interventions where the true treatment effect is +1. As a first test, we repeated the analysis from the article with as little random variation in the counts of dependent events as possible. As Figure B.6 indicates, in this case there are virtually no differences between the performances of MWA, MWA with matching on previous interventions, and MWA with deletion of overlapping observations. All three methods perform similarly well even for situations with larger overlaps.

### B.3. Additional insights from Monte Carlo simulations

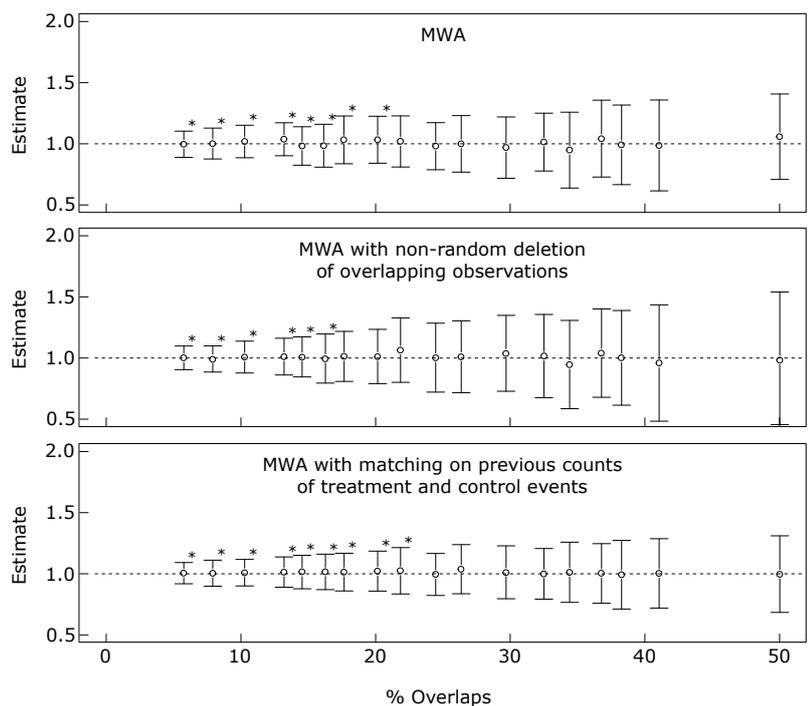


**Figure B.6:** Average estimates with 95% confidence intervals as a function of the overlaps of the spatiotemporal cylinders. The dataset was constructed to have a treatment effect of +1 with no unrelated dependent events. The graph shows estimates for MWA (top), MWA with non-random deletion of overlapping observations (middle), and MWA with matching on counts of previous treatment and control events (bottom). Asterisks indicate that the estimates for all simulated data sets at a given overlap were significant at the 0.05 level and the dotted line marks the true effect.

In a second series of simulations, we distributed an additional 2500 dependent events randomly in the simulated space. As Figure B.7 indicates, estimates for the treatment effect become less reliable for all levels of spatiotemporal overlaps. More importantly, the proposed remedy for correcting bias resulting from SUTVA violations performs best in this scenario: the lowest row shows the smallest variance for estimates treatment effects. However, an important take-home message from this analysis is that it is impossible to assess the reliability of the treatment estimates only by looking at the fraction of overlapping interventions. Additionally, goodness-of-fit statistics such as adjusted  $R^2$  of the DD regression should also be taken into account.

As an illustrative example, we focused on two simulated datasets with 28% overlap. Completely analogously to the simulation setups used for Figure B.6 and Figure B.7 respectively, the first one was created without unrelated dependent events and the second one featured 2500 additional dependent events that were unrelated to the interventions. In the case without unrelated dependent events we calculated an adjusted  $R^2$  of 0.526 on average for 100 simulation runs (with a std. dev. of 0.093). In comparison, in the example with higher levels of overlap, the adjusted  $R^2$  was only

**Appendix B. Supplementary Information (SI): “Matched wake analysis”**



**Figure B.7:** Average estimates with 95% confidence intervals as a function of the overlaps of the spatiotemporal cylinders. Data was generated with 2500 additional dependent events spread over 1 year. The graph shows estimates for MWA (top), MWA with non-random deletion of overlapping observations (middle), and MWA with matching on counts of previous treatment and control events (bottom). Asterisks indicate that the estimates for all simulated data sets at a given overlap were significant at the 0.05 level and the dotted line marks the true effect.

.356 (with a std. dev. of 0.119). Recall that in the first case all estimates are significant with tight confidence bounds whereas in the latter those bounds are much wider and not all estimates are significant (see Figure B.6 and B.7). This example thus clearly illustrates that the percentage of overlapping interventions (i.e. SUTVA violations) cannot be used as the only criterion for judging the reliability of the results. Signal-to-noise ratio in the data is similarly important. However, we have consistently found matching on previous interventions to be the best strategy to mitigate the effects of SUTVA violations across a wide spectrum of simulation runs.

## B.4 Supplementary Tables

| Time (days) | Space (km) | Treatment effect | P-value     | SO          | MO          |
|-------------|------------|------------------|-------------|-------------|-------------|
| 2           | 2          | 0.00             | 1.00        | 0.00        | 0.00        |
| 2           | 4          | 0.00             | 1.00        | 0.00        | 0.00        |
| 2           | 6          | 0.00             | 1.00        | 0.00        | 0.01        |
| 2           | 8          | 0.00             | 1.00        | 0.01        | 0.01        |
| 2           | 10         | 0.00             | 1.00        | 0.01        | 0.01        |
| 4           | 2          | 0.00             | 1.00        | 0.00        | 0.00        |
| 4           | 4          | 0.00             | 1.00        | 0.00        | 0.00        |
| 4           | 6          | 0.00             | 1.00        | 0.01        | 0.01        |
| 4           | 8          | 0.00             | 1.00        | 0.02        | 0.01        |
| 4           | 10         | 0.00             | 1.00        | 0.02        | 0.01        |
| 6           | 2          | 0.00             | 1.00        | 0.00        | 0.00        |
| 6           | 4          | 0.00             | 1.00        | 0.00        | 0.00        |
| 6           | 6          | 0.00             | 1.00        | 0.01        | 0.01        |
| 6           | 8          | 0.00             | 1.00        | 0.02        | 0.01        |
| 6           | 10         | 0.00             | 1.00        | 0.02        | 0.01        |
| 8           | 2          | 0.00             | 1.00        | 0.00        | 0.00        |
| 8           | 4          | 0.00             | 1.00        | 0.00        | 0.00        |
| 8           | 6          | 0.00             | 1.00        | 0.01        | 0.01        |
| <b>8</b>    | <b>8</b>   | <b>0.98</b>      | <b>0.00</b> | <b>0.02</b> | <b>0.01</b> |
| <b>8</b>    | <b>10</b>  | <b>0.97</b>      | <b>0.00</b> | <b>0.02</b> | <b>0.01</b> |
| 10          | 2          | 0.00             | 1.00        | 0.00        | 0.00        |
| 10          | 4          | 0.00             | 1.00        | 0.00        | 0.00        |
| 10          | 6          | 0.00             | 1.00        | 0.01        | 0.01        |
| <b>10</b>   | <b>8</b>   | <b>0.98</b>      | <b>0.00</b> | <b>0.02</b> | <b>0.01</b> |
| <b>10</b>   | <b>10</b>  | <b>0.96</b>      | <b>0.00</b> | <b>0.02</b> | <b>0.01</b> |

**Table B.7:** Full summary statistics for the contour plot of the simulated pattern shown in Figure 4.4 of the article (significant estimates in bold). SO (“same overlap”) refers to situations where either the cylinders of two or more treatment or two or more control events overlap. MO (“mixed overlap”) refers to situations where treatment and control cylinders overlap.

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| Time (days) | Space (km) | $C_{pre}$  | $T_{pre}$  | $L1_{pre}$  | $\%Sup_{pre}$ | $C_{post}$ | $T_{post}$ | $L1_{post}$ | $\%Sup_{post}$ |
|-------------|------------|------------|------------|-------------|---------------|------------|------------|-------------|----------------|
| 2           | 2          | 200        | 100        | 0.33        | 56.30         | 182        | 96         | 0.29        | 61.90          |
| 2           | 4          | 200        | 100        | 0.33        | 56.30         | 182        | 96         | 0.29        | 61.90          |
| 2           | 6          | 200        | 100        | 0.34        | 53.30         | 180        | 94         | 0.28        | 61.90          |
| 2           | 8          | 200        | 100        | 0.34        | 50.60         | 174        | 92         | 0.29        | 58.70          |
| 2           | 10         | 200        | 100        | 0.36        | 49.40         | 174        | 91         | 0.30        | 58.70          |
| 4           | 2          | 200        | 100        | 0.34        | 54.80         | 180        | 96         | 0.29        | 61.90          |
| 4           | 4          | 200        | 100        | 0.34        | 53.30         | 179        | 95         | 0.29        | 61.90          |
| 4           | 6          | 200        | 100        | 0.34        | 51.30         | 178        | 93         | 0.28        | 61.90          |
| 4           | 8          | 200        | 100        | 0.34        | 50.60         | 178        | 93         | 0.28        | 61.90          |
| 4           | 10         | 200        | 100        | 0.37        | 48.20         | 175        | 92         | 0.30        | 61.90          |
| 6           | 2          | 200        | 100        | 0.34        | 54.80         | 180        | 96         | 0.29        | 61.90          |
| 6           | 4          | 200        | 100        | 0.34        | 52.60         | 179        | 95         | 0.29        | 61.90          |
| 6           | 6          | 200        | 100        | 0.34        | 50.60         | 178        | 93         | 0.28        | 61.90          |
| 6           | 8          | 200        | 100        | 0.34        | 50.60         | 178        | 93         | 0.28        | 61.90          |
| 6           | 10         | 200        | 100        | 0.37        | 48.20         | 175        | 92         | 0.30        | 61.90          |
| 8           | 2          | 200        | 100        | 0.34        | 54.80         | 180        | 96         | 0.29        | 61.90          |
| 8           | 4          | 200        | 100        | 0.34        | 52.60         | 179        | 95         | 0.29        | 61.90          |
| 8           | 6          | 200        | 100        | 0.35        | 48.80         | 178        | 92         | 0.29        | 60.30          |
| <b>8</b>    | <b>8</b>   | <b>200</b> | <b>100</b> | <b>0.61</b> | <b>32.00</b>  | <b>121</b> | <b>71</b>  | <b>0.52</b> | <b>40.60</b>   |
| <b>8</b>    | <b>10</b>  | <b>200</b> | <b>100</b> | <b>0.63</b> | <b>29.30</b>  | <b>116</b> | <b>70</b>  | <b>0.54</b> | <b>37.50</b>   |
| 10          | 2          | 200        | 100        | 0.34        | 54.80         | 180        | 96         | 0.29        | 61.90          |
| 10          | 4          | 200        | 100        | 0.34        | 52.60         | 179        | 95         | 0.29        | 61.90          |
| 10          | 6          | 200        | 100        | 0.36        | 47.60         | 177        | 92         | 0.29        | 60.30          |
| <b>10</b>   | <b>8</b>   | <b>200</b> | <b>100</b> | <b>0.61</b> | <b>31.20</b>  | <b>120</b> | <b>71</b>  | <b>0.53</b> | <b>39.60</b>   |
| <b>10</b>   | <b>10</b>  | <b>200</b> | <b>100</b> | <b>0.62</b> | <b>30.10</b>  | <b>119</b> | <b>70</b>  | <b>0.53</b> | <b>38.10</b>   |

**Table B.8:** Full matching statistics for the simulated pattern analyzed in Figure 4.4 of the article (significant estimates in bold).  $C$ : control and  $T$ : treatment cases,  $\%Sup$ . denotes the percentage of common support and  $pre$  and  $post$  indicate statistics before and after matching.

## B.4. Supplementary Tables

| Time (days) | Space (km) | Treatment effect | P-value     | SO          | MO          |
|-------------|------------|------------------|-------------|-------------|-------------|
| 6           | 0.5        | 0.00             | 1.00        | 0.01        | 0.00        |
| 6           | 1.5        | 0.02             | 0.54        | 0.09        | 0.05        |
| 6           | 2.5        | 0.02             | 0.58        | 0.17        | 0.10        |
| 6           | 3.5        | 0.04             | 0.35        | 0.25        | 0.17        |
| 6           | 4.5        | -0.00            | 0.99        | 0.29        | 0.26        |
| 6           | 5.5        | -0.02            | 0.83        | 0.36        | 0.29        |
| 7           | 0.5        | 0.00             | 1.00        | 0.01        | 0.00        |
| 7           | 1.5        | 0.03             | 0.28        | 0.10        | 0.05        |
| 7           | 2.5        | 0.06             | 0.12        | 0.19        | 0.11        |
| 7           | 3.5        | 0.08             | 0.14        | 0.28        | 0.20        |
| 7           | 4.5        | 0.02             | 0.73        | 0.33        | 0.29        |
| 7           | 5.5        | -0.01            | 0.94        | 0.40        | 0.32        |
| 8           | 0.5        | 0.00             | 1.00        | 0.00        | 0.00        |
| 8           | 1.5        | 0.03             | 0.28        | 0.10        | 0.06        |
| <b>8</b>    | <b>2.5</b> | <b>0.08</b>      | <b>0.03</b> | <b>0.20</b> | <b>0.13</b> |
| 8           | 3.5        | 0.06             | 0.27        | 0.30        | 0.23        |
| 8           | 4.5        | 0.01             | 0.90        | 0.36        | 0.33        |
| 8           | 5.5        | 0.04             | 0.66        | 0.43        | 0.37        |
| 9           | 0.5        | 0.00             | 1.00        | 0.00        | 0.00        |
| 9           | 1.5        | 0.03             | 0.33        | 0.11        | 0.07        |
| 9           | 2.5        | 0.08             | 0.06        | 0.22        | 0.14        |
| 9           | 3.5        | 0.06             | 0.31        | 0.32        | 0.25        |
| 9           | 4.5        | 0.01             | 0.86        | 0.39        | 0.35        |
| 9           | 5.5        | 0.02             | 0.83        | 0.47        | 0.39        |
| 10          | 0.5        | 0.00             | 1.00        | 0.00        | 0.00        |
| 10          | 1.5        | 0.04             | 0.27        | 0.12        | 0.07        |
| <b>10</b>   | <b>2.5</b> | <b>0.09</b>      | <b>0.03</b> | <b>0.23</b> | <b>0.15</b> |
| 10          | 3.5        | 0.08             | 0.18        | 0.33        | 0.27        |
| 10          | 4.5        | -0.04            | 0.69        | 0.41        | 0.37        |
| 10          | 5.5        | 0.01             | 0.95        | 0.49        | 0.41        |
| 11          | 0.5        | 0.00             | 1.00        | 0.00        | 0.00        |
| 11          | 1.5        | 0.04             | 0.33        | 0.12        | 0.08        |
| <b>11</b>   | <b>2.5</b> | <b>0.11</b>      | <b>0.01</b> | <b>0.24</b> | <b>0.16</b> |
| 11          | 3.5        | 0.10             | 0.10        | 0.34        | 0.28        |
| 11          | 4.5        | 0.02             | 0.81        | 0.42        | 0.39        |
| 11          | 5.5        | 0.02             | 0.88        | 0.51        | 0.43        |
| 12          | 0.5        | 0.00             | 1.00        | 0.00        | 0.00        |
| 12          | 1.5        | 0.03             | 0.47        | 0.13        | 0.08        |
| <b>12</b>   | <b>2.5</b> | <b>0.14</b>      | <b>0.01</b> | <b>0.26</b> | <b>0.17</b> |
| 12          | 3.5        | 0.14             | 0.05        | 0.36        | 0.29        |
| 12          | 4.5        | 0.04             | 0.73        | 0.44        | 0.40        |
| 12          | 5.5        | 0.08             | 0.58        | 0.52        | 0.45        |
| 13          | 0.5        | 0.00             | 1.00        | 0.01        | 0.00        |
| 13          | 1.5        | 0.05             | 0.32        | 0.15        | 0.09        |
| <b>13</b>   | <b>2.5</b> | <b>0.13</b>      | <b>0.03</b> | <b>0.27</b> | <b>0.18</b> |
| <b>13</b>   | <b>3.5</b> | <b>0.16</b>      | <b>0.03</b> | <b>0.37</b> | <b>0.30</b> |
| 13          | 4.5        | 0.15             | 0.20        | 0.45        | 0.40        |
| 13          | 5.5        | 0.07             | 0.64        | 0.54        | 0.45        |
| 14          | 0.5        | 0.00             | 1.00        | 0.03        | 0.00        |
| 14          | 1.5        | 0.02             | 0.65        | 0.16        | 0.10        |
| 14          | 2.5        | 0.11             | 0.05        | 0.29        | 0.20        |
| 14          | 3.5        | 0.12             | 0.14        | 0.41        | 0.32        |
| 14          | 4.5        | 0.17             | 0.14        | 0.49        | 0.42        |
| 14          | 5.5        | 0.10             | 0.50        | 0.57        | 0.47        |

**Table B.9:** Full summary statistics for the Baghdad contour plot shown in Figure 4.7 of the article (significant estimates in bold). SO (“same overlap”) refers to situations where either the cylinders of two or more treatment or two or more control events overlap. MO (“mixed overlap”) refers to situations where treatment and control cylinders overlap.

**Appendix B. Supplementary Information (SI): “Matched wake analysis”**

| Time (days) | Space (km) | $C_{pre}$  | $T_{pre}$  | $L1_{pre}$  | $\%Sup_{pre}$ | $C_{post}$ | $T_{post}$ | $L1_{post}$ | $\%Sup_{post}$ |
|-------------|------------|------------|------------|-------------|---------------|------------|------------|-------------|----------------|
| 6           | 0.5        | 172        | 246        | 0.40        | 34.60         | 144        | 185        | 0.25        | 58.10          |
| 6           | 1.5        | 172        | 246        | 0.43        | 31.00         | 137        | 179        | 0.26        | 55.10          |
| 6           | 2.5        | 172        | 246        | 0.46        | 27.50         | 128        | 167        | 0.27        | 51.90          |
| 6           | 3.5        | 172        | 246        | 0.51        | 23.90         | 122        | 150        | 0.27        | 46.60          |
| 6           | 4.5        | 172        | 246        | 0.54        | 25.00         | 113        | 149        | 0.33        | 43.70          |
| 6           | 5.5        | 172        | 246        | 0.56        | 23.30         | 107        | 137        | 0.32        | 48.50          |
| 7           | 0.5        | 172        | 245        | 0.40        | 34.60         | 144        | 184        | 0.25        | 58.10          |
| 7           | 1.5        | 172        | 245        | 0.43        | 31.70         | 139        | 177        | 0.26        | 54.90          |
| 7           | 2.5        | 172        | 245        | 0.48        | 26.20         | 124        | 162        | 0.28        | 50.00          |
| 7           | 3.5        | 172        | 245        | 0.52        | 23.60         | 121        | 146        | 0.27        | 45.60          |
| 7           | 4.5        | 172        | 245        | 0.54        | 24.80         | 111        | 143        | 0.34        | 41.70          |
| 7           | 5.5        | 172        | 245        | 0.58        | 21.30         | 103        | 135        | 0.34        | 44.90          |
| 8           | 0.5        | 171        | 244        | 0.40        | 34.60         | 144        | 183        | 0.25        | 58.10          |
| 8           | 1.5        | 171        | 244        | 0.44        | 32.40         | 137        | 177        | 0.27        | 55.80          |
| <b>8</b>    | <b>2.5</b> | <b>171</b> | <b>244</b> | <b>0.50</b> | <b>26.20</b>  | <b>118</b> | <b>160</b> | <b>0.30</b> | <b>51.00</b>   |
| 8           | 3.5        | 171        | 244        | 0.54        | 23.50         | 121        | 143        | 0.30        | 44.30          |
| 8           | 4.5        | 171        | 244        | 0.57        | 24.30         | 112        | 142        | 0.35        | 45.80          |
| 8           | 5.5        | 171        | 244        | 0.61        | 21.70         | 104        | 124        | 0.36        | 45.10          |
| 9           | 0.5        | 171        | 243        | 0.40        | 34.60         | 144        | 182        | 0.25        | 58.10          |
| 9           | 1.5        | 171        | 243        | 0.45        | 29.80         | 132        | 174        | 0.27        | 54.00          |
| 9           | 2.5        | 171        | 243        | 0.50        | 27.80         | 117        | 162        | 0.31        | 53.80          |
| 9           | 3.5        | 171        | 243        | 0.55        | 22.60         | 122        | 142        | 0.31        | 45.90          |
| 9           | 4.5        | 171        | 243        | 0.59        | 23.00         | 113        | 135        | 0.38        | 45.20          |
| 9           | 5.5        | 171        | 243        | 0.65        | 19.10         | 105        | 119        | 0.39        | 45.10          |
| 10          | 0.5        | 171        | 242        | 0.40        | 34.60         | 144        | 181        | 0.25        | 58.10          |
| 10          | 1.5        | 171        | 242        | 0.45        | 30.20         | 132        | 169        | 0.27        | 54.00          |
| <b>10</b>   | <b>2.5</b> | <b>171</b> | <b>242</b> | <b>0.52</b> | <b>25.40</b>  | <b>118</b> | <b>153</b> | <b>0.32</b> | <b>51.90</b>   |
| 10          | 3.5        | 171        | 242        | 0.56        | 21.00         | 120        | 138        | 0.32        | 42.90          |
| 10          | 4.5        | 171        | 242        | 0.59        | 24.20         | 114        | 130        | 0.37        | 48.60          |
| 10          | 5.5        | 171        | 242        | 0.66        | 20.00         | 104        | 113        | 0.39        | 52.20          |
| 11          | 0.5        | 171        | 242        | 0.40        | 34.60         | 144        | 181        | 0.25        | 58.10          |
| 11          | 1.5        | 171        | 242        | 0.45        | 30.60         | 132        | 168        | 0.27        | 56.00          |
| <b>11</b>   | <b>2.5</b> | <b>171</b> | <b>242</b> | <b>0.52</b> | <b>27.40</b>  | <b>118</b> | <b>151</b> | <b>0.32</b> | <b>57.70</b>   |
| 11          | 3.5        | 171        | 242        | 0.56        | 22.00         | 117        | 134        | 0.31        | 48.30          |
| 11          | 4.5        | 171        | 242        | 0.60        | 24.60         | 113        | 124        | 0.38        | 52.20          |
| 11          | 5.5        | 171        | 242        | 0.68        | 18.40         | 95         | 108        | 0.41        | 51.60          |
| 12          | 0.5        | 170        | 242        | 0.40        | 34.60         | 143        | 181        | 0.25        | 58.10          |
| 12          | 1.5        | 170        | 242        | 0.47        | 30.00         | 128        | 165        | 0.28        | 57.10          |
| <b>12</b>   | <b>2.5</b> | <b>170</b> | <b>242</b> | <b>0.53</b> | <b>27.50</b>  | <b>118</b> | <b>149</b> | <b>0.33</b> | <b>57.10</b>   |
| 12          | 3.5        | 170        | 242        | 0.57        | 22.60         | 114        | 132        | 0.32        | 50.00          |
| 12          | 4.5        | 170        | 242        | 0.62        | 24.40         | 110        | 119        | 0.40        | 55.10          |
| 12          | 5.5        | 170        | 242        | 0.68        | 20.00         | 91         | 105        | 0.43        | 56.50          |
| 13          | 0.5        | 169        | 242        | 0.41        | 34.10         | 141        | 178        | 0.25        | 58.10          |
| 13          | 1.5        | 169        | 242        | 0.46        | 29.70         | 127        | 165        | 0.28        | 54.90          |
| <b>13</b>   | <b>2.5</b> | <b>169</b> | <b>242</b> | <b>0.53</b> | <b>25.70</b>  | <b>120</b> | <b>152</b> | <b>0.34</b> | <b>56.40</b>   |
| <b>13</b>   | <b>3.5</b> | <b>169</b> | <b>242</b> | <b>0.57</b> | <b>22.90</b>  | <b>116</b> | <b>128</b> | <b>0.32</b> | <b>50.00</b>   |
| 13          | 4.5        | 169        | 242        | 0.64        | 20.40         | 100        | 113        | 0.40        | 50.80          |
| 13          | 5.5        | 169        | 242        | 0.69        | 20.40         | 93         | 106        | 0.43        | 55.20          |
| 14          | 0.5        | 168        | 241        | 0.42        | 31.00         | 140        | 179        | 0.26        | 54.50          |
| 14          | 1.5        | 168        | 241        | 0.49        | 27.00         | 125        | 163        | 0.30        | 54.00          |
| 14          | 2.5        | 168        | 241        | 0.54        | 23.80         | 119        | 148        | 0.34        | 53.40          |
| 14          | 3.5        | 168        | 241        | 0.59        | 22.50         | 117        | 127        | 0.34        | 50.00          |
| 14          | 4.5        | 168        | 241        | 0.64        | 19.70         | 101        | 111        | 0.38        | 52.30          |
| 14          | 5.5        | 168        | 241        | 0.70        | 17.10         | 90         | 103        | 0.41        | 52.20          |

**Table B.10:** Full matching statistics for the Baghdad estimation shown in Figure 4.7 of the article (significant estimates in bold).  $C$ : control and  $T$ : treatment cases,  $\%Sup$ . denotes the percentage of common support and  $pre$  and  $post$  indicate statistics before and after matching.

## B.4. Supplementary Tables

| Time (days) | Space (km) | Treatment effect | P-value     | SO          | MO          |
|-------------|------------|------------------|-------------|-------------|-------------|
| 1           | 5          | 0.00             | 1.00        | 0.05        | 0.03        |
| 1           | 15         | 0.03             | 0.08        | 0.11        | 0.07        |
| 1           | 25         | 0.01             | 0.63        | 0.13        | 0.09        |
| 1           | 35         | -0.01            | 0.77        | 0.15        | 0.10        |
| <b>2</b>    | <b>5</b>   | <b>0.03</b>      | <b>0.02</b> | <b>0.08</b> | <b>0.07</b> |
| <b>2</b>    | <b>15</b>  | <b>0.05</b>      | <b>0.03</b> | <b>0.16</b> | <b>0.14</b> |
| <b>2</b>    | <b>25</b>  | <b>0.08</b>      | <b>0.01</b> | <b>0.19</b> | <b>0.17</b> |
| 2           | 35         | 0.03             | 0.35        | 0.23        | 0.20        |
| <b>3</b>    | <b>5</b>   | <b>0.03</b>      | <b>0.04</b> | <b>0.10</b> | <b>0.09</b> |
| 3           | 15         | 0.02             | 0.44        | 0.22        | 0.19        |
| 3           | 25         | 0.04             | 0.32        | 0.26        | 0.23        |
| 3           | 35         | 0.03             | 0.56        | 0.30        | 0.28        |
| 4           | 5          | 0.03             | 0.09        | 0.13        | 0.11        |
| 4           | 15         | -0.00            | 0.94        | 0.25        | 0.23        |
| 4           | 25         | -0.02            | 0.69        | 0.30        | 0.29        |
| 4           | 35         | -0.03            | 0.66        | 0.35        | 0.34        |
| 5           | 5          | 0.03             | 0.10        | 0.16        | 0.14        |
| 5           | 15         | 0.02             | 0.58        | 0.31        | 0.28        |
| 5           | 25         | -0.01            | 0.84        | 0.37        | 0.35        |
| 5           | 35         | 0.07             | 0.32        | 0.43        | 0.40        |
| 6           | 5          | 0.03             | 0.18        | 0.19        | 0.16        |
| 6           | 15         | -0.05            | 0.30        | 0.36        | 0.31        |
| 6           | 25         | -0.09            | 0.16        | 0.43        | 0.40        |
| 6           | 35         | -0.05            | 0.56        | 0.49        | 0.44        |
| 7           | 5          | 0.03             | 0.29        | 0.21        | 0.18        |
| 7           | 15         | -0.02            | 0.79        | 0.38        | 0.34        |
| 7           | 25         | -0.02            | 0.81        | 0.45        | 0.43        |
| 7           | 35         | 0.07             | 0.50        | 0.52        | 0.47        |

**Table B.11:** Full summary statistics for the Iraq contour plot shown in Figure B.1 (significant estimates in bold). SO (“same overlap”) refers to situations where either the cylinders of two or more treatment or two or more control events overlap. MO (“mixed overlap”) refers to situations where treatment and control cylinders overlap.

## Appendix B. Supplementary Information (SI): “Matched wake analysis”

| Time (days) | Space (km) | $C_{pre}$  | $T_{pre}$  | $L1_{pre}$  | $\%Sup_{pre}$ | $C_{post}$ | $T_{post}$ | $L1_{post}$ | $\%Sup_{post}$ |
|-------------|------------|------------|------------|-------------|---------------|------------|------------|-------------|----------------|
| 1           | 5          | 358        | 371        | 0.43        | 39.40         | 316        | 295        | 0.33        | 72.60          |
| 1           | 15         | 358        | 371        | 0.46        | 35.20         | 299        | 282        | 0.34        | 71.10          |
| 1           | 25         | 358        | 371        | 0.48        | 32.60         | 296        | 274        | 0.35        | 70.40          |
| 1           | 35         | 358        | 371        | 0.50        | 30.90         | 295        | 266        | 0.37        | 67.90          |
| <b>2</b>    | <b>5</b>   | <b>357</b> | <b>371</b> | <b>0.44</b> | <b>36.80</b>  | <b>312</b> | <b>288</b> | <b>0.33</b> | <b>70.90</b>   |
| <b>2</b>    | <b>15</b>  | <b>357</b> | <b>371</b> | <b>0.49</b> | <b>29.80</b>  | <b>288</b> | <b>270</b> | <b>0.34</b> | <b>68.70</b>   |
| <b>2</b>    | <b>25</b>  | <b>357</b> | <b>371</b> | <b>0.52</b> | <b>26.80</b>  | <b>276</b> | <b>260</b> | <b>0.36</b> | <b>65.50</b>   |
| 2           | 35         | 357        | 371        | 0.54        | 24.90         | 269        | 250        | 0.38        | 63.70          |
| <b>3</b>    | <b>5</b>   | <b>355</b> | <b>371</b> | <b>0.46</b> | <b>33.70</b>  | <b>306</b> | <b>276</b> | <b>0.33</b> | <b>70.50</b>   |
| 3           | 15         | 355        | 371        | 0.53        | 26.00         | 270        | 244        | 0.30        | 79.20          |
| 3           | 25         | 355        | 371        | 0.56        | 23.90         | 261        | 239        | 0.36        | 64.20          |
| 3           | 35         | 355        | 371        | 0.60        | 20.70         | 246        | 221        | 0.38        | 63.30          |
| 4           | 5          | 352        | 370        | 0.47        | 34.90         | 302        | 269        | 0.33        | 68.20          |
| 4           | 15         | 352        | 370        | 0.54        | 28.30         | 263        | 235        | 0.29        | 82.70          |
| 4           | 25         | 352        | 370        | 0.58        | 22.30         | 240        | 211        | 0.35        | 67.40          |
| 4           | 35         | 352        | 370        | 0.61        | 19.30         | 243        | 228        | 0.40        | 48.70          |
| 5           | 5          | 351        | 369        | 0.48        | 33.90         | 297        | 265        | 0.34        | 70.20          |
| 5           | 15         | 351        | 369        | 0.56        | 28.60         | 260        | 234        | 0.34        | 77.00          |
| 5           | 25         | 351        | 369        | 0.59        | 23.40         | 239        | 216        | 0.37        | 63.50          |
| 5           | 35         | 351        | 369        | 0.64        | 18.90         | 218        | 205        | 0.41        | 55.10          |
| 6           | 5          | 350        | 368        | 0.48        | 34.60         | 290        | 264        | 0.33        | 72.70          |
| 6           | 15         | 350        | 368        | 0.55        | 28.70         | 258        | 236        | 0.37        | 66.40          |
| 6           | 25         | 350        | 368        | 0.61        | 21.50         | 231        | 222        | 0.40        | 49.30          |
| 6           | 35         | 350        | 368        | 0.65        | 19.30         | 206        | 196        | 0.38        | 53.90          |
| 7           | 5          | 349        | 368        | 0.49        | 33.90         | 288        | 244        | 0.32        | 72.70          |
| 7           | 15         | 349        | 368        | 0.58        | 27.40         | 252        | 216        | 0.37        | 65.50          |
| 7           | 25         | 349        | 368        | 0.65        | 18.70         | 228        | 198        | 0.43        | 45.50          |
| 7           | 35         | 349        | 368        | 0.68        | 17.40         | 210        | 196        | 0.46        | 43.20          |

**Table B.12:** Full matching statistics for the Iraq estimation shown in Figure B.1 (significant estimates in bold).  $C$ : control and  $T$ : treatment cases,  $\%Sup$ . denotes the percentage of common support and  $pre$  and  $post$  indicate statistics before and after matching.

# C Supplementary Information (SI): “Views to a war”<sup>†</sup>

## C.1 Data

In our analysis we rely on detailed event data from two Iraq-specific datasets: Iraq Body Count (IBC), a web-based data collection initiative administered by Conflict Casualties Monitor Limited (London) (IBC, 2014), whose data can be accessed at <http://www.iraqbodycount.org>, and U.S. military (SIGACT) data downloaded from *The Guardian* website (Rogers, 2010a). In this section we outline details on data format and preparation.

The IBC database records violent events resulting in civilian deaths from January 1, 2003 onward with records updated continuously until the present day. Our analysis relies on the publicly available version of the IBC records that does not disaggregate by perpetrator group. Data used in this study was downloaded on November 15, 2011 and provides the following information on each incident: (i) a unique “IBC code”, (ii) “Start date” and “End date” of the incident, (iii) “Time” information (if known), given either as time of day with resolution of half an hour (e.g. 9:30 AM), or as time interval (9:00–10:00 AM) or as approximate time of the day (AM or PM). Each data entry also contains (iv) a verbal description of the “Location” (e.g. “al-Thaqafiyah, north of Mosul”), (v) information on the “Target” (e.g. “civilian car driven by mobile phone store owner”) and (vi) which “Weapon” (e.g. “magnetic bomb attached to car”) was used. The (vii) number of casualties is given as a range between “Reported minimum” and “Reported maximum”. Finally, IBC provides (viii) a “Source” field with the name of the news source(s) used to code the incident.

The IBC dataset contains a number of events with a one month interval between “Start Date” and “End Date”. Generally, the “End Date” of these entries falls on the last day of the month and the entries are usually recognizable as aggregate monthly casualty counts because the event location is coded, for example, as “19 Baghdad hospitals”. Though the number of civilian fatalities

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<sup>†</sup>This chapter is an edited version of the supplementary information for the following article: Karsten Donnay and Vladimir Filimonov. (2014). “Views to a war: systematic differences in media and military reporting of the war in Iraq.” Forthcoming in *EPJ Data Science*.

## Appendix C. Supplementary Information (SI): “Views to a war”

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reported in such aggregated counts can be quite large (up to several hundreds in early 2006–2008), we excluded them from our analysis because they do not code individual, recognizable conflict events. For the same reason we excluded all events in the IBC dataset where “Start Date” and “End Date” fields differ by more than a day. Note that this amounts to excluding less than 1.5% of all entries in our period of analysis.

In order to reliably extract the location information in the IBC dataset we used a comprehensive dictionary of locations in Iraq that codes hamlets, villages, city quarters etc. to the city or settlement in the direct geographic proximity. This, of course, also allows for an efficient extraction of the Baghdad subset that our analysis rests on. The automated dictionary-based routine recognizes over 99% of IBC locations—we then additionally ensured that none of the entries that could not be automatically location-coded corresponds to locations in Baghdad. As outlined in the article we further restricted our analysis to the period June 1, 2004 to February 28, 2009—a period covered by both datasets without any gaps. We provide this data in a .csv file that contains the “IBC code”, “Start Date”, “End Date”, “Time”, “Reported Minimum” and “Reported Maximum” of civilian casualties for each incident. In our analysis we did not use the “Time” information as it is only available for a small subset of events. All events therefore carry a “00:00” timestamp. Note further that, as detailed in the article, we used the “Reported Minimum” of casualties for our analysis since it is the more conservative estimate. Also, where “Start Date” and “End Date” of events differ we use “Start Date” to mark the timestamp of events. In section C.3 of this supplementary information we demonstrate that none of these coding choices affect our substantive findings.

The data made available through *The Guardian* contains information on all “significant actions” (SIGACTs) reported by units of the U.S. military in Iraq that resulted in at least one casualty. The dataset covers the period January 1, 2004 until December 31, 2009 but is missing 2 intervals of 1 month length each (from April 30, 2004 to June 1, 2004 and from February 28, 2009 to April 1, 2009) (Rogers, 2010a), which restricts our period of analysis to the period June 1, 2004 to February 28, 2009 (see also above). Data used in this study was downloaded on September 3, 2013 and provides the following information for each incident: (i) the “Report Key”, (ii) its “Date and time” with a resolution up to minutes, (iii) the “Type” of incident (e.g. “Explosive Hazard” or “Enemy Action”), (iv) a “Category” of events the incident is coded to (e.g. “Attack” or “Raid”), (v) the “Title” of the incident with detailed information on its occurrence, (vi) the military regional command or “Region” the incident was reported in, (vii) information on the target of the attack coded as “Attack on” either “NEUTRAL”, “ENEMY” or “FRIEND”, (viii) casualty counts—both *killed-in-action* (KIA) and *wounded-in-action* (WIA)—disaggregated by “Coalition forces”, “Iraq forces”, “Civilians” and “Enemy”, (ix) the total number of casualties and (x) the longitude and latitude of where the incident was reported. These geo-coordinates are truncated at a tenth of a degree (about 10 km) for Iraq outside of Baghdad and at a hundredth of a degree (about 1 km) for the military zone of Baghdad. In order to be able to compare it to IBC we restricted the SIGACT data to entries pertaining to deadly violence directed at civilians. As outlined in the article, focusing only on civilian casualties rather than also including incidents that wounded civilians may lead to a biased view of the violence dynamics. To control for this, we

performed robustness checks in which we additionally included the number of wounded civilians reported in SIGACT. These results are provided in section C.3 of this supplementary information demonstrating that this does not affect our substantive conclusions.

In selecting for events in the Baghdad area we rely on two different criteria outlined in the article. On the one hand we use the U.S. military’s definition of the greater Baghdad area and the corresponding regional command “MND-BAGHDAD”. We also performed each of our analysis for subdatasets generated by selecting all events that fall within a radius of 20 km, 30 km and 40 km from the city center (LON 44.422, LAT 33.325). These four dataset are provided in separate .csv files that contain the “Report key”, “Date”, “Latitude”, “Longitude”, “Region”, “Coalition forces wia”, “Coalition forces kia”, “Iraq forces wia”, “Iraq forces kia”, “Civilian wia”, “Civilian kia”, “Enemy wia” and “Enemy kia” for each incident. Notice that any detailed information on the type of event, target and details on the incident have been intentionally removed from these data.

Note that SIGACT data on Iraq was already published at the time we downloaded the corresponding IBC records. In principal, IBC records may thus have been updated and/or added based on these new informations. In fact, IBC did analyze the correspondence of the casualty records with SIGACT data in detail in 2010 (see <http://www.iraqbodycount.org/analysis/numbers/warlogs/>). If SIGACT information did indeed enter the IBC database it at best led to a better correspondence of the two datasets and at most our comparative analysis may thus provide a more conservative estimate of the original reporting differences.

## C.2 Event matching algorithm

In section 5.3.3 of the article we compare the day-by-day match of SIGACT to IBC events using an automated event matching algorithm. Note that we group events with a given casualty count ( $s$ ) in broad categories and then match each category independently. Specifically, we consider the following categories:  $S_1 = \{1\}$ ,  $S_2 = \{2,3\}$ ,  $S_3 = \{4,5,6\}$ ,  $S_4 = \{7,8,9,10\}$ ,  $S_5 = \{11,12,\dots,19,20\}$  and  $S_6 = \{21,22,\dots\}$ .

Given that the resolution of IBC is days, i.e., events all carry the timestamp “00:00”, we also round SIGACT to daily resolution for this comparison. The matching algorithm then proceeds as follows. For each SIGACT event at date  $t_{SIGACT}$  in given category  $S$ , we select all IBC events within the same size category and with dates in the range  $t_{SIGACT} - w + 1 \leq t_{IBC} \leq t_{SIGACT} + w$ , where  $w$  is the allowed tolerance in days.  $w = 1$  then selects only IBC entries that are recorded on the same calendar day as the SIGACT event. For  $w = 2$  we consider all events on the same day and on the previous and subsequent day, i.e.,  $\pm 1$  days timestamp uncertainty. Similarly,  $w = 3$  allows  $\pm 2$  days of uncertainty, etc. Among these possible matches, we then randomly select one IBC event (without replacement) and mark the original SIGACT event as “matched” in our records. This procedure is repeated for the next unmatched event in the SIGACT database wherein only previously unmatched IBC events are considered (because we selected without

## Appendix C. Supplementary Information (SI): “Views to a war”

| Casualties  | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|-------------|-------------------|-------|-------|---------|-------|-------|
|             | matched           | total | %     | matched | total | %     |
| $s = 1$     | 1166              | 1473  | 79.15 | 2890    | 11871 | 24.34 |
| $s = 2, 3$  | 278               | 417   | 66.66 | 1479    | 3054  | 48.42 |
| $s = 4-6$   | 75                | 133   | 56.39 | 420     | 693   | 60.60 |
| $s = 7-10$  | 17                | 45    | 37.77 | 125     | 202   | 61.88 |
| $s = 11-20$ | 16                | 36    | 44.44 | 69      | 143   | 48.25 |
| $s > 20$    | 15                | 23    | 65.21 | 47      | 67    | 70.14 |

**Table C.1:** Number of SIGACT reports matched to IBC entries,  $w = 1$

| Casualties  | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|-------------|-------------------|-------|-------|---------|-------|-------|
|             | matched           | total | %     | matched | total | %     |
| $s = 1$     | 1316              | 1473  | 89.34 | 2942    | 11871 | 24.78 |
| $s = 2, 3$  | 375               | 417   | 89.92 | 1579    | 3054  | 51.70 |
| $s = 4-6$   | 97                | 133   | 72.93 | 518     | 693   | 74.74 |
| $s = 7-10$  | 28                | 45    | 62.22 | 161     | 202   | 79.70 |
| $s = 11-20$ | 18                | 36    | 50.00 | 97      | 143   | 67.83 |
| $s > 20$    | 15                | 23    | 65.21 | 61      | 67    | 91.04 |

**Table C.2:** Number of SIGACT reports matched to IBC entries,  $w = 4$

replacement).

Once all SIGACT events are processed, we count the number of events per month that could be successfully matched. In order to avoid possible suboptimal solutions through our random “matching” algorithm, we use a Monte-Carlo approach: we simply repeat the random matching procedure 100 times and then select the best match achieved. The method is significantly faster than considering all possible combinations, and at the same time provides similar results. For larger windows  $w$  we, of course, expect to obtain a better match. For the article we considered  $w = 2$ , which most closely corresponds to the manual matching prescription used in a study performed at Columbia University (Carpenter et al., 2013) where IBC events were matched to SIGACT entries within 24h prior and 48h following the IBC event. Note that we also alternatively centered our search for matches on SIGACT instead of IBC entries using the full SIGACT timestamp. We find that this has no systematic effect on the quantitative results.

The results for  $w = 2$  are discussed in the article. Table C.1 summarizes the results of matching SIGACT events to IBC using  $w = 1$ , i.e., only considering events reported on the same date. Table C.2 presents results for  $w = 4$ , which allows  $\pm 3$  days of uncertainty in timestamps. Decreasing the timestamp tolerance significantly decreases the number of events that can be matched, while increasing it improves the quantitative match, as expected. Interestingly, for extreme events ( $s > 20$ ) in 2004–2005 and 2008–2009 and for very small events ( $s = 1$ ) during the escalation of the conflict in 2006–2007, the quality of matching remains almost unchanged for different timestamp uncertainties.

| Casualties | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|------------|-------------------|-------|-------|---------|-------|-------|
|            | matched           | total | %     | matched | total | %     |
| s = 1      | 1225              | 1757  | 69.72 | 2921    | 2974  | 98.21 |
| s = 2, 3   | 314               | 630   | 49.84 | 1544    | 2019  | 76.47 |
| s = 4–6    | 83                | 202   | 41.08 | 456     | 680   | 67.05 |
| s = 7–10   | 18                | 74    | 24.32 | 134     | 257   | 52.14 |
| s = 11–20  | 18                | 59    | 30.50 | 76      | 167   | 45.50 |
| s > 20     | 15                | 34    | 44.11 | 54      | 151   | 35.76 |

**Table C.3:** Number of IBC entries matched to SIGACT reports,  $w = 2$

Note that the matching results reported thus far are always expressed as the fraction of SIGACT reports. The analysis in the article, however, suggests that especially for large events IBC reports significantly more events than SIGACT. We have thus also considered the matches for  $w = 2$  expressed as fraction of IBC entries (Table C.3). Note that we here correspondingly centered our search on IBC rather than SIGACT events. The high match of IBC entries with few casualties and the low match of IBC entries with many casualties in the period 2006–2007, simply reflects the fact that IBC reports substantially less small events and more large events than SIGACT respectively. The generally lower match in the other periods simply reflects the fact that there IBC overall reports more events than SIGACT.

### C.3 Sensitivity Checks

We performed extensive sensitivity checks in order to guarantee that the substantial findings reported in the article do not depend on particular coding choices. Wherever applicable we report the results for each of the following variations of our data (see section C.1 of this supplementary information for details):

- (a) instead of the start date of an event in IBC we use its end date as timestamp (if these are different)
- (b) instead of the lower IBC casualty estimate we use the upper casualty estimate
- (c) instead of civilian KIA we consider civilian KIA + WIA in the SIGACT dataset
- (d) instead of “SIGACT Baghdad” we use “SIGACT 20km”, “SIGACT 30km” or “SIGACT 40km”, i.e., the datasets that cover all events in a 20, 30 or 40 km radius around Baghdad.

The sensitivity checks are grouped according to the corresponding figures and tables in the article. Note that we only report tables or figures for results that differ noticeably from those presented in the article.

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**Table 2**

In Table 2 of the article we show a detailed comparison of the total number of events in IBC and SIGACT and used a two-sample Anderson-Darling test to evaluate their quantitative agreement. The results in Table C.4 and C.5 confirm that for data variations (b) and (c) the pairwise comparison of the distribution of casualties in SIGACT and IBC does not differ substantially from those reported in the article. For large events (threshold of 40 and more casualties) we find a slightly improved distributional agreement for (c), simply because SIGACT KIA + WIA contains more events with many casualties than SIGACT KIA. Data variation (a) does not affect the aggregate statistics and (d) is already accounted for in the table.

| Threshold | Number of events |       |       |       |       | $A^2$ statistic |             |             |             |
|-----------|------------------|-------|-------|-------|-------|-----------------|-------------|-------------|-------------|
|           | (i)              | (ii)  | (iii) | (iv)  | (v)   | (i)-(ii)        | (i)-(iii)   | (i)-(iv)    | (i)-(v)     |
| 1         | 9068             | 18157 | 17533 | 18548 | 19369 | 1275.05         | 1279.05     | 1273.51     | 1268.11     |
| 2         | 4442             | 4813  | 4611  | 4940  | 5201  | 126.03          | 122.69      | 130.00      | 128.16      |
| 5         | 1284             | 876   | 851   | 901   | 952   | 8.25            | 8.87        | 9.73        | 9.81        |
| 10        | 548              | 323   | 310   | 325   | 340   | 7.20            | 6.71        | 6.60        | 6.69        |
| 15        | 335              | 159   | 154   | 161   | 169   | <b>1.10</b>     | <b>1.04</b> | <b>0.86</b> | <b>1.10</b> |
| 20        | 227              | 105   | 100   | 105   | 108   | <b>1.61</b>     | <b>1.20</b> | <b>1.03</b> | <b>0.98</b> |
| 25        | 173              | 77    | 75    | 79    | 82    | <b>2.30</b>     | <b>2.05</b> | <b>1.80</b> | <b>1.87</b> |
| 30        | 135              | 47    | 47    | 51    | 52    | <b>1.54</b>     | <b>1.54</b> | <b>1.37</b> | <b>1.39</b> |
| 40        | 79               | 29    | 29    | 31    | 32    | <b>2.41</b>     | <b>2.41</b> | <b>2.60</b> | <b>2.46</b> |

**Table C.4:** Results of the pairwise comparison of the distributions of casualties. The datasets are (i) “IBC Baghdad”, (ii) “SIGACT Baghdad”, (iii) “SIGACT 20km”, (iv) “SIGACT 30km” and (v) “SIGACT 40km”. We used a two-sample Anderson-Darling tests (adjusted for ties) for comparison (see the caption for Table 2 of the article for details), data variation (b)

| Threshold | Number of events |       |       |       |       | $A^2$ statistic |             |             |             |
|-----------|------------------|-------|-------|-------|-------|-----------------|-------------|-------------|-------------|
|           | (i)              | (ii)  | (iii) | (iv)  | (v)   | (i)-(ii)        | (i)-(iii)   | (i)-(iv)    | (i)-(v)     |
| 1         | 9004             | 18504 | 17854 | 18919 | 19782 | 359.92          | 381.87      | 355.70      | 328.97      |
| 2         | 4273             | 6313  | 6013  | 6477  | 6877  | 9.49            | 9.80        | 8.97        | 9.49        |
| 5         | 1163             | 1880  | 1795  | 1922  | 2052  | 27.87           | 29.26       | 25.90       | 24.07       |
| 10        | 484              | 992   | 957   | 1010  | 1067  | 8.70            | 9.18        | 8.27        | 7.03        |
| 15        | 296              | 675   | 653   | 682   | 715   | 3.81            | 4.08        | 3.91        | 3.16        |
| 20        | 206              | 503   | 490   | 509   | 526   | <b>1.44</b>     | <b>1.35</b> | <b>1.47</b> | <b>1.32</b> |
| 25        | 159              | 392   | 382   | 394   | 406   | <b>1.34</b>     | <b>1.25</b> | <b>1.42</b> | <b>1.33</b> |
| 30        | 123              | 294   | 287   | 299   | 307   | <b>2.59</b>     | <b>2.34</b> | <b>2.45</b> | <b>2.37</b> |
| 40        | 69               | 175   | 168   | 176   | 180   | 3.82            | 3.89        | 4.14        | 4.04        |

**Table C.5:** Results of the pairwise comparison of the distributions of casualties. The datasets are (i) “IBC Baghdad”, (ii) “SIGACT Baghdad”, (iii) “SIGACT 20km”, (iv) “SIGACT 30km” and (v) “SIGACT 40km”. We used a two-sample Anderson-Darling tests (adjusted for ties) for comparison (see the caption for Table 2 of the article for details), data variation (c)

**Table 4**

The results in Table C.6 to C.11 confirm that the day-by-day correspondence of IBC and SIGACT (Table 4 of the article) does not critically depend on data variations (a), (b) and (d). However considering both KIA and WIA events in SIGACT (variation (c)), results in a slight improvement in the day-by-day match of small events ( $s = 1$ ) and at the same time significantly decreases the match for large events ( $s > 7$ ) compared to the analysis reported in Table 4 of the article. Considering KIA+WIA thus does not make IBC and SIGACT more consistent.

| Casualties  | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|-------------|-------------------|-------|-------|---------|-------|-------|
|             | matched           | total | %     | matched | total | %     |
| $s = 1$     | 1263              | 1473  | 85.74 | 2921    | 11871 | 24.60 |
| $s = 2, 3$  | 337               | 417   | 80.81 | 1558    | 3054  | 51.01 |
| $s = 4-6$   | 83                | 133   | 62.40 | 486     | 693   | 70.12 |
| $s = 7-10$  | 22                | 45    | 48.88 | 148     | 202   | 73.26 |
| $s = 11-20$ | 18                | 36    | 50.00 | 82      | 143   | 57.34 |
| $s > 20$    | 15                | 23    | 65.21 | 55      | 67    | 82.08 |

**Table C.6:** Number of SIGACT reports matched to IBC entries, data variation (a),  $w = 2$

| Casualties  | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|-------------|-------------------|-------|-------|---------|-------|-------|
|             | matched           | total | %     | matched | total | %     |
| $s = 1$     | 1248              | 1473  | 84.72 | 2857    | 11871 | 24.06 |
| $s = 2, 3$  | 341               | 417   | 81.77 | 1578    | 3054  | 51.66 |
| $s = 4-6$   | 87                | 133   | 65.41 | 487     | 693   | 70.27 |
| $s = 7-10$  | 25                | 45    | 55.55 | 150     | 202   | 74.25 |
| $s = 11-20$ | 21                | 36    | 58.33 | 91      | 143   | 63.63 |
| $s > 20$    | 16                | 23    | 69.56 | 57      | 67    | 85.07 |

**Table C.7:** Number of SIGACT reports matched to IBC entries, data variation (b),  $w = 2$

| Casualties  | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|-------------|-------------------|-------|-------|---------|-------|-------|
|             | matched           | total | %     | matched | total | %     |
| $s = 1$     | 1039              | 1135  | 91.54 | 2883    | 11056 | 26.07 |
| $s = 2, 3$  | 396               | 546   | 72.52 | 1721    | 3298  | 52.18 |
| $s = 4-6$   | 113               | 251   | 45.01 | 522     | 848   | 61.55 |
| $s = 7-10$  | 30                | 115   | 26.08 | 168     | 343   | 48.97 |
| $s = 11-20$ | 29                | 118   | 24.57 | 125     | 317   | 39.43 |
| $s > 20$    | 28                | 127   | 22.04 | 126     | 350   | 36.00 |

**Table C.8:** Number of SIGACT reports matched to IBC entries, data variation (c),  $w = 2$

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| Casualties | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|------------|-------------------|-------|-------|---------|-------|-------|
|            | matched           | total | %     | matched | total | %     |
| s = 1      | 1185              | 1320  | 89.77 | 2912    | 11602 | 25.09 |
| s = 2, 3   | 314               | 377   | 83.28 | 1547    | 2944  | 52.54 |
| s = 4–6    | 80                | 120   | 66.66 | 472     | 671   | 70.34 |
| s = 7–10   | 21                | 42    | 50.00 | 144     | 200   | 72.00 |
| s = 11–20  | 19                | 35    | 54.28 | 80      | 137   | 58.39 |
| s > 20     | 15                | 21    | 71.42 | 52      | 64    | 81.25 |

**Table C.9:** Number of SIGACT reports matched to IBC entries, data variation (d), 20km,  $w = 2$

| Casualties | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|------------|-------------------|-------|-------|---------|-------|-------|
|            | matched           | total | %     | matched | total | %     |
| s = 1      | 1274              | 1488  | 85.61 | 2924    | 12120 | 24.12 |
| s = 2, 3   | 348               | 427   | 81.49 | 1576    | 3139  | 50.20 |
| s = 4–6    | 86                | 130   | 66.15 | 487     | 719   | 67.73 |
| s = 7–10   | 21                | 44    | 47.72 | 150     | 210   | 71.42 |
| s = 11–20  | 19                | 37    | 51.35 | 85      | 144   | 59.02 |
| s > 20     | 16                | 24    | 66.66 | 54      | 66    | 81.81 |

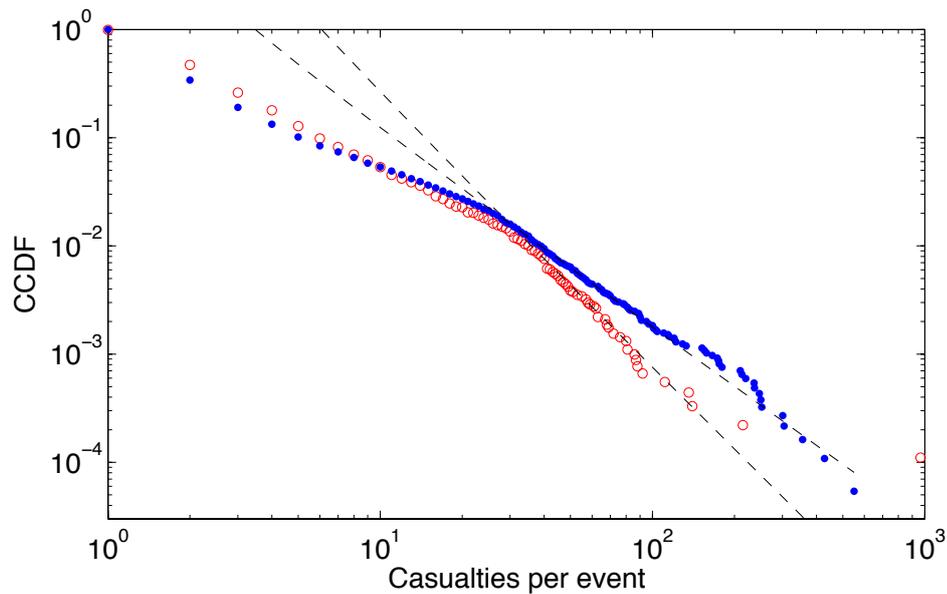
**Table C.10:** Number of SIGACT reports matched to IBC entries, data variation (d), 30km,  $w = 2$

| Casualties | 2004-05 & 2008-09 |       |       | 2006-07 |       |       |
|------------|-------------------|-------|-------|---------|-------|-------|
|            | matched           | total | %     | matched | total | %     |
| s = 1      | 1345              | 1626  | 82.71 | 2932    | 12542 | 23.37 |
| s = 2, 3   | 376               | 470   | 80.00 | 1612    | 3275  | 49.22 |
| s = 4–6    | 93                | 148   | 62.83 | 495     | 749   | 66.08 |
| s = 7–10   | 23                | 51    | 45.09 | 157     | 223   | 70.40 |
| s = 11–20  | 20                | 40    | 50.00 | 88      | 152   | 57.89 |
| s > 20     | 16                | 25    | 64.00 | 56      | 68    | 82.35 |

**Table C.11:** Number of SIGACT reports matched to IBC entries, data variation (d), 40km,  $w = 2$

### Figure 3

Data variation (a) has by definition no influence on the aggregate casualty statistics, and (b) and (d) do not result in significant changes to Figure 3 of the article. We would expect variation (c) to affect the overall casualty statistics in SIGACT though, most notably because it significantly increases casualty counts for many events. Figure C.1 confirms that KIA + WIA casualty counts do not feature the same robust power law scaling as reported in Figure 3 of the article and, qualitatively, the shape of the ccdf is more similar to that of IBC. However, the visual similarity is somewhat misleading: the Anderson-Darling tests robustly rejects the null hypothesis of agreement for all thresholds between 20 and 40 casualties per event (see also Table C.5). Note further that the tail behavior is also considerably different: the dashed lines correspond to power



**Figure C.1:** Complementary cumulative distribution function (ranking plot) of number of casualties in the “IBC Baghdad” (red circles) and “SIGACT Baghdad” (blue dots) datasets. Dashed lines correspond to power law fits using maximum likelihood estimation (also see the text of the article), data variation (c).

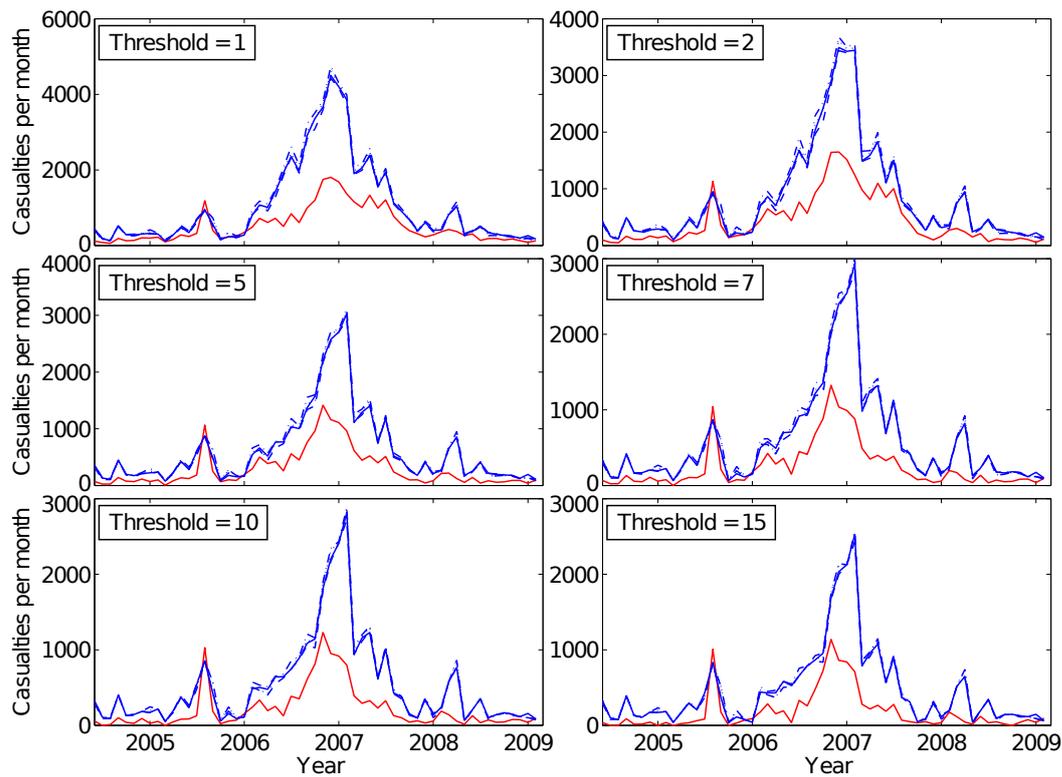
law fits to the tail of the data with exponents of 3.5 for IBC and 2.79 for SIGACT.

#### Figure 4

Variations (a) and (b) do not result in significant changes to Figure 4 of the article and variation (d) is already accounted for in the figure. Considering civilian KIA + WIA events in SIGACT, we find that the dynamics of the number of casualties per month more significantly differs from the IBC datasets for all thresholds (see Figure C.2) compared to the dynamics reported in the article. In fact, other than in Figure 4(b) where the number of casualties per month agreed for a threshold of 2 and IBC reported more casualties per month than SIGACT for all larger thresholds, we here find that SIGACT always reports more casualties than IBC. Using KIA + WIA counts thus certainly does not render IBC and SIGACT more consistent.

#### Figure 5

Data variations (a) and (d) do not result in significant changes to Figure 5 of the article. However, relying on the upper casualty estimates in the IBC dataset (data variation (b)) or KIA + WIA casualty counts in the SIGACT dataset (data variation (c))—or also both data variations taken together—generally decreases the agreement between the dynamics of the number of events per day in IBC and SIGACT. This is visible both in the *RMS* difference and the results of the Anderson-Darling tests, especially for large thresholds (see Figures C.3 and C.4).



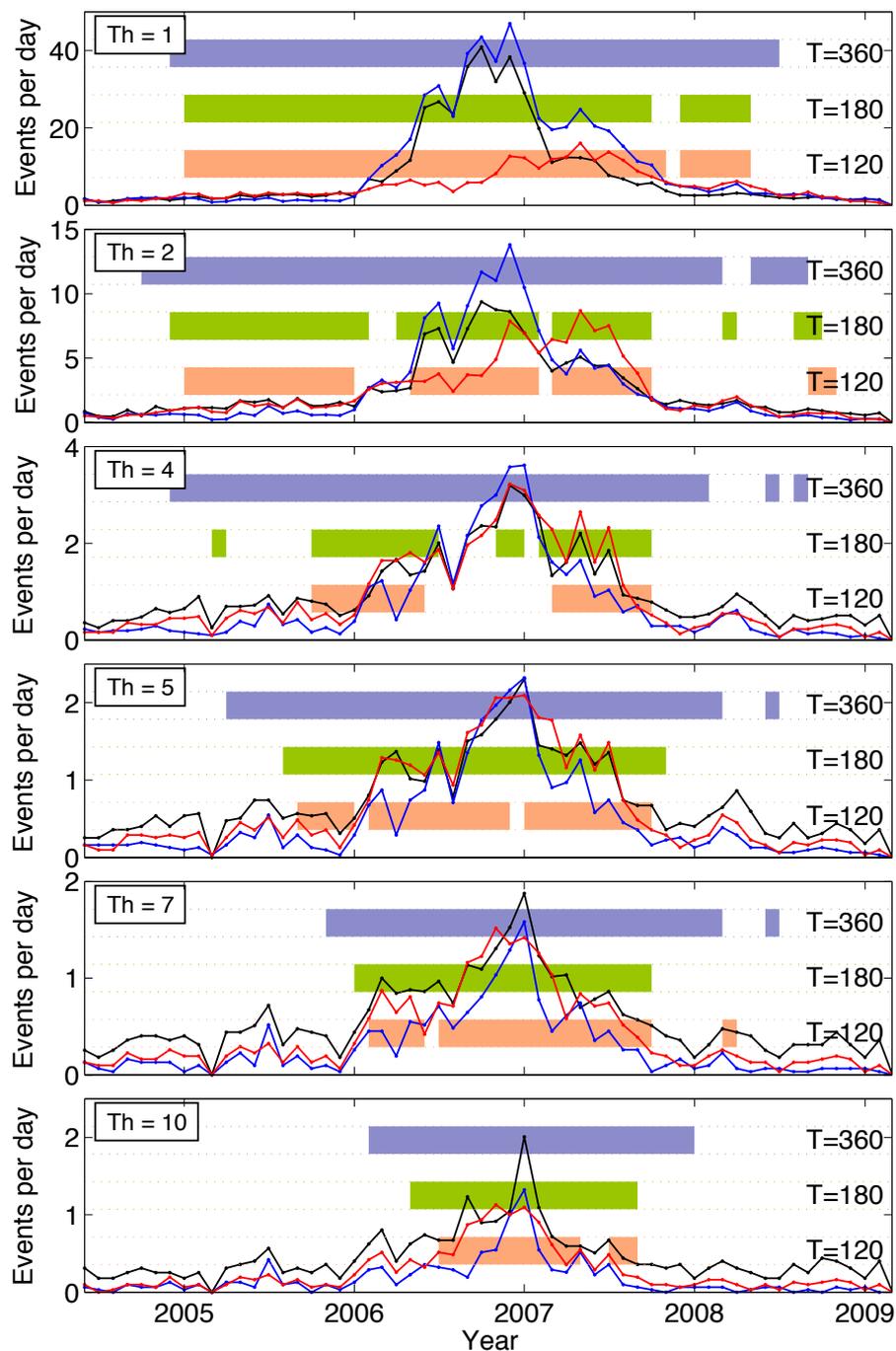
**Figure C.2:** Dynamics of the number of casualties per months in “IBC Baghdad” (red line), “SIGACT Baghdad” (solid blue line), “SIGACT 20km” (dashed blue line), “SIGACT 30km” (dotted blue line) and “SIGACT 40km” (dash-dotted blue line). The panels correspond to subsets of events for thresholds of 1, 2, 5, 7, 10 and 15 casualties respectively. Note that the plots for the different SIGACT datasets (blue lines) are almost indistinguishable. Data variation (c).

### Figure 7

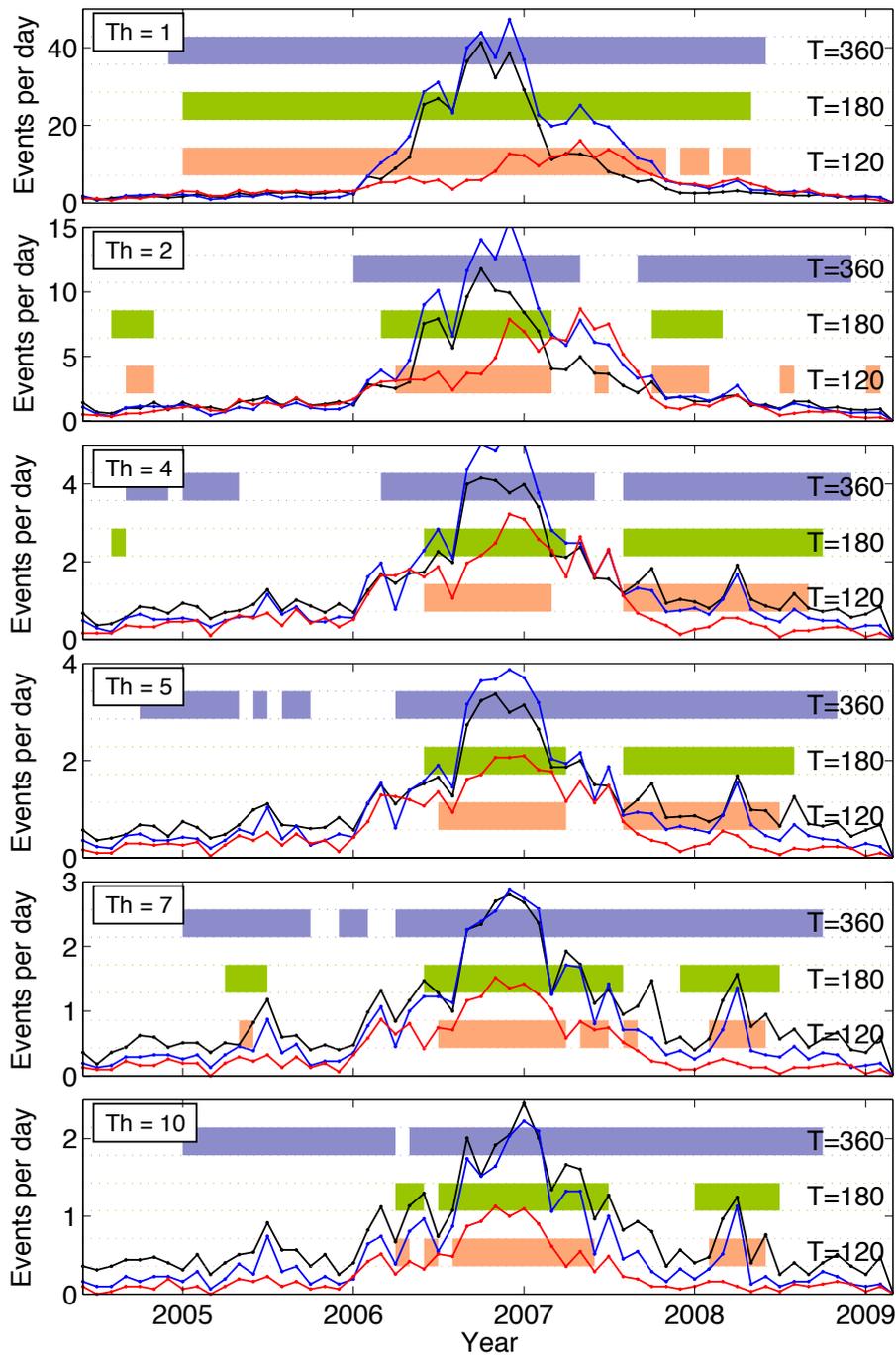
Data variations (a), (b) and (d) do not result in significant changes to Figure 7 of the main article. Data variation (c), i.e., considering KIA + WIA casualties in the SIGACT dataset, almost insignificantly increase the number of small events ( $s = 1$ ) in the SIGACT dataset that can be matched to events with the same number of casualties within  $\pm 1$  day in the IBC dataset. At the same time, however, it significantly decreases the fraction of large events matched (Figure C.5).

### Figure 8

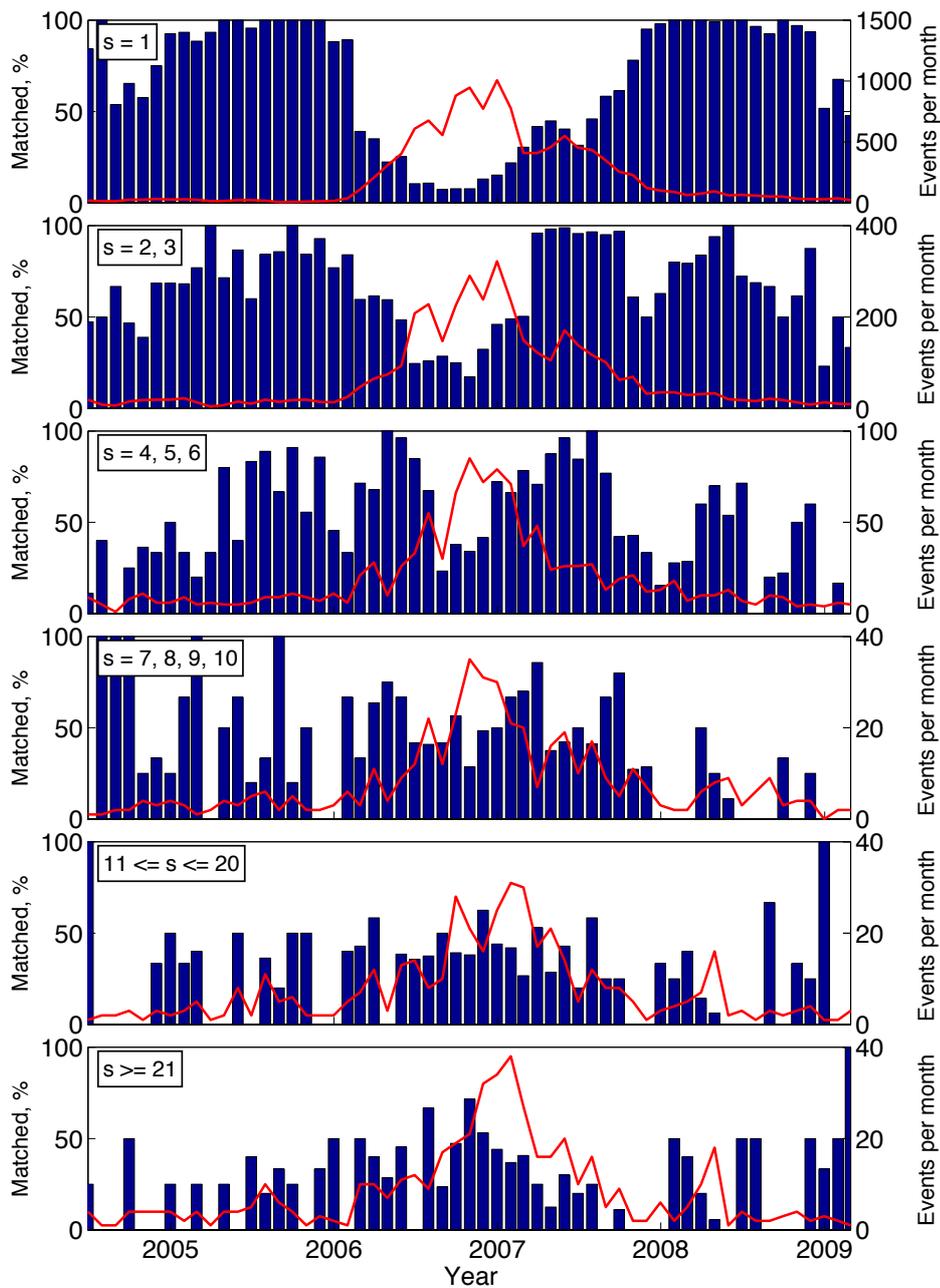
The results reported in Figure 8 of the article are not significantly affected by data variations (a), (b) and (d). However, considering KIA + WIA casualty counts results in an increase of the non-trivial timing structure in the SIGACT dataset. In Figure C.6 this is reflected in the fact that the null hypothesis of the Poisson (i.e., trivial random) dynamics can be rejected over much broader period of analysis, in particularly for large thresholds.



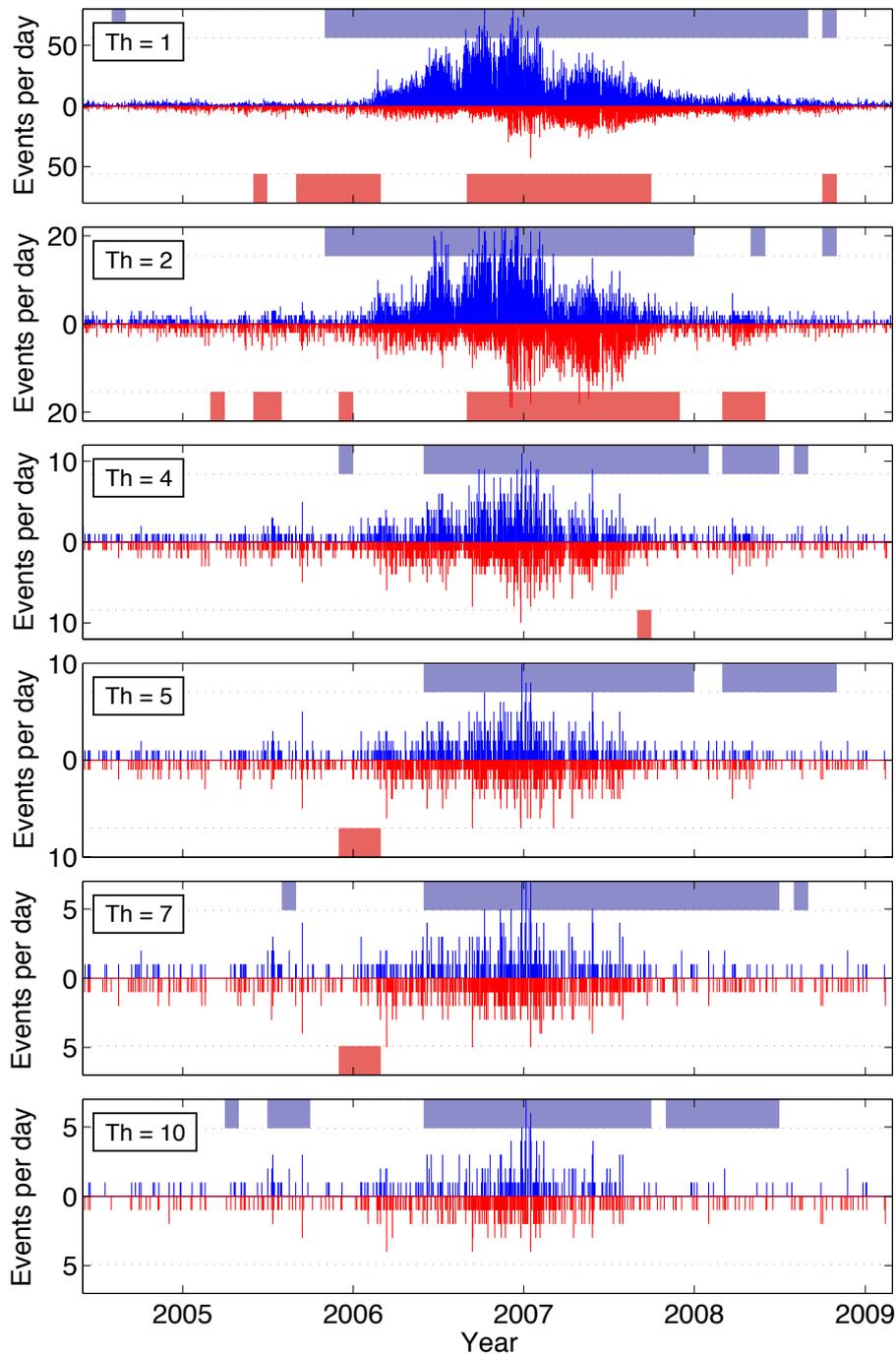
**Figure C.3:** Distributional agreement of “IBC Baghdad” and “SIGACT Baghdad”. Color bars illustrate the results of a 2-sample Anderson-Darling tests for the distribution of number of events for time windows of  $T = 120$  days (orange bars),  $T = 180$  days (green bars) and  $T = 360$  days (violet bars) for thresholds equal to 1, 2, 4, 5, 7 and 10 casualties. The bars indicate the center of those time windows for which the hypothesis of agreement of the distribution of events per day can be rejected at a 5% significance level. The black line represents the RMS difference between “IBC Baghdad” and “SIGACT Baghdad”, red and blue lines are the monthly averages of the number of events per day for the two datasets respectively. Data variation (b).



**Figure C.4:** Distributional agreement of “IBC Baghdad” and “SIGACT Baghdad”. Color bars illustrate the results of a 2-sample Anderson-Darling tests for the distribution of number of events for time windows of  $T = 120$  days (orange bars),  $T = 180$  days (green bars) and  $T = 360$  days (violet bars) for thresholds equal to 1, 2, 4, 5, 7 and 10 casualties. The bars indicate the center of those time windows for which the hypothesis of agreement of the distribution of events per day can be rejected at a 5% significance level. The black line represents the RMS difference between “IBC Baghdad” and “SIGACT Baghdad”, red and blue lines are the monthly averages of the number of events per day for the two datasets respectively. Data variation (c).



**Figure C.5:** Day-by-day match of events of a given size  $s$  in “SIGACT Baghdad” to entries in “IBC Baghdad”. Blue bars indicate the number of matched events as a fraction of the total number of events in SIGACT for every months in the dataset (left axis), the red line illustrates the overall number events per months for the given casualty sizes (right axis). When matching events we allow for a timestamp uncertainty of  $\pm 1$  day. Data variation (c).



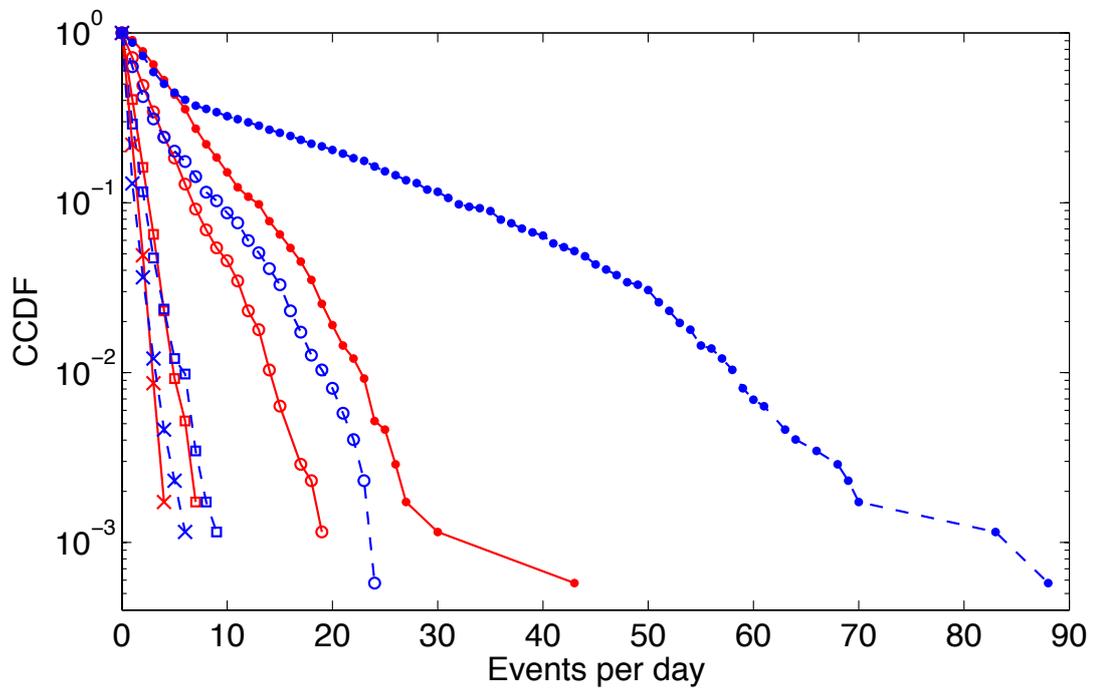
**Figure C.6:** Inter-event timing signatures. Color bars illustrate the results of a KS-test for exponential distribution of the inter-event times in time windows of  $T = 180$  days for thresholds equal to 1, 2, 4, 5, 7 and 10 casualties (see text for details). The bars indicate the center of those time windows for which the hypothesis of agreement of the distribution of inter-event times with an exponential distribution can be rejected at a 5% significance level. (i.e., the datasets exhibits a non-trivial timing structure). The graph also shows the dynamics of the number of events per day in “IBC Baghdad” (red) and “SIGACT Baghdad” (blue). The vertical axis for the IBC dataset was mirrored for clarity purposes. Data variation (c).

## Figure 9

We find that neither of the data variations has a significant impact on the results reported in Figure 9 of the article.

## C.4 Distribution of events per day

In the daily time series comparison (section 5.3.3 of the article) we emphasize that the distributions of events per day do not have fat-tails and typically decay almost exponentially. Figure C.7 demonstrates this for both “IBC Baghdad” and “SIGACT Baghdad” at various thresholds.



**Figure C.7:** Complementary cumulative distribution function (ranking plot) of number of events per day in the datasets “IBC Baghdad” (red solid line) and “SIGACT Baghdad” (blue dashed line) for thresholds equal to 1 (solid circles), 2 (open circles), 5 (squares) and 10 (crosses) casualties per event.

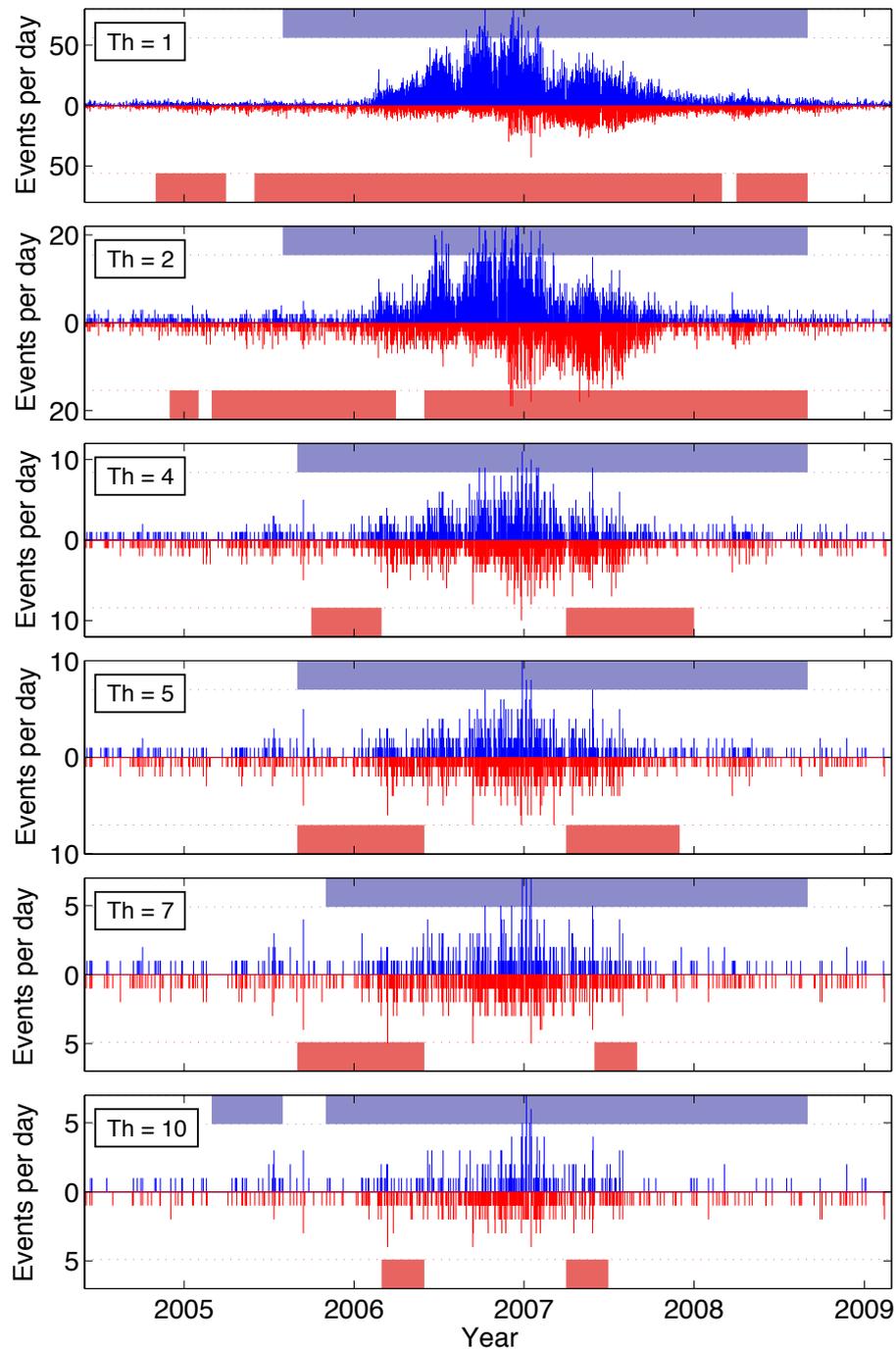
## C.5 Sensitivity analysis for distributional comparisons

In our analysis of distributional signatures in IBC and SIGACT (section 5.3.4 of the article) we test the distribution of inter event times against the null hypothesis of exponential distribution, which indicates Poisson dynamics for the process. In order to verify that results of Figure 8 of the article for larger thresholds (more than 2 casualties per event) are not an artifact of small sample size, we applied the same method for much larger moving time windows of 360 days.

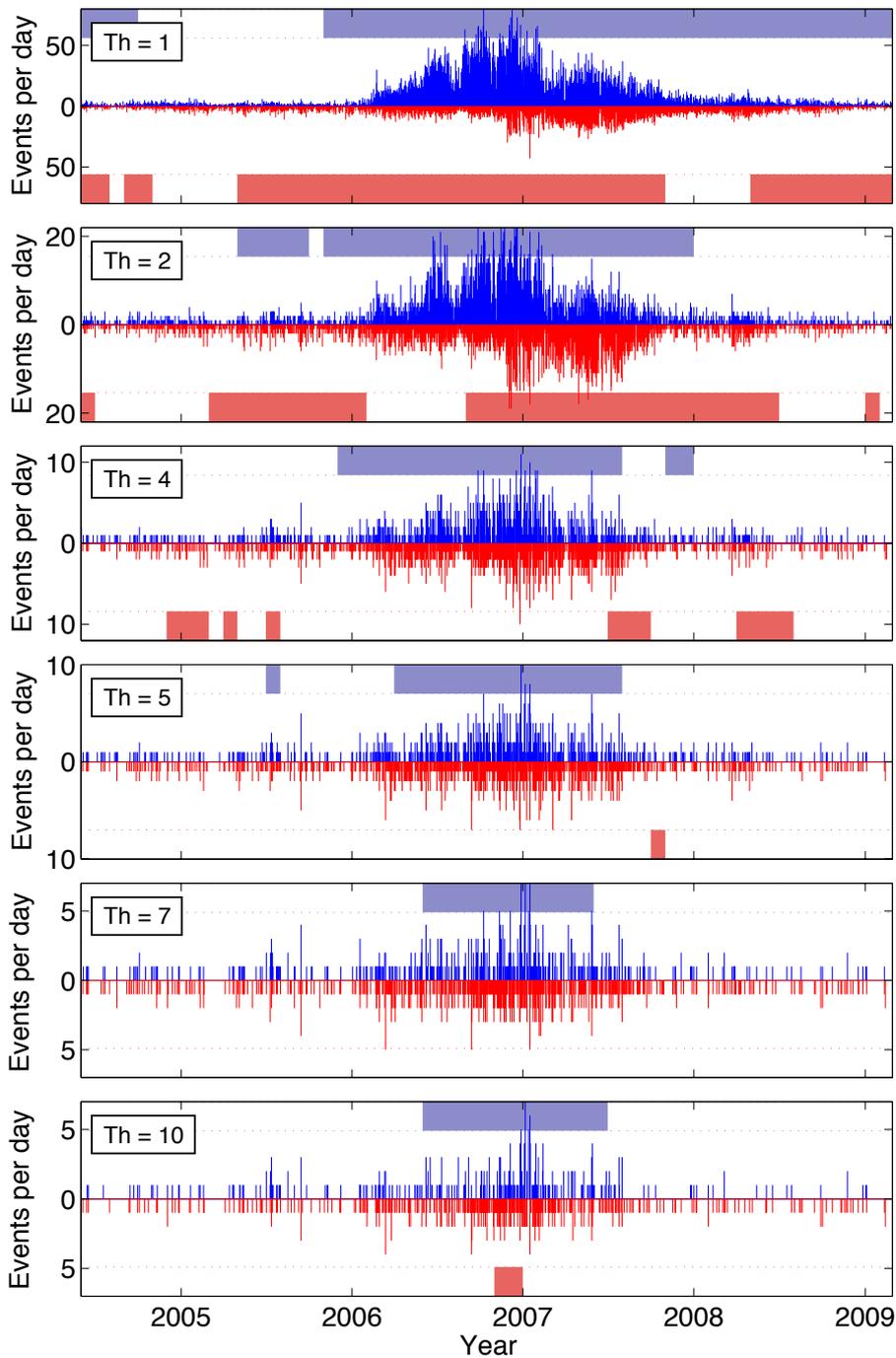
Figure C.8 shows the results of this analysis. One can clearly see that due to the non-stationarity of the data within the larger time window we can now reject the hypothesis of feature-less dynamics in much wider time intervals, as one should expect. This is clearly visible for both IBC and SIGACT at thresholds of 1 and 2 casualties. However, for the IBC dataset and large thresholds (larger than 2 casualties per event) we can—despite the non-stationarity—for most of the time period analyzed not reject the null hypothesis of exponential distribution. Notice in particular that this is true for the period in which the conflict escalated (second half of 2006 and first half of 2007). The results thus confirm the featureless dynamics of IBC for larger thresholds.

Additionally, in section 5.3.4 we have also emphasized that testing the null hypothesis of the Poisson distribution of events per day leads to substantially equivalent results. Figure C.9 and Figure 8 of the article indeed yield very consistent estimates of where both datasets exhibit non-trivial timing structures. Notable exceptions are short time windows in 2005 and 2006 where the event per day statistics suggest more non-trivial timing structure in IBC (for low thresholds) and more trivial timing structure in SIGACT (for high thresholds) compared to the inter-event statistics.

Notice that both tests effectively complement each other with respect to statistical power. In case of large number of observed events per window the test for exponential distribution of inter-event times provides much more robust results. However, if the samples are small (such as in 2005 or 2008–2009 and in case of large thresholds) the test for Poisson distribution of events per day is more powerful and can reject the null hypothesis of Poisson dynamics even when the clustering is moderate. This gives us additional confidence in the results of Figure 8, in particular for the periods with lower intensity of violence.



**Figure C.8:** Inter-event timing signatures. Color bars illustrate the results of a KS-test for exponential distribution of the inter-event times in time windows of  $T = 360$  days for thresholds equal to 1, 2, 4, 5, 7 and 10 casualties. The bars indicate the center of those time windows for which the hypothesis of agreement of the distribution of inter-event times with an exponential distribution can be rejected at a 5% significance level. (i.e., the datasets exhibits a non-trivial timing structure). The graph also shows the dynamics of the number of events per day in “IBC Baghdad” (red) and “SIGACT Baghdad” (blue). The vertical axis for the IBC dataset was mirrored for clarity purposes.



**Figure C.9:** Number of events per day signatures. Color bars represents results of the chi-square test for the Poisson distribution for both datasets and time window of  $T = 180$  days for thresholds equal to 1, 2, 4, 5, 7 and 10 casualties (see text for details). The bars indicate the center of those time windows for which the null hypothesis of Poisson distribution for the numbers of events per day can be rejected at a 5% significance level. (i.e., the datasets exhibits a non-trivial timing structure). The graph also shows the dynamics of the number of events per day in “IBC Baghdad” (red) and “SIGACT Baghdad” (blue). The vertical axis for the IBC dataset was mirrored for clarity purposes.

# D Supplementary Information (SI): “Severity matters”<sup>†</sup>

## D.1 Data

In our analysis we rely on detailed event data on the conflict in Iraq downloaded from *The Guardian* website (Rogers, 2010a) on September 3, 2013. It contains information on all “significant actions” (SIGACTs) reported by units of the U.S. military in Iraq that resulted in at least one casualty. The dataset covers the period January 1, 2004 until December 31, 2009 but is missing 2 intervals of 1 month length each (from April 30, 2004 to June 1, 2004 and from February 28, 2009 to April 1, 2009). This effectively restricts our period of analysis to the period June 1, 2004 to February 28, 2009.

In our study we rely on the following information coded for every incident in the dataset: “Date and time” with a resolution of minutes, the total number of casualties “Total deaths”, and the “Longitude” and “Latitude” of where the incident occurred. These geo-coordinates are truncated at a tenth of a degree (about 10 km) for Iraq outside of Baghdad and at a hundredth of a degree (about 1 km) for the military zone of Baghdad. In our analysis we consequently only analyze distances between events at a maximal resolution of 10 km for all of Iraq and 1 km for Baghdad. Note that we excluded all events of type “Non-Combat Event” as these correspond to traffic accidents etc. and thus do not constitute conflict events.

We intentionally chose the total number of casualties as the dependent variable and did not rely on the more disaggregate coding of victims as “Coalition forces”, “Iraq forces”, “Civilians” and “Enemy” provided in the data because these detailed categorizations are very prone to bias (Rogers, 2010b). We also exclusively rely on counts of individuals “killed in action” (KIA) and do not consider the counts of “wounded in action” (WIA) because the former is usually coded far more reliably. Note, too, that if considering aggregate counts of KIA + WIA, for example, the conflict dynamics the dataset covers do not change substantially (Donnay et al., 2014).

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<sup>†</sup>This chapter is an edited version of the supplementary information for the following article: Karsten Donnay. (2014). “Severity matters: Analyzing the spatiotemporal relationship of small- and large-scale violence in Iraq.” Manuscript in preparation.

It is also important to emphasize that for many entries in the dataset perpetrator identities can not be reliably identified. Prior work has relied on information regarding the “type” of events and the “affiliation” of perpetrators to distinguish events initiated by coalition or insurgent forces (Linke et al., 2012). A detailed analysis of the dataset, however, reveals that these categorizations can be very unreliable classifiers. Note, for example, that “Friendly Actions”—perpetrator affiliations reported here are exclusively “FRIEND”, i.e. coalition or Iraqi forces—are not limited to incidents actually perpetrated by coalition or Iraqi forces but also contain reports about shootings among civilians, for example. They also contain reports of casualties that can not be clearly ascribed to enemy action or reports of engagements that were actually not initiated by friendly forces but in which then only insurgents suffered casualties. In such cases it would clearly be incorrect to classify the incident as initiated by coalition forces.

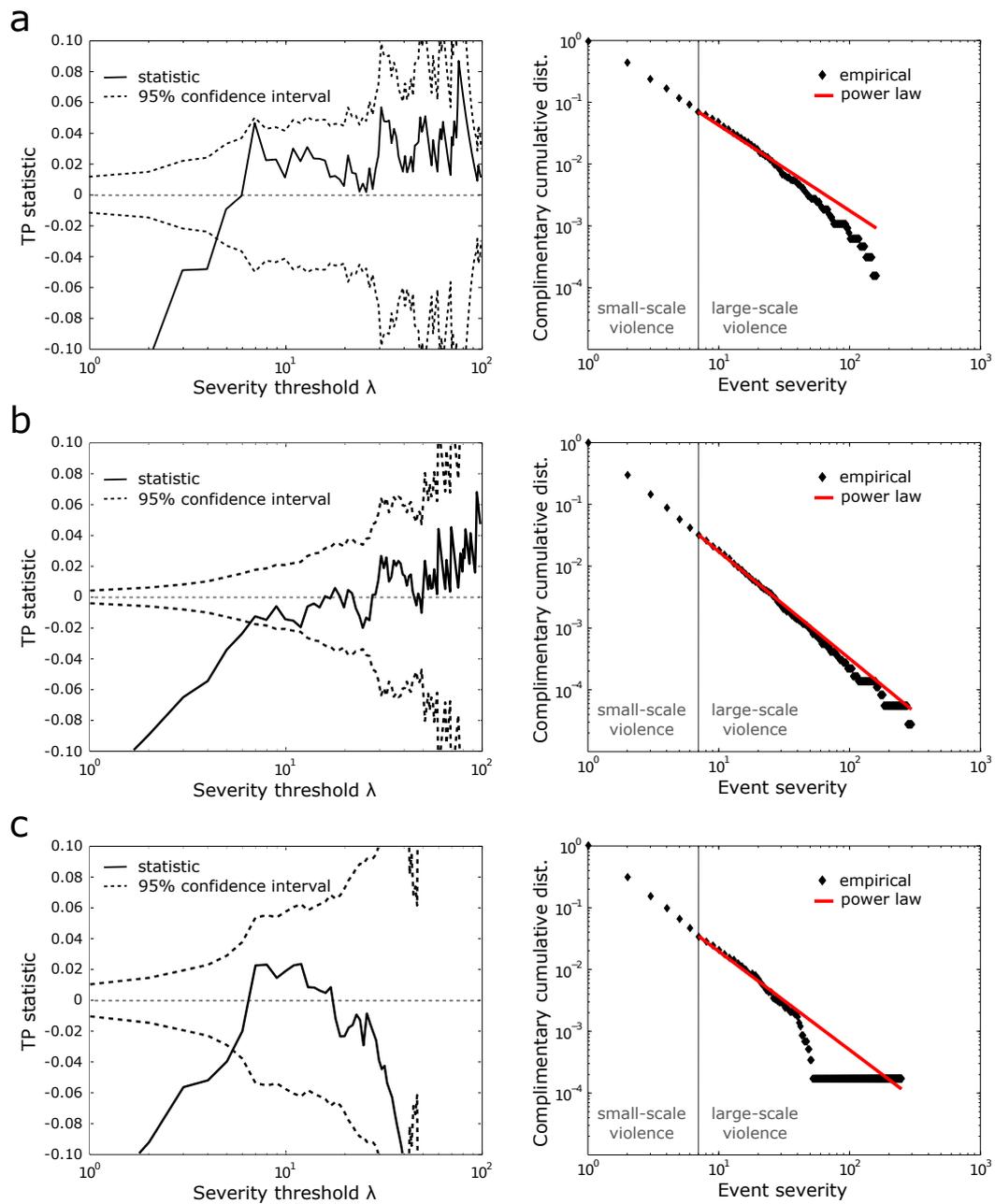
### D.2 Violence classification for sub-periods and provinces

In the article we rely on the full dataset—all of Iraq for the period 2004–2009—to classify conflict events into two broad categories of small- and large-scale violence. We here show that the classification for the three main periods of the conflict and for individual provinces leads to a substantially identical classification, i.e., we find the same or very similar values of  $\lambda$  to characterize the onset of the power law tail in the severity size statistics.

Figures D.1a to D.1c show the results of the classification for all of Iraq in the three main periods of the conflict, 2004–2005, 2006–2007 and 2008–2009 respectively. In all cases we find that the power law tail in the complementary cumulative distribution (ccdf) of event severities starts at  $\lambda = 7$ , exactly as for the full period 2004–2009. In the first period the TP statistic first lies within the confidence interval for  $\lambda = 6$  but it is closer to 0 at  $\lambda = 7$  (Figure D.1a, left panel). Note that it fluctuates relatively strongly already for  $\lambda > 7$  but does not leave the confidence bound. The identification of the power law tail is more robust for the second period where for  $\lambda = 7$  the statistic first lies within the confidence bounds and then stays relatively close to 0 until thresholds larger than 30 (Figure D.1b, left panel). In the third period the power law tail is again more difficult to identify. We here determine  $\lambda = 7$  as the value for which the statistic is first closest to 0 (Figure D.1c, left panel). Note that while the power law fit to the ccdf in the second period is visually excellent (Figure D.1b, right panel), the tails of the ccdf in the first and third period more visibly deviate from power law for large event sizes (Figure D.1a and c, right panel)—this is consistent with the large fluctuations we observe for the TP statistic at large values of  $\lambda$ , in particular in the third period.

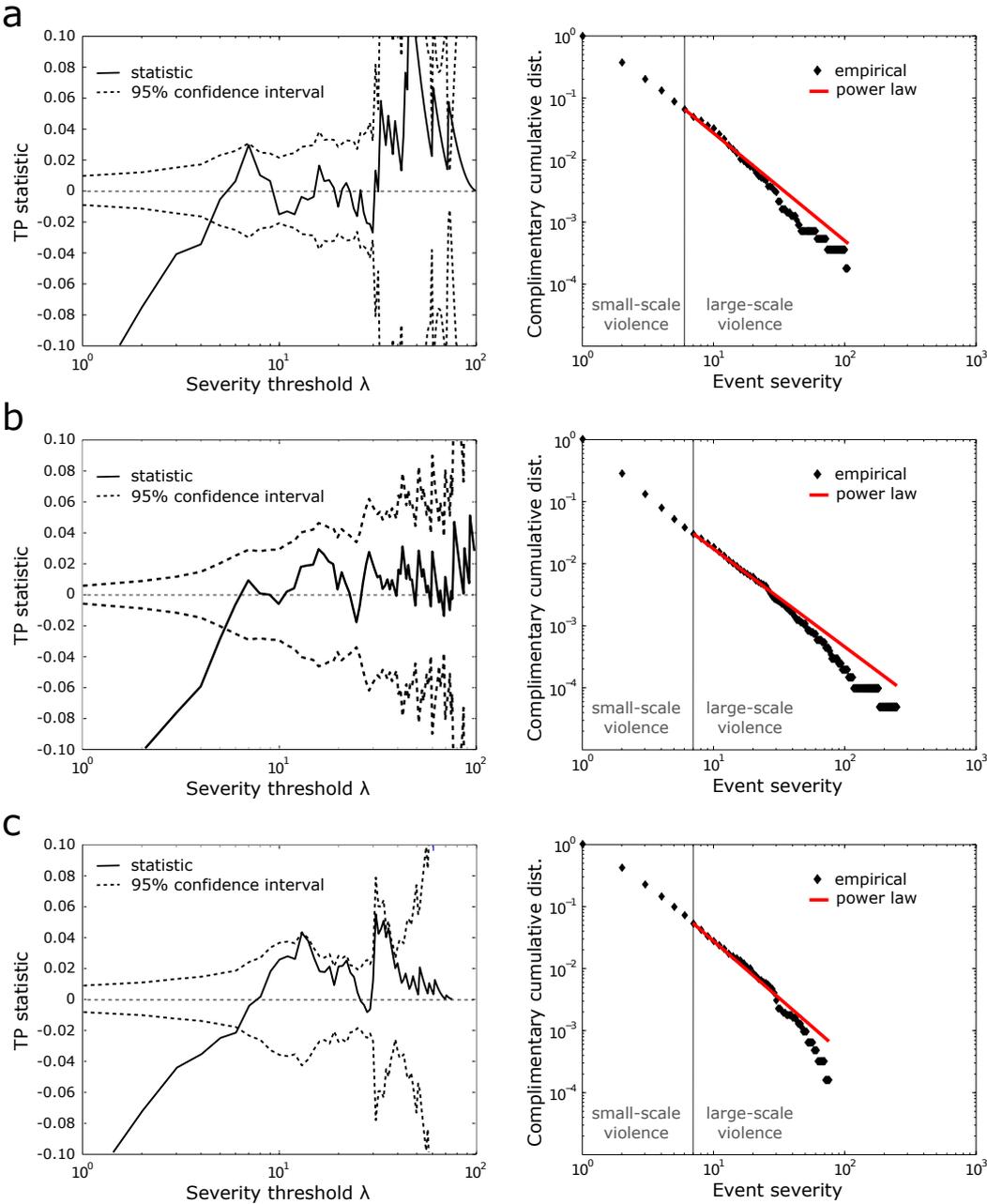
In Figures D.2a to D.2c we repeat the classification for the full period 2004–2009 for each of the three most violent provinces. In Al Anbar—the second most violent province—the TP statistic first comes closest to zero for a value of  $\lambda = 6$  (Figure D.2a, left panel), whereas both in Baghdad—the most violent province—and in Diyala—the third most violent province—we find a minimum threshold value of  $\lambda = 7$  (Figures D.2b and c, left panel). The estimation is clearly the most robust for Baghdad but for not too large thresholds the TP statistic for both Al Anbar and

## D.2. Violence classification for sub-periods and provinces



**Figure D.1:** Statistical classification of events into small- and large-scale violence for **a** 2004–2005, **b** 2006–2007 and **c** 2008–2009. The left panels shows the change of the TP statistic as a function of the threshold  $\lambda$ , the right panel depicts the complementary cumulative distribution of event sizes.

Appendix D. Supplementary Information (SI): “Severity matters”



**Figure D.2:** Statistical classification of events into small- and large-scale violence for **a** Al Anbar, **b** Baghdad and **c** Diyala for the full period 2004–2009. The left panels shows the change of the TP statistic as a function of the threshold  $\lambda$ , the right panel depicts the complementary cumulative distribution of event sizes.

Diyala also does not leave the confidence bounds. Similar to the comparison before, the power law fit of the ccdf in Baghdad is also visually excellent (Figure D.2b, right panel). In comparison, the tails of the ccdf for Al Anbar and Diyala clearly deviate from power law for large event size (Figure D.2a and c, right panel).

Overall, these supplementary analyses clearly confirm that the statistical classification of events into small- and large-scale violence is neither critically dependent on the period nor the region of analysis. Note though that our results here also highlight that the smaller the sample, the more difficult it is to robustly identify the tail of the distribution—this is the case for the first and third period but also for Al Anbar and Diyala. The classification using the largest sample, i.e., the full data, is clearly the most robust (see Figure 3 of the article).

### D.3 Knox test

The analysis in the article relies on the *Knox* test, an elegant non-parametric clustering test that has previously been used by Braithwaite & Johnson (2012) for the analysis of conflict event data. Since it does not require specification of an expected baseline of events when testing for significant clustering, it is particularly suited for this kind of data. The Knox test detects significant clustering using a simple permutation test that randomly swaps the locations of events while preserving their temporal order. Note that this, of course, trivially ensures that the empirical data and the permuted sample tested against have identical trends or *momentum*. In other words, temporal non-stationarity is intrinsically accounted for. Conflict event data, however, generally also features significant variation in the prior *exposure* of a given location to violence.

In order to ensure that we do not introduce spurious signatures from changes in exposure over time, we only permute locations in small moving windows of 6 months length to generate the random baseline. This guarantees that only locations enter the baseline that were actually exposed to violence in a given period. Note that we avoid artifacts from the definition of the time windows, in which we swap locations, by continuously varying the center of those time windows with every random baseline. In total all of our estimates are then based on  $n = 1000$  such baselines.

Exactly as in Braithwaite & Johnson (2012), we repeat the test for a series of spatiotemporal windows thus overcoming the limitation of choosing arbitrary spatiotemporal bins. Specifically, for all events in the dataset we count the number of subsequent events that lie within a given spatial and temporal window, i.e., the Knox metric. The factor by which the number of empirical events deviates from our null expectation, the Knox ratio,  $K$ , is then simply the empirical Knox metric divided by the average simulated Knox metric. The significance of this Knox ratio estimate is then given by  $p = (r + 1)/(n + 1)$ . This significance level can be calculated for a significant increase of event counts compared to the baseline ( $K > 1$ ) but also for a significant decrease ( $K < 1$ ).  $r$  then is the number of cases where a simulated Knox metric is larger or equal to or smaller or equal to the empirical Knox metric respectively (Braithwaite & Johnson, 2012).

## D.4 Sensitivity analysis

The substantive findings with regard to spatiotemporal clustering of small- and large-scale events presented in the article are not critically dependent on the exact choice of the classification cutoff  $\lambda$ . Figures D.3 to D.5 correspond to Figures 5 to 7 of the manuscript respectively, each showing results for a threshold value of (a)  $\lambda = 6$  and (b)  $\lambda = 8$ . The results are overall clearly very consistent across different thresholds. The spatiotemporal correlations among small-scale violence are in all three periods the most insensitive to variations in  $\lambda$  but also the correlations among large-scale violence are very consistent. Note though that the maximal correlations here tend to increase with larger thresholds, i.e., a more restrictive definition of large-scale violence.

The cross-correlations between small- and large-scale violence are very robust in the period 2004–2005. The observed patterns here are, in fact, almost identical (Figure D.3). The only effect of changes in  $\lambda$  on the cross-correlations is visible in the second and third period. In the period 2005–2007, in which the level of cross-correlations is anyway very weak and mostly insignificant, increasing the threshold makes this separation of the spatiotemporal dynamics of small- and large-scale violence only more complete. Specifically, while the maximal correlations observed again increase slightly, less combinations of spatial and temporal window sizes yield significant results (Figure D.4b). This effect is even more visible in the period 2008–2009 (Figure D.5b). Note that this effect is probably mainly a consequence of lack of sampling precision. First, overall the number of events in this period is comparably smaller than in the other two. Second, we observe much fewer large-scale events—increasing  $\lambda$  thus only makes the sample smaller. Taken together it thus becomes increasingly more difficult to detect significant cross-correlations but also to detect correlations among large-scale events, even though the maximal effect size here increases.

Figures D.6 and D.7 show *hot spot* and *hot phase* signatures for a threshold value of (a)  $\lambda = 6$  and (b)  $\lambda = 8$ . The figures are substantively identical to Figures 8 and 9 of the article. The results for small-scale violence are the most consistent. Note that some signatures are less clear for larger thresholds, for example the *hot spot* signature for large-scale violence in Diyala province (Figure D.6b, upper right panel) or the *hot phase* signature for large-scale violence in Baghdad (Figure D.7b, upper right panel). Generally variations in threshold appear to affect mainly signatures that were not very robust for  $\lambda = 7$ . This is, for example, the case for the *hot spot* signature of large-scale violence in Baghdad (Figure D.6b, upper right panel). Overall, however, the analysis confirms that also at the level of provinces our substantive results are robust to the exact choice of classification threshold.

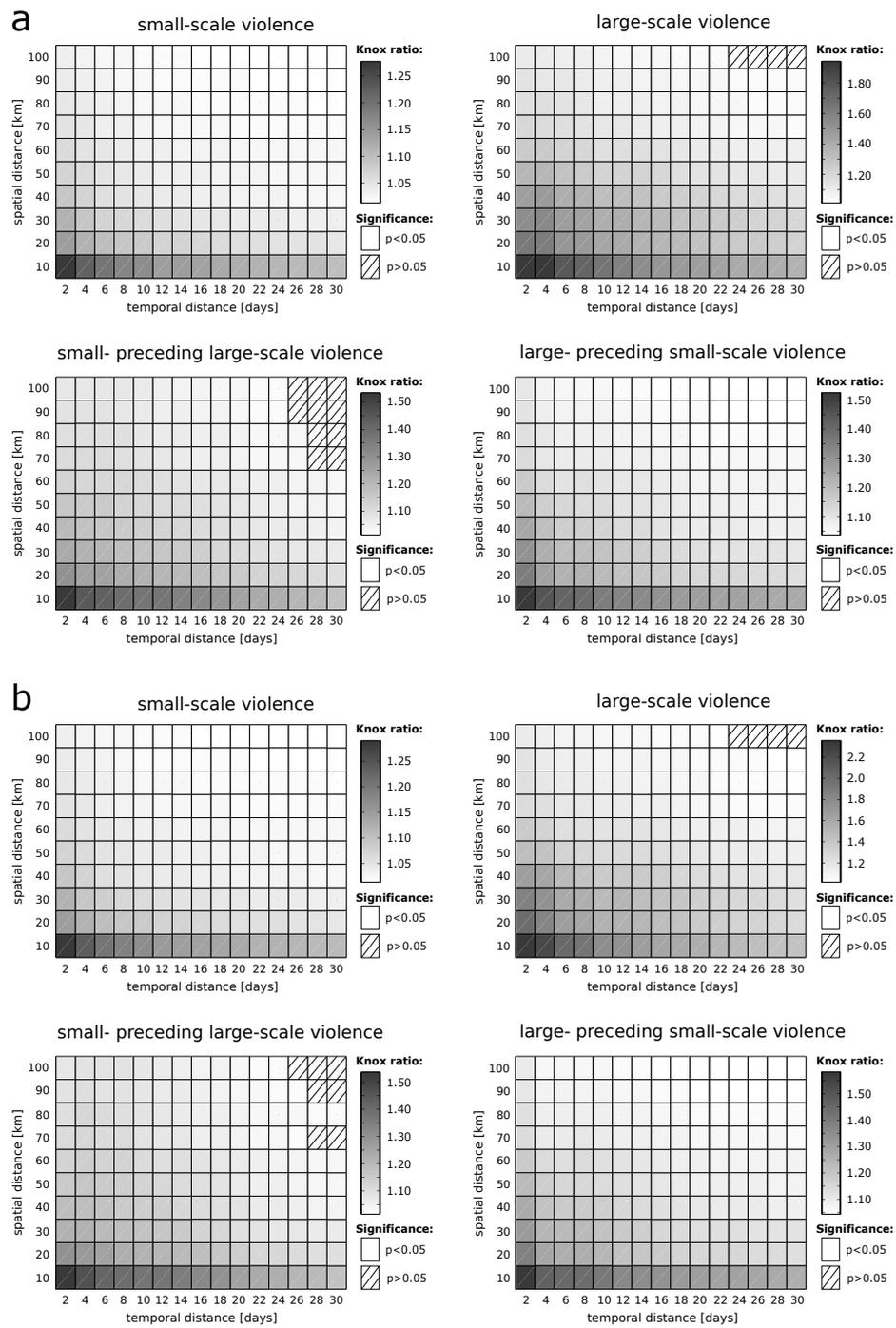


Figure D.3: Knox test results for the period 2004–2005 for **a**  $\lambda = 6$  and **b**  $\lambda = 8$ .

Appendix D. Supplementary Information (SI): “Severity matters”

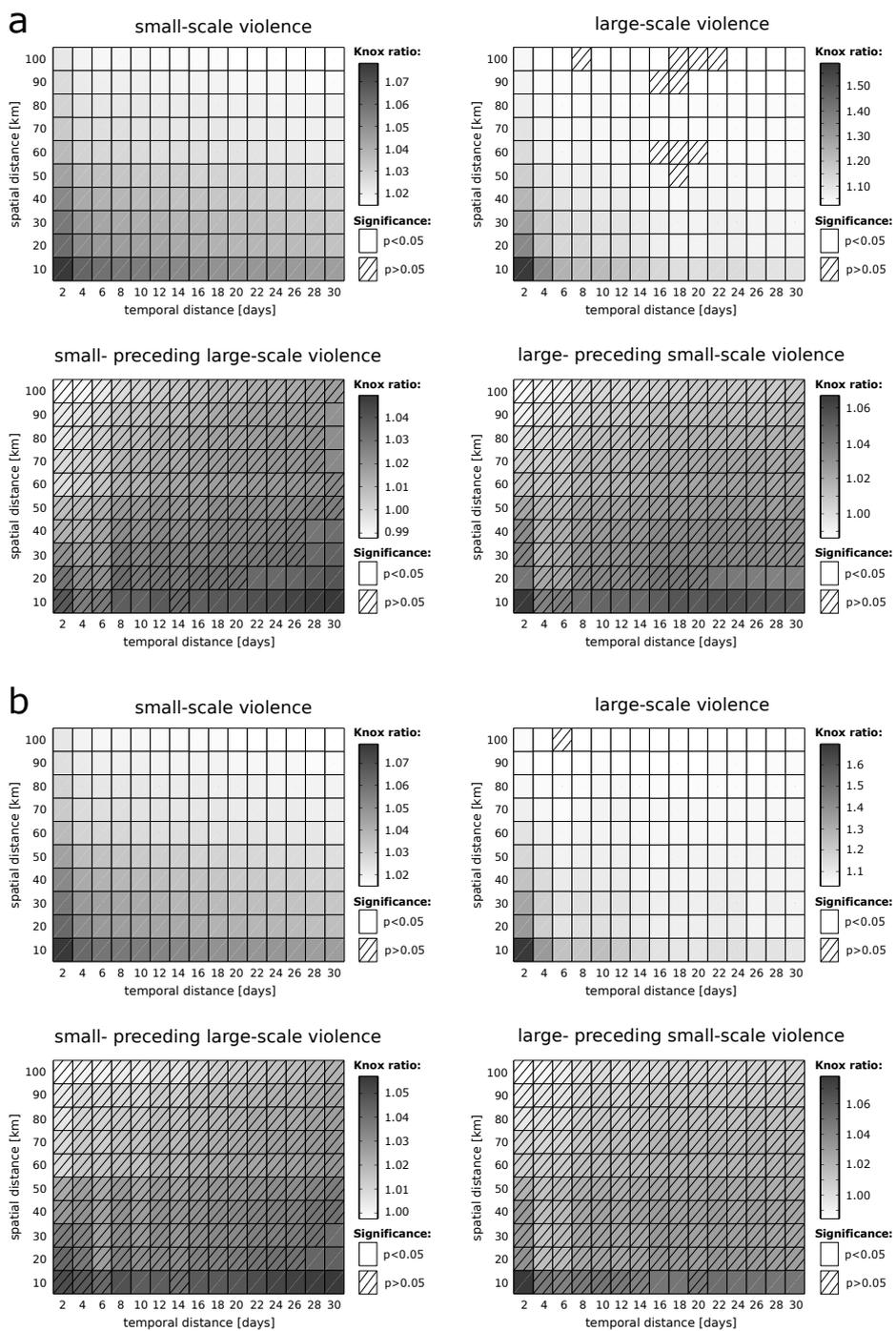


Figure D.4: Knox test results for the period 2006–2007 for **a**  $\lambda = 6$  and **b**  $\lambda = 8$ .

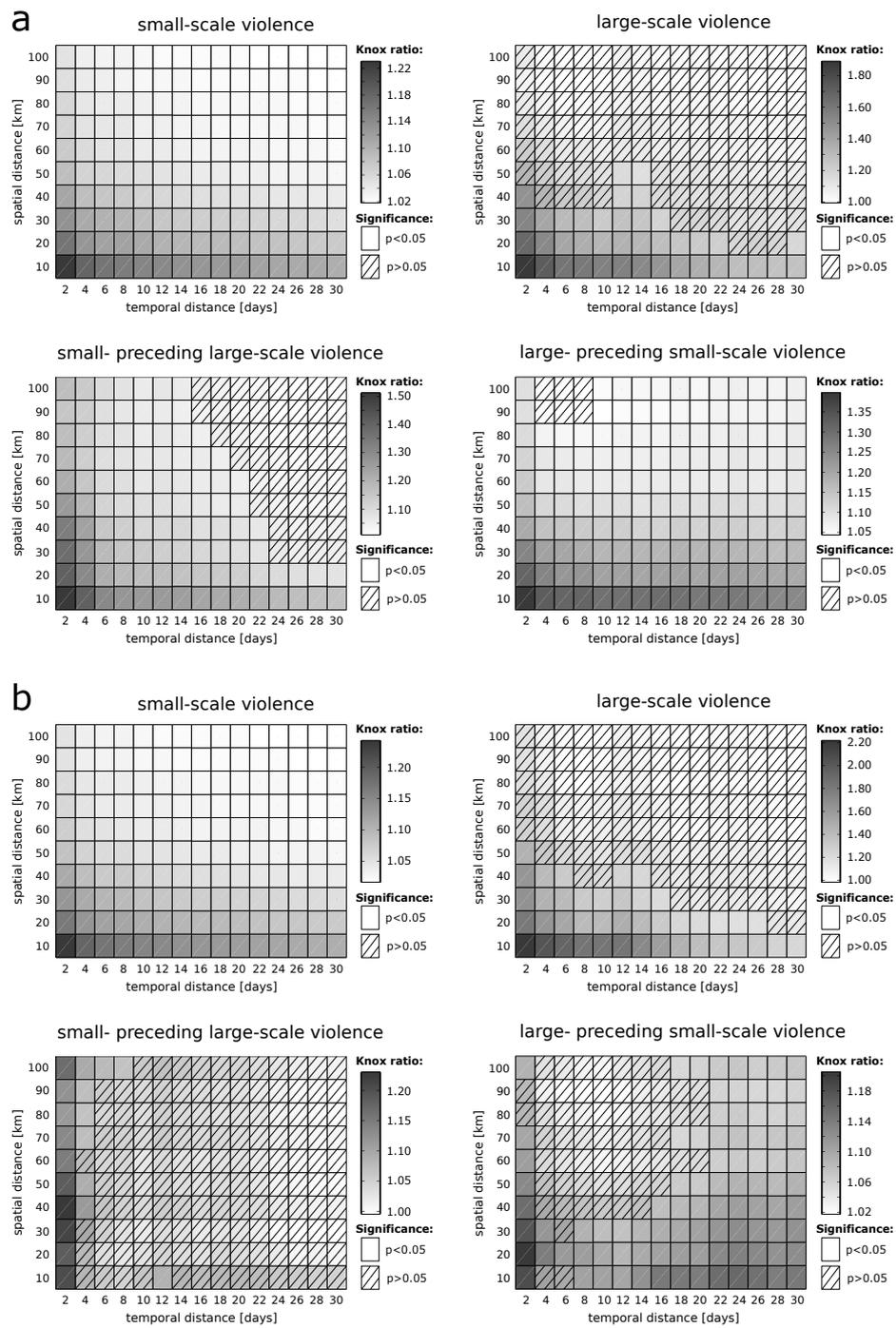


Figure D.5: Knox test results for the period 2008–2009 for **a**  $\lambda = 6$  and **b**  $\lambda = 8$ .

Appendix D. Supplementary Information (SI): “Severity matters”

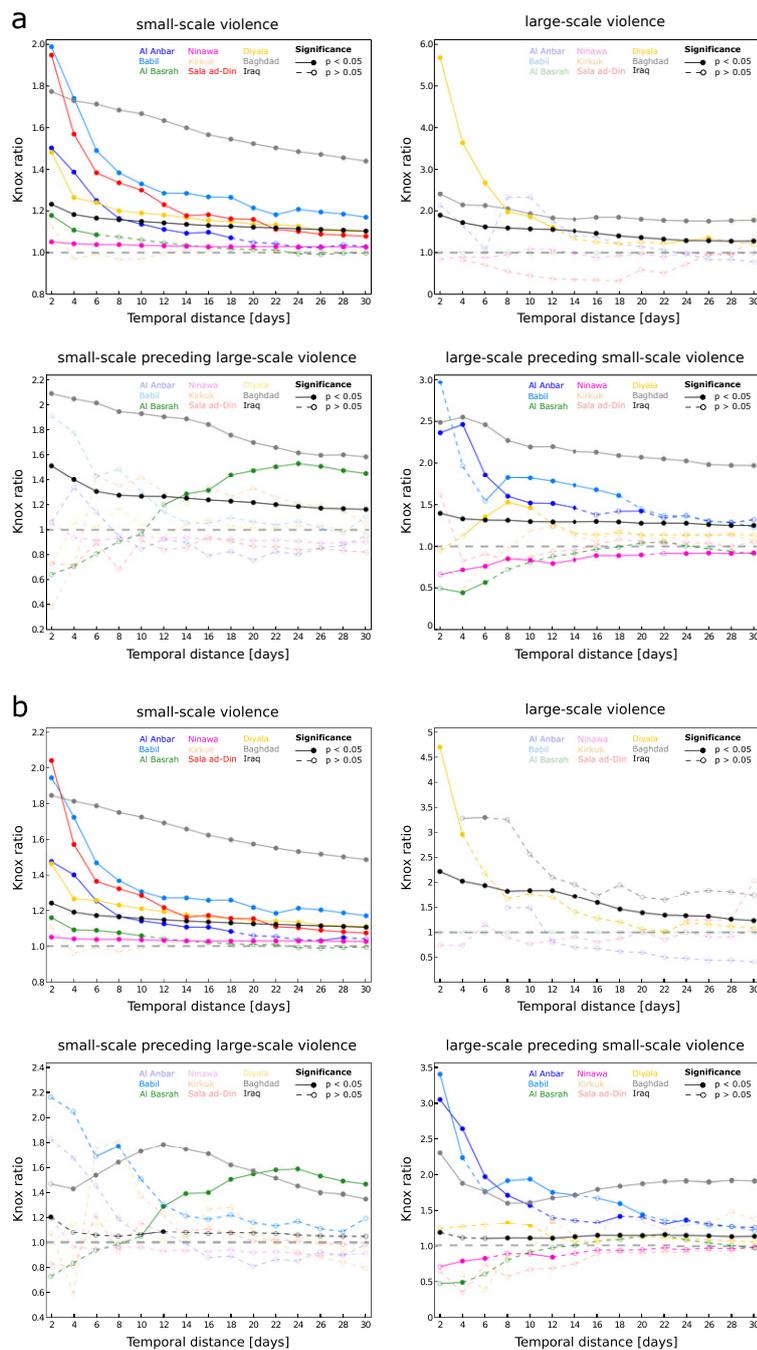
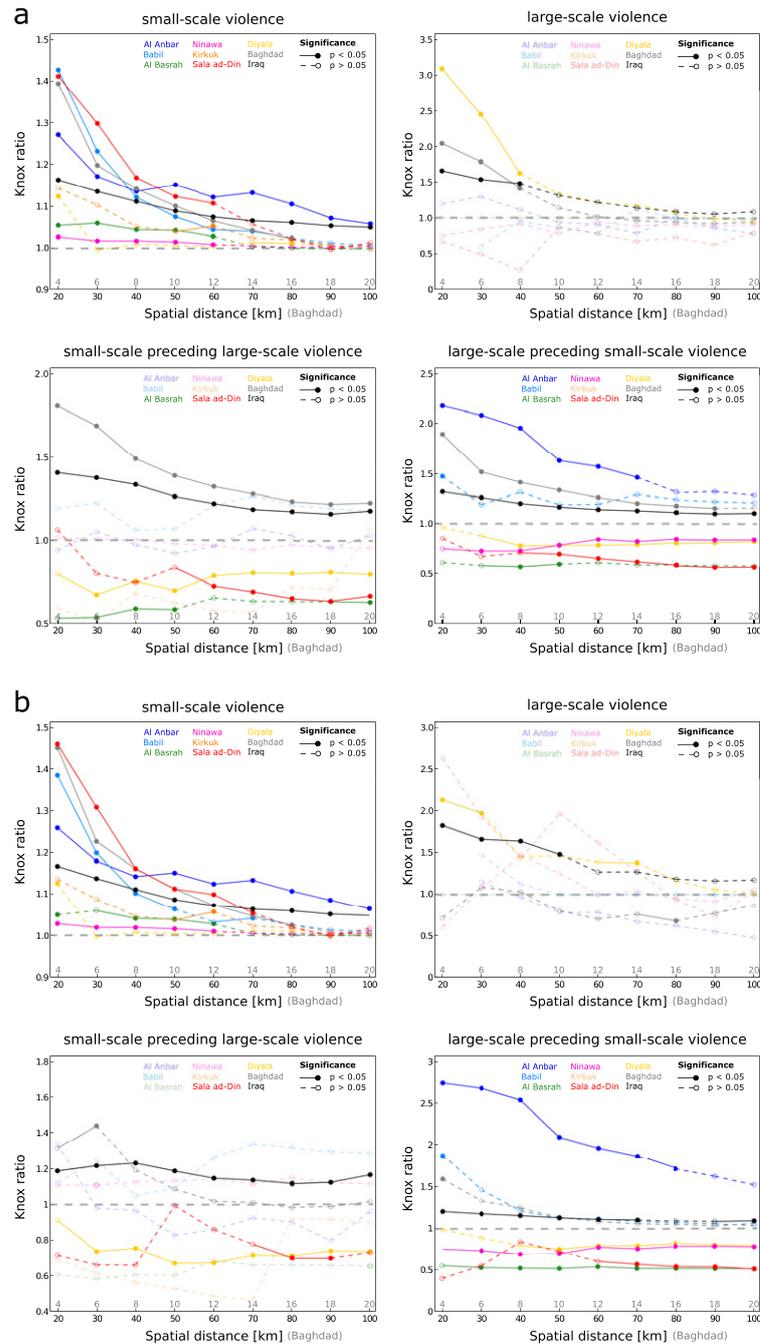


Figure D.6: Hot spot signatures across provinces for the period 2008–2009 for a  $\lambda = 6$  and b  $\lambda = 8$ ; provinces without any significant estimates are shown semi-transparent.

## D.4. Sensitivity analysis



**Figure D.7:** *Hot phase* signatures across provinces for the period 2008–2009 for **a**  $\lambda = 6$  and **b**  $\lambda = 8$ ; provinces without any significant estimates are shown semi-transparent.



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