A Semi-Parametric Spatial Regression Approach to Post-War Human Security: *Cambodia, 2002-2004*

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Abstract

Human security in post-war societies depends on incentives to forgo violence in local interactions. The government of Cambodia monitors domestic violence, land conflicts and serious crime in over 15,000 villages and urban neighborhoods. We use three annual data collections to estimate the response of these conflicts to the legacy of the war, poverty and resource competition, urbanity as well as governance quality. Bayesian spatial regressions help identify socio-economic thresholds beyond which conflicts expand or contract significantly. The spatial terms also mitigate the effects of correlated measurement errors and of unobserved factors (among which the demographic impact of the genocide). We find numerous non-linearities in the propensities for violence. Notably, predicted rates decrease in response to quality of governance only at a high level of service provision, which realistically most communities may not soon achieve. This may justify dedicated programs addressing particular types of conflicts, such as through victim assistance and land titling. We propose alternative analytic approaches, including some that would make the problem of endogeneity more tractable once updated poverty estimates become available.

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Introduction

Local communities in post-war countries recover unevenly. Do those in more favorable socio-economic conditions offer their residents better security? Do threats to life and the integrity of persons diminish apace with the recovery, or do they linger at elevated levels until growth and development have crossed certain thresholds? Should policy assume that economic opportunity and public service provision will take care of the risk of violent conflict, or does it call for direct intervention?

This article takes advantage of a rich database of local conflicts, many of them violent, in Cambodia to shed light on such questions. To do so, we focus on non-linear associations between social and economic conditions and post-war human security at the community level. Pervasive and widespread local conflict characterizes many poor countries (Barron, Kaiser and Pradhan 2004); in post-war situations conflict and violence may linger at particularly high levels (Pearce 1999). Although local-level studies of the causal nexus between socio-economic development and violent conflict are less common than those relying on country-level data, more of them have appeared in recent years and have refined insights, linking them more closely to policy considerations. Deininger‘s study from Uganda is seminal in this line (Deininger 2003).

Where such studies use statistical models, these tend to be parametric, assuming that the effects on levels of conflict are uniform across observed ranges of the socio-economic covariates. At most, expected non-linear effects are modeled through pre-defined functional forms, chiefly as squared terms.

By contrast, our study employs Bayesian methods to allow full expression of non-linear effects, letting the “data speak for themselves.” We choose these methods over other non-parametric models, such as local regression, because they admit spatial dependency in count data models. The results reveal thresholds in the factors that exacerbate or diminish conflict, some of which are dramatic. Non-linear effects are present on the levels of all three types of conflict that are distinguished in the database – domestic violence, land conflicts, and serious crime – and are, for some factors, similar across the three.

For example, in a typical community in the year 2002, we predict that one in every 57 households had a domestic violence problem during the year, close to the observed 1 : 55 rate. At that point, an important governance indicator – the fraction of households with access to safe water – stands at 35 percent. Other factors remaining constant, improved water provision initially causes very little change in domestic violence; the rate edges to 1 : 58 (at 55 percent safe water access), then to 1 : 60 (75 percent). Then, after another such increment (to 95 percent), the violence rate drops disproportionately to 1 : 69.

By “factors”, we mean explanatory variables that one may also consider as indicators taken from four potential causal domains – the legacy of the war, poverty and resource competition, urbanity, and the quality of governance. The conflict and indicator data is available for virtually all of the 1,628 local communes in Cambodia; this allows us to control for spatial dependency.

Our analysis strategy remains cross-sectional, despite the fact that the conflict event counts and some of the socio-economic variables were recorded for three consecutive years. This is so primarily because of reliability concerns (addressed below). Consequently, and in the absence of conflict-resistant instruments, we cannot answer questions of endogeneity. Such effects – from conflict and violence to socio-economic development – are plausible, particularly in an aid-dependent country like Cambodia, where the presence and effectiveness of development agencies depends on a minimum of local security. Therefore our findings are limited to associations between development and conflict, without establishing causality. Throughout, the focus remains on non-linear community-contextual risk factors in inter-personal violence.

Earlier, we estimated the effects of those factors using parametric models (Owen and Benini 2005). Key findings were:

- Better governance dampens violence. Using proxies for the cumulative effects of development agencies, significant effects on all types of violence appear.
- The intensity of the war has a lingering effect on violence. With the passage of time, however, it has become weak, compared to the strength of other factors.

- Severe poverty breeds violence. Violence is much more frequent in communities with higher destitution and with keener competition for resources.

This article examines whether those relationships are linear or change disproportionately once communities cross certain socio-economic thresholds. Also it tests the significance of the relationships more rigorously by controlling for unobserved factors, to the extent that these are spatially correlated.

What to expect

The article proceeds as follows: After briefly surveying the relevance of high-levels of interpersonal conflict and violence for socio-economic development, we reference select findings from a small number of micro-level studies[1]. Some of these are set in countries still at war (e.g. Uganda, Colombia), yet appear relevant to post-war situations. We then relate to the work of others the four potential causal domains that serve as an ordering device between the socio-economic covariates and the conflict data.

Together with the usual descriptive statistics of our data, we discuss aspects of their reliability and of the validity of their use for our models. Next we justify the motivation for Bayesian methods (not least the fact that one of our students, Pedersen Dyreng (2004) used them on a subset of this data to critique our parametric results). We detail the models, which use the same set of explanatory variables and contiguity map across conflict types. Our final models mix fixed (year effects), linear and semi-parametric specifications. We present fixed and linear effects as the means and credible intervals of the coefficients.

This and other regression output are placed in the appendix, and we focus the reader's attention on a matrix graph that arranges the effect curves by covariate and conflict type. While it is easy to see the non-linear effects, the locations of thresholds are somewhat arbitrary. They depend on visual inspection as much as on determining the fraction of observations that have effects significantly different from zero. We point out important

[1] Typologies of violence are not the subject of this article. Our categories are pre-defined by the monitoring organization (see further below) that collected the data, but we will comment on some of its definitions. For typological reflections, see Tilly (2003) and, in the context of violence and development, Moser and McIlwaine (2006).

differences between Bayesian model results and those from the parametric models. We discuss the meaning of those results for development and violence studies and suggest some lines for future work, some of which are inspired by approaches used in other fields, such as disease mapping.

For space reasons, this article cannot address a number of relevant themes. The human security framework for this Cambodia data was given in our earlier work; Mack (2005: VIII) and Owen (2002) discuss reasons for isolating personal security and treating violence as a dependent variable in models of human security. The decades of violent conflict that decimated Cambodia have been chronicled and analyzed in many scholarly works, some of which are classics for the interested reader (Kiernan 1996: ; Heuveline 1998: ; Valentino 2004).

Violence and socio-economic development

Domestic, political and criminal violence are antithetic to human security. Research linking them to broader development concerns expanded in the 1990s (Heise, Raikes, Watts et al. 1994: ; Fajnzylber, Lederman and Loayza 1999: ; Ellsberg, Heise, Pena et al. 2001), in response to types of conflicts surging from the collapse of the Cold War order. Goodhand (2003) reviews the linkages between violent conflict and chronic poverty in both directions, concluding that the causal arrow from violence to poverty is stronger than vice versa.

The persistence of the violence-poverty nexus into the post-war era has multiple significance – war leaves behind a social structure that favors violent behavior beyond formal hostilities (Wood 2004), legitimizing violence in arenas other than in which is was expressly employed during the war – such as in party politics and among family members and neighbors. Secondly, conflict and violence in post-war society may escalate and push countries back into full-blown armed conflict; Deininger (Deininger and World Bank. 2003: 157) makes this argument even for minor land conflicts if left unresolved. Short of that, high post-war levels of violence feed on themselves and in the process perpetuate, or even create, high levels of poverty. Crime illustrates this process most readily. Mehlum, Moene, and Torvik (1999) demonstrate the theoretical possibility of crime-induced poverty traps; empirically, increasing returns to criminal activity have been shown for Colombia (Gaviria 2000). These arguments could be typified as "War causes post-war violence", "Post-war violence may cause war", and "War and post-war violence cause poverty".

The insight that post-war countries tend to have high levels of conflict and violence is not new. Just after the Vietnam War, Archer and Gartner (1976) demonstrated that post-war countries had significantly higher homicide rates than countries not participating in recent wars. Violent acts, however, are not evenly distributed within post-war countries. Their variation in space and alongside socio-economic and war-legacy conditions is a much younger field of study. Deininger (2003) spells out motivations for studies disaggregated within affected countries. Cross-country model findings are not sufficiently close to factors on which policy conclusions may hinge; they are too broad to unravel two-way causation paths between conflict and poverty. His findings, based on community and household data from Uganda, cut both ways. With conflict on the dependent side, he demonstrates that the factors to which political violence responds are not the same as those operating on crime aimed at individuals (this result is important for studies like ours that estimate effects on different kinds of conflicts and violence). Conversely, economic recovery is inhibited by local civil strife.

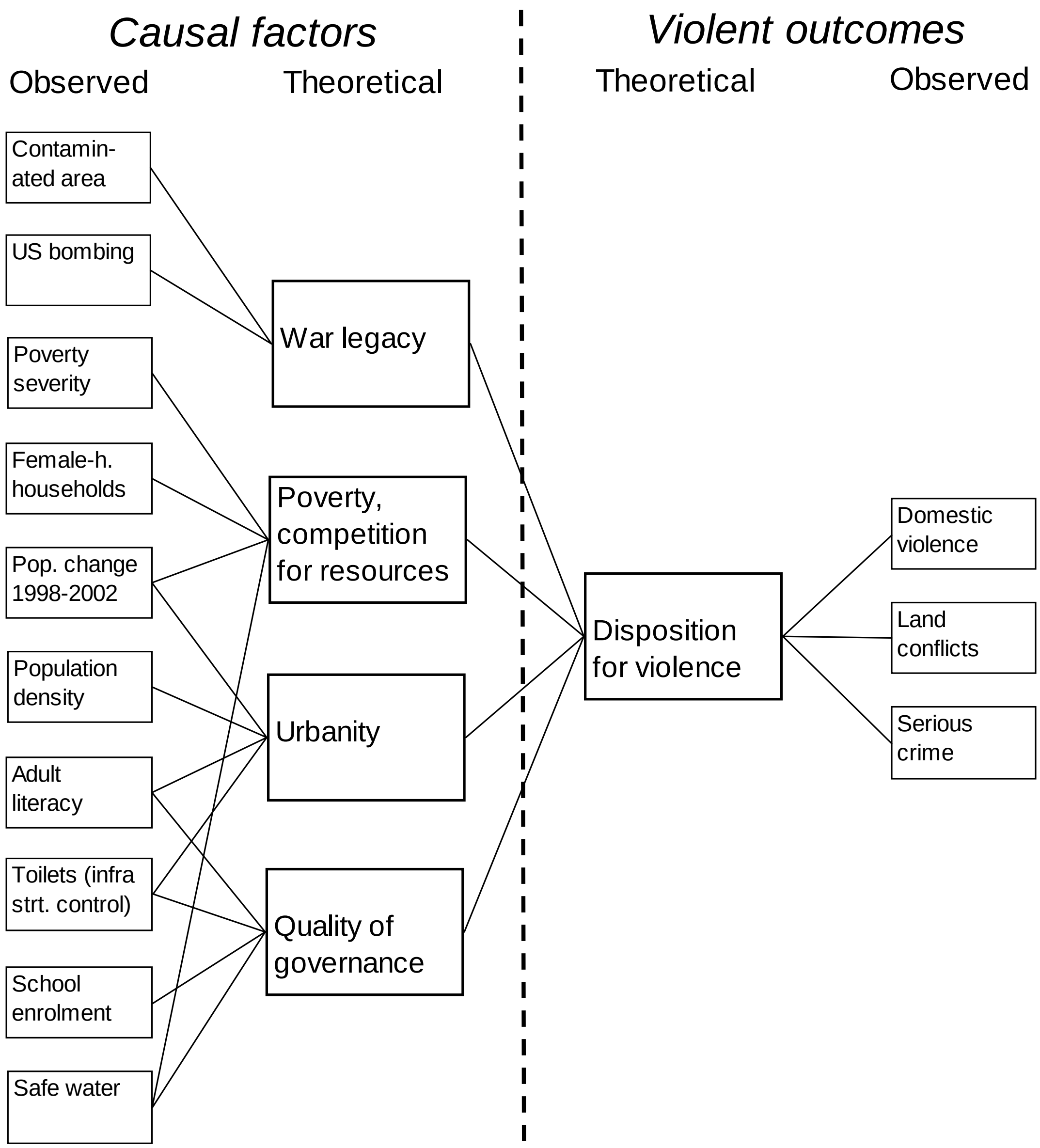
We highlight another recent micro-level study, for the size of the database as well as for some methodological concerns that it shares with ours. Barron, Kaiser, and Pradhan (2004) exploit data that a government census in Indonesia returned from more than 69,000 villages and neighborhoods, to find that seven percent of those units reported local conflict during the previous year. A quarter of the reported conflicts involved fatalities. These estimates result from key informants; the authors find significant underreporting when comparing those claims with the findings of more in-depth studies of a sample of communities. While definitions of conflict lack precision, quantitative analysis nevertheless establishes that "poverty by itself has very little correlation with conflict. Changes in economic conditions, on the other hand do. [For example,] … unemployment is universally closely associated with higher conflict rates" (op.cit., 31).

A feature increasingly seen in micro-level studies of violence and poverty is the explicit modeling of spatial dependence. The dependence may be substantive or simply a data problem. Certain types of perpetrators (e.g. robbers) move between communities and districts, constituting a human diffusion process. Neighboring units may also be correlated on unobserved factors that foster violence. Conversely, measurement error may be larger in certain contiguous areas, such as in crime underreported from sparsely populated rural communes. Studies that grow out of a sample survey tradition (the a.m. Uganda study in the World Bank's Living Standards Measurement Surveys) or the one from Indonesia with her non-contiguous island areas limit controls to second-level parameters (mostly as fixed effects for a district or region). In an intermediate step to spatial modeling, Demombynes and Ozler (2002), studying crime and inequality in post-apartheid South Africa, invent a makeshift "criminal catchment area" made up of neighboring police districts. More canonical formulations, using distance or adjacency-based measures, are still rare in the field; Sánchez, Diaz E., and Formisano (2003) analyze the relationship between the armed conflict and the different manifestations of violence and criminal activity in Colombia through a combined time and spatial lag model. Our models, while not dynamic, are spatial.

Causal domains and indicators

Our preceding parametric work sorted available socio-economic variables into four domains that we believe have a strong cause-effect nexus with local conflict and violence. Those are, as mentioned above, the legacy of the war, poverty and competition for resources, urbanity and the quality of governance. In this section, we discuss the theoretical importance of these domains, the indicators for which we have data and their relationship to the concepts, and refer to the descriptive statistics. Definitions and statistics for the three conflict types that respond to them will follow later. Some of our indicators are polysemous, meaning they relate to several domains. Literacy, for example, arguably is an aspect of urbanity, but it also an outcome of governance. This figure graph displays the conceptual links.

Figure 1: Conflict model schematic



War legacy

Like Archer and Gartner (op.cit.), Ember and Ember (1994) argue that war socializes violence, leading to the legitimization of violent interpersonal conflict resolution. Its mirror-image companion process is the destruction during war of social capital that leads to heightened risks of violence in post-war society (Colletta and Cullen 2000: ; Goodhand, Hulme and Lewer 2000). Moreover, massive proliferation of small arms may make violent resolution of personal, local and political disputes commonplace; this has been observed for Cambodia as well (Barnitz, Path and Catalla 2001).

Our study measures the war legacy at the commune level through two variables, the land contaminated with landmines and unexploded ordnance, as well as the intensity of the US bombing. The Cambodia Landmine Impact Survey reported that 6,422 out of 13,863 villages were affected by landmines or unexploded ordnance, that 2.5 percent of the country's surface area may be contaminated and that 5.1 million out of the 11.4 million population were at risk (International Campaign to Ban Landmines 2003). In the period 1965 to 1973, the US Air Force dropped over half a million tons of bombs onto Cambodian territory (this figure goes back to Shawcross (1987)) The bombing data is from the US Government (DSCA 2004) and is described in Miguel and Roland (2005: 48). For both variables, we use magnitudes rather than ratios to commune area or population; the rationale is given in Owen and Benini (2005: 16). What is missing for a fuller capture of the war legacy is data on the demographic footprint of war and genocide; in the extant population census data, it is not possible to separate lingering genocide effects from recent migration effects[2].

Poverty and resource competition

No less complex than those of the war legacy, the effects of poverty and resource competition on post-war conflict and violence are difficult to detail. Fajnzylber, Lederman, and Loayza (1999) investigate linkages to violent crime in a dynamic cross-country framework. They conclude that the income share of the poor does not affect crime rates once income inequality is accounted for. With land conflicts, poverty should be positively associated because marginal farmers will be socially weak and will be tempting targets for violent removal; they may also use violent means to defend their status (Simmons 2004). Finally, poverty is likely to exacerbate intra-household conflict because coping with extraordinary needs such as a health crisis is more difficult; this should manifest itself in more frequent domestic violence (for a review of the linkages between poverty and domestic violence, see Benson, Fox, DeMaris et al. 2003: ; Brush, Raphael and Tolman 2003).

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[2] At the local level, that is – the overall extent of the genocide is the subject of several country-level studies such as Kiernan (2003), Neupert and Prum (2005), and others based on sample surveys, e.g. De Walque (2005).

We use three measures to capture poverty and resource competition at the commune level – the severity of the poverty, the fraction of households headed by women, and the magnitude of the population change between 1998 and 2002 year. Three poverty measures – head count, poverty gap and poverty severity – were estimated for each commune for the year 1998, using small-area estimation techniques that combine sample survey and population census data. The methodology is described in detail in Fujii (2003). Severity, which emphasizes the number of the very poor, as the measure of choice was adapted on account of the stronger relationships that it produced in our parametric models; this is in line with the a.m. findings by Fajnzylber et al., op.cit. Since 1998, no further countrywide commune-level poverty estimates have been produced (Do 2005).

As an additional poverty measure, and one that is thought to capture levels of absolute destitution (Devereux 2003), we also use the proportion of female-headed households. We use estimates by the same local key informants that supplied the conflict counts. Assuming that rapid population influxes lead to keener resource competition, we use the logarithm of the ratio of the 2002 to 1998 local populations as an indicator of this poverty facet. The logarithmic transformation was chosen also out of the need for a balanced gain/loss measure, in other words, one that captured the intensity of the change in both directions, giving, for example, the same "weight" to a 50 percent loss as to a 100 percent increase. During this period, relative population growth has been stronger in rural than in urban communities; this indicator therefore is not empirically tied to urbanity.

Urbanity

The use of "urbanity" as a concept with explanatory power for post-war violence may surprise readers. This takes into account the mixing of two processes confounded with community population density that may have opposite effects on violence. In the long-run evolution of the modern nation-state, at least in Europe, violence was primarily a phenomenon found in the poor rural areas at the periphery (Eisner 2001). In this line, cities, with their cultural and institutional resources for conflict management, are thought to be exerting a civilizing, violence-repressing influence. The counter-argument is more recent and is made more often by observers of rapid urbanization in poor countries. "When combined with economic stress, state failure and increasing calls for democratization, urbanization may increase the tendency to unrest and violence" (Koppell and Sharma 2003: 67) summarizes it as a set of interaction effects. In either case, there are reasons to expect non-linear effects from urbanity variables (Gillis and Regoeczi 2000: ; Osgood and Chambers 2000), some of which may result from different dynamics that drive rural as opposed to urban violence (Villarreal 2004).

We measure urbanity as literacy, population change and population density. The literacy definitions were construed on the basis of 1998 census variables (persons with at least 6 years of schooling in given age-gender brackets) rather than using the key informant estimates contemporary with the violence years. We find the latter to be so unrealistically high that we prefer to stay with census-based levels from some years ago. This is a similar drawback to the use of the 1998 poverty estimates. We calculated separate rates for male and female literacy; in our final models, we only use male literacy. The reason is that across a variety of parametric models and influence analyses coefficients on female literacy did not produce any intelligible patterns. The population change measure was described above; because of strong intra-rural migration, it belongs more to the poverty and resource competition domain. Population density data is contemporary, i.e. based on population estimates offered by the village and neighborhood leaders in each of the years for which the violence was monitored.

Quality of governance

The quality of governance as a key factor in solidifying peace and reconstruction has received growing recognition in post-Cold War years (Chr. Michelsen Institute 2004), leading to a donor boom in funding related initiatives. These extend to the local government level (Woodward 2002), in Cambodia notably through the vast decentralization initiative that, among other things, produced the violence data for this study (Andersen 2004).

Theory-supported variables at the local level are not easy to come by for the measurement of governance quality. Our leading idea is to use service level variables that are shorthand for the penetration of local communities by programs emanating from the international community or from higher levels of government. While we cannot measure the *quality* of services, it is plausible that *levels* are highly correlated with larger conditions conducive to non-violent conflict resolution, such as access to law enforcement and the quality of local arbitration.

Given data limitations, two indicators are constructed from key informant-supplied data to capture governance quality – the fraction of households with access to safe drinking water, and the proportion of girls aged 6 – 14 who attend school. Safe water provision is a technology that exceeds the self-help capacity of local communities; as a measure of good governance it has received some validation in recent work on how post-apartheid social policies improved service provision in poor South African communities (Beall, Crankshaw and Parnell 2000: ; Cavill and Sohail 2003). We use female school enrolment rates because effective governance implies the ability to counteract school dropout, which is higher for girls than for boys (McGrew, Frieson and Chan 2004: 21). To distinguish governance effects from infrastructure effects, we use the fraction of households with sanitary toilets as a control. This latter technology, while responding to public health campaigns, is more flexible and more easily adopted by individual households; it is also more strongly correlated with population density than the two governance indicators.

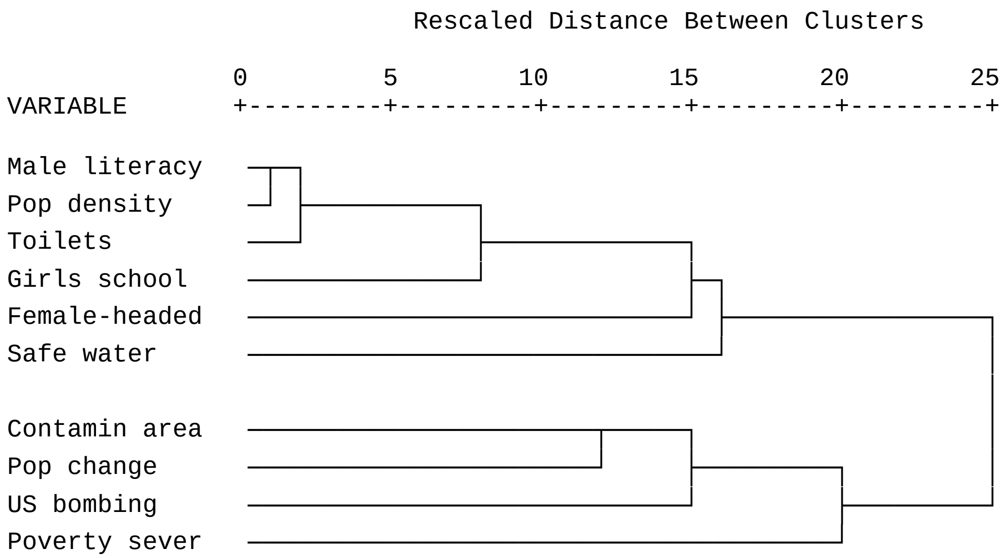
Associations among covariates

Data sources and descriptive statistics for ten covariates used in the final models, assigned to four likely causal domains, are given in Table 2 in the appendix. The statistics are for the year 2002 subsample to make it easier to compare counts to the number of communes in the country. The models, described further below, use 3 x 1,628 = 4,884 observations pooled for the years 2002 – 2004; 4,753 have complete values. Statistics for the two alternative poverty measures (head count and gap) and for 2002 male literacy (not used because we consider it highly exaggerated) are included for comparison. The exposure variable (households = household years for a one-year period) is also summarized.

For validity concerns, we study the associations among the variables included in the final models. We use hierarchical cluster analysis of their normalized values. We prefer this exploratory procedure to principal component analysis because it does not require linear assumptions; the normalization gives equal weight to each variable, while maintaining some sensitivity to extremes. The dendrogram below suggests two sets of loosely associated variables: indicators of urbanity and governance quality form one cluster; those operationalizing war legacy as well as poverty and resource competition are for the most part in the second.

Two covariates are in unexpected positions. Local key informants in sparsely populated communes tended to estimate low fractions of female-headed households. As a result, this variable appears tied up with urban conditions. Recent population change is in the same large cluster as poverty severity, but chiefly because growth has been stronger in rural than in urban areas, and this places it closer to landmine and UXO contamination.

Figure 2: Hierarchical cluster analysis of the covariates, dendrogram



Our four causal domains are thus separated not only by the associational pattern of their indicators but also by amenability to policy (governance more so than urbanity) and by timing (bombs were dropped, and mines planted, before the time when population change and poverty were measured). The war legacy variables are the only truly exogenous ones.

Conflict and violence rates

The SEILA monitoring system

The conflict and violence data used in this study has been collected by the monitoring arm of the Government of Cambodia's large decentralization program, known by the Khmer Sanskrit word for foundation stone, SEILA (Andersen, op.cit.: 2). The enormous range of this monitoring effort contrasts with a notable lack of public documentation of its methods. While reports on various other substantive areas of the SEILA-run monitoring activities are available at the official Web site (SEILA 2005), our understanding of the conflict and violence data generation process was formed during two visits in its headquarters by one of us (Owen, in 2003 and 2005). By 2005, SEILA had collected key informant estimates of domestic violence, land conflicts and serious crime in four annual drives for the years 2001 to

2004. We were given to understand that the 2001 data was not good. However, SEILA used it in order to give written feedback to most of the commune authorities, offering some comparisons of conflict figures. While we have not seen any of those reports, SEILA staff were unanimous in stressing that these meetings during 2002 were very motivating for commune authorities, who took their participation in subsequent years much more seriously.

We use the data collected for the years 2002, 2003 and 2004. Estimated counts were elicited from headmen and leaders of over 13,000 villages and urban neighborhoods. The nature of units counted varied between types of conflict. Authorities estimated for their small local communities in the past calendar year:

- The number of households known to have domestic violence problems
- The number of land conflicts
- The number of serious crimes committed. These included murder, robbery, rape and serious theft.

SEILA managed the data in year and province-wise spreadsheets which identify participating units by name and census ID. We aggregated counts to the commune level because some of the covariate data disaggregates only as far as the communes. Also, communes are geo-referenced with polygon shapes while we do not even have point coordinates on the villages.

Conflict rates

The implications of the conflict definitions and data sources will be discussed after a brief explanation of the rates. Because domestic violence was counted as affected households, we use households as rate denominator for all three types. The number of households was reported by the same key informants who supplied the conflict counts, in each year.

Table 3 in the appendix supplies the means, population-weighted means, maximum and coefficients of variation (standard deviation / mean) for commune-level rates together with the fraction of communes that estimated zero events during the reporting year. At this point, some graphic ratios may help the reader:

During an average one-year observation period, roughly one in every 55 households had domestic violence problems, the ratio of land conflicts to households was close to 1 : 100, and the corresponding ratio for serious crime to households was close to 1 : 170. The ranges, however, are considerable; the maximum ratios are 1 : 4 for domestic violence, 1 : 2 for land conflicts, and 2 : 3 for serious crime. The mean rate for all three kinds of conflict declined from 2002 to 2004; for measurement error reasons explained further below, however, we do not attempt dynamic explanations.

Validity and reliability concerns

From a human security perspective, the ability of the Cambodian government to extract conflict estimates from thousands of local communities virtually covering the entire nation attests to a considerable (and, given history, non-trivial) degree of administrative penetration. One may speculate whether the mere government-supervised collection activity brings local communities a measure of protection from violent conflict. As Soares (2004) has shown for crime reporting, the degree of conflict reporting is itself determined by the institutional development of a country. Similarly, one may assume considerable variation within countries, across types of conflicts, and according to whether conflicts are reported case-wise (such as by victims to police) or are the subject of collective estimates (as in our case, by local key informants).

Key informant based systems like SEILA's pose both validity and reliability issues. As Barron et al. (op.cit.) noted for Indonesia, conflict definitions are vague. The relationship between conflicts and violence is largely undefined, and except for domestic violence, it is not known what proportion of conflicts involved violence. There are no additional data on fatalities and injuries. Basic observation units differ; they are: affected households (domestic violence), events (serious crime), and possibly multiple units - disputed plots, concerned parties, new cases (land conflicts). "Serious crime" mixes crimes against persons with some types of property crimes. Since it is well known that violent and property crime rates differ in their social-structural determinants (e.g., Kelly 2000: ; Andrienko 2002), mixed estimates are likely to compromise explanations of varying crime rates. A more general criticism may be leveled at the typology of conflicts used by SHEILA. Ninh and Henke (2005), in a study of commune councils, surveyed the prevalence of twelve different types of local conflict. 24 percent of the polled voters held that youth gangs were affecting their families most, outranking the three types of conflict captured in the SEILA reporting (land conflicts: 18 percent; domestic conflict: 15 percent; "other crime": 7 percent).

The SHEILA categories are a-priori for our study, the result of the particular cognitive workflows organized under the vast decentralization endeavor. How reliable are the estimates from the cooperating village and neighborhood leaders? From an autonomy and resource-balancing perspective, one would expect leaders to underreport conflict and to remain ambivalent about social problem and service levels – enough to show progress, but short of disqualifying from targeted aid. But observers of post-war community life have also found that leaders and key informants, in response to the local social changes brought about by the war itself, learn a conflict and human rights discourse of their own, and that their reporting tends to be sincere (Archibald and Richards 2002).

Regardless of motivations, key informant estimates are socially conditioned, and one expects systematic differences from other methods. Two examples must suffice here: SEILA's estimates of families with domestic violence problems in 2002 work out as a 2.0 percent population-weighted mean; by contrast, Bott, Morrison, and Ellsberg (2005) report a 2000 survey that found, in a national sample of 2,403 women aged 15-49, 15 percent assaulted by a partner in the previous 12 months. The male literacy rate (men aged 15+ with 6+ years of schooling) in 1998, based on the population census, was 34 percent; the key informant based estimate for 2002 was 79 percent. Improvements on this scale are not plausible.

We deal with the validity and reliability threats in the following manner. First, for two important covariates, we opt to use the 1998 census-based measures, rather than the more recent SEILA data. For male literacy, we use the census-derived rates because they appear more reliable. For poverty measures, we use the World Food Program estimates extrapolated from census variables although economic growth and migration may render them obsolete. An alternative approach might seek to construct asset-based wealth measures (Filmer and Pritchett 2001) from the 2002-2004 data on durable consumer goods prevalence. However, SHEILA used different items for urban and rural communes, and we have not been able to find anchors for scale equivalency. Also, a commune-level wealth index would not be conceptually equivalent to the poverty *severity* measure used in our models.

We assume that measurement errors in conflict estimates are correlated in ways that the models can partly control. Since instructions for the key informants were passed down a hierarchy, we assume that errors correlate within administrative units (analogously to supervisor error in sample surveys; see Bassi and Fabbris (1997: 733)). Moreover, since communities assess the consequences of sharing information with outsiders in the light of previous experience and the way they understand current instructions, errors should also have a year-wise component. This differs from the assumption in Fajnzylber et al. (op.cit.: 9) that measurement error varies little over time.

The measurement error can thus be subdivided into four components – the mean national (yet conflict-specific) rate of underreporting, the error specific of administrative units that supervise data collection, year-wise influences, and the individual error at the lowest level. In our models, this level is the commune.

In response to these caveats, our models include fixed effects of the observation years (with 2002 as the base) as well as spatially unstructured effects of the districts to absorb part of the measurement error. In addition, we introduce commune-level adjacency-based (i.e. spatially structured) effects. These spatial effects, we believe, reflect substantive influences more validly because measurement errors across district boundaries should be less strongly correlated.

Bayesian analysis

Our previous parametric work relied on negative binomial regressions, as frequently used in count variable models, especially to overcome the difficulties arising from unobserved heterogeneity in the data. Pedersen Dyreng (2004) used the year 2002 segment of the SHEILA data to probe the limitation of the parametric approach. She demonstrated that a number of effects were no longer significant once spatial and random effects (her models placed both at the district level) were taken into account. She also found a small number of covariates with distinctly non-linear effects, although only poverty severity and contaminated area had effects with credible intervals outside zero, the first only on land conflict, the second on land conflict and domestic violence.

Her results have motivated further Bayesian investigation of our full (2002 – 2004) data set for the reasons that Fahrmeir and Osuna (2005: 1) enumerate: Parametric models may suffer from "individual unobserved heterogeneity caused by omitted covariates, temporal or spatial correlation, and possibly nonlinear effects of metrical covariates or time scales." Moreover, it is usually "very difficult if not impossible to specify nonlinear effects of metrical covariates or of time scales and, in particular, spatial effects a priori through conventional parametric functional forms".

Semi-parametric Bayesian analysis allows to mitigate those defects. Its advantages for unraveling causal complexity have been highlighted in political science (Jackman 2000), sociology (Western 2001), criminology (Law and Haining 2004: , with special emphasis on spatial effects) and in epidemiology. Bayesian disease mapping (Best, Richardson and Thomson 2005) may be the field closest to our research situation, thanks to its methods for concurrent and spatial modeling of several diseases. For background on some of the most prominent modern algorithms through which Bayesian analysis is implemented, we refer again to Jackman (2000: ; 2004: , notably for Markov Chain Monte Carlo simulation) and to Brezger, Kneib, and Lang (2005b) (for spatial effects modeled by Markov random fields or two-dimensional P-splines). As mentioned before, the count variable process and our desire to incorporate spatial structures have motivated the choice of these methods over other non-parametric methods, some of which offer more convenient computation and output formats[3].

The model

In the first stage of the Bayesian hierarchical model, we impose distributional assumptions on the observed data. To ease the notation, let y be a generic observation from commune i at time t. We assume that the likelihood is a Poisson-Gamma mixture

$$y \sim \text{Poisson}(\eta v), \quad v \sim \text{Gamma}(\delta, \delta) \qquad (1),$$

which can equivalently be expressed in terms of a negative binomial model for y. In our case both η and δ are unknown. The interpretation is that the data y arrives from a Poisson distribution with mean ηv where v is $G(\delta, \delta)$-distributed. Such a formulation extends the widely used Poisson-regression to include an extra vector of multiplicative Gamma-

[3] We estimated our model using the freeware program BayesX (Brezger, Kneib and Lang 2005a). Typically, a single model run, with 100,000 iterations, in BayesX would take several hours, differently from, say, a multivariable smoothing procedure such as STATA's mrunning (Royston and Cox 2005), which would execute in less than a minute (but would not consider spatial dependence). In turn, BayesX is much faster than the most widely used software for Bayesian inference, WinBUGS (Brezger, Kneib and Lang 2005b: 8).

distributed random effects. The multiplicative random effects were shown necessary to fit the observed data, and a pure Poisson likelihood was not appropriate.

Both η and v are unknown, but only η depends on the observed covariates at each commune i and of time t. Further, v is a "random effect", hence independent for each observation.

At the second stage, the log-link is used to relate η to observed covariates, as follows:

$$\begin{aligned}\log(\eta) = \quad & \text{constant + year effect + offset + linear terms} \\ & + \text{ semi-parametric terms + spatial term} \\ & + \text{ random effect at district level.} \qquad (2)\end{aligned}$$

Each term is now discussed in more detail:

Constant This is an overall unknown constant and common for all communes i and times t.

Year effect We add unknown constants for data related to year 2003 and 2004, thus we treat the year-effect for year 2002 as zero.

Offset This variable is fixed and equals the log of the number of families in commune i at time t. The rationale is that the expected number of events y is proportional to the number of families under a Poisson likelihood.

Linear terms This term is the sum of the linear effects of (possibly transformed) covariates x_j , hence its $\sum_j \beta_j x_j$, where x_j is the covariate j for commune i and time t. The coefficient β_j is unknown and is constant for all i and t.

Semi-parametric terms This term is the sum of semi-parametric terms of covariates x_k . Rather than a strict parametric form for how covariate x_k influences log η, we impose a smoothness restriction: whatever functional form the effect of x_k has on log η, a smooth curve is assumed. The degree of smoothness is considered as an unknown, too.

The term “semi-parametric” relates to the fact that although the model for the effect of x_k is in fact parametric in a strict interpretation, it behaves similarly to non-parametric models. The effects are constructed by means of P-splines; for technical details, see Brezger and Lang (2006).

Whether the effect of a covariate should be linear or semi-parametric was determined in two stages. First, in models with only semi-parametric effects of continuous variables, those with approximately (from visual inspection) linear effects across all three types of violence were made linear in all subsequent models. This was done chiefly to shorten estimation times. Second, for covariate with each semi-parametric, model fit comparisons were made with models that specified this particular effect linear; these are discussed further in the appendix.

Spatial term To account for possible unobserved covariates with a spatial trend, we include a spatial term in the model. This (Gaussian) term introduces spatial dependence between the communes by assuming that the spatial effect in commune i, u_i - if we know the spatial effects u_j in the m_i neighboring communes {j} - is drawn from

$$u_i \sim \mathrm{N}\left(\frac{1}{m_i}\sum_j u_j, \frac{1}{m_i}\right).$$

Hence, the spatial effect in commune i is assumed to be conditionally independent from the remaining communes given the spatial effects of the neighboring communes. Nevertheless, from an unconditional perspective, all communes are in fact spatially dependent according to this model specification. From the conditional distributions, the joint density for u can easily be obtained. The scale of the spatial effect is considered unknown, too.

Random effect on district level We use a district-level Gaussian random effect to account for correlated measurement errors that occur within all the districts at all three years.

The Bayesian model is completed by specifying, as the third stage of the Bayesian hierarchical model, priors for the parameters of the distributions at the second stage. Here, we use vague priors, meaning either zero-mean Gaussians with huge but finite variance for signed parameters, or vague Gamma distributions for the unknown precisions.

For the default values used by BayesX, see Brezger et al. (2005a). Regarding inference, model run details and model selection considerations, see the annex.

Results

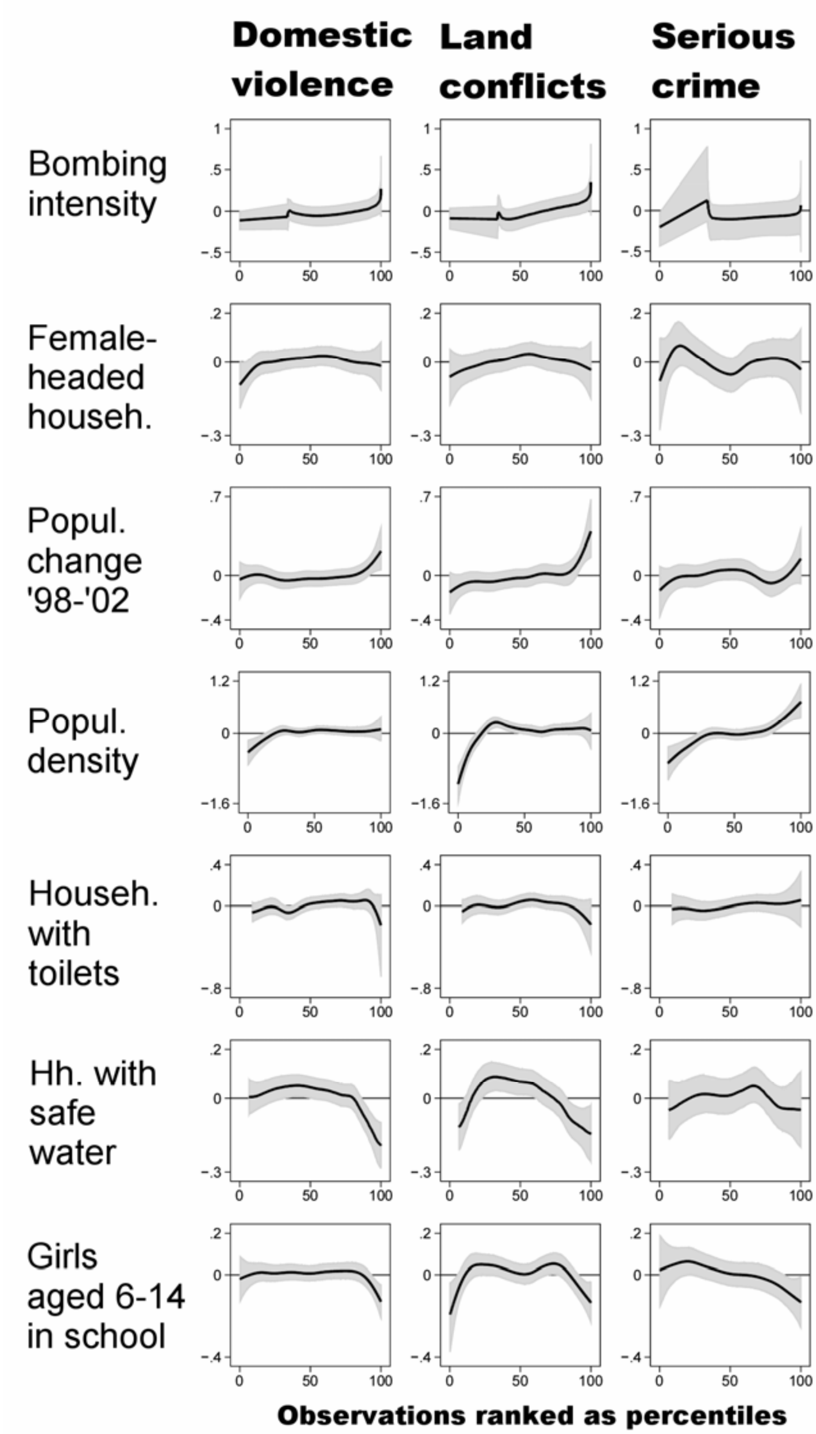
We report the effects of the year and socio-economic variables as well as of district membership and commune neighborhood. The term “effect” is used abundantly in this paper; in the model context, it strictly means the contribution to the linear predictor of the conflict rates. “Credible interval” is another frequently used term; notwithstanding nuances, it is the Bayesian analog to confidence interval.

For example, the effect of the US bombing intensity on domestic violence is -.1123 for the 546 communes not subject to air strikes. In other words, at this lower extreme, the model predicts about 11 percent (= 1 - exp(-.1123)) less domestic violence than for the average commune, all other things being equal. This effect is significant because the credible interval at this point is [-.2262; -.0030]. At the other extreme, for the five communes on each of which more than one hundred million pounds of bombs were dropped, a mean effect of +.2007 corresponds to roughly a 22 percent increase. This effect is not statistically significant; because of the small number of observations in this range, the credible intervals contain both positive and negative values.

Tables are in the appendix; here we briefly describe their contents. Year of observation-fixed and linear effects, comparing the latter to the 2002 parametric model results, are presented in Table 4. Cells with significant results are shaded. The non-linear effects are presented in two formats. Ranges of effects and fractions of observations for which the 95-percent credible intervals are outside zero are given in Table 5. Here, too, the reader can compare with the results of earlier parametric models.

The reader may find it much easier to grasp the shape and strength of the non-linear effects by comparing graphs with identical y-scales for a given covariate, and x-axes showing all covariates as percentiles. The graphs are arranged in matrix form (Figure 3, next page), with covariates as rows and types of conflicts as columns.

Figure 3: Matrix graph of the semi-parametric effects



Note: Covariates were track-ranked (the lowest value was ranked 1), then transformed to percentiles. As a result of numerous zero values, the lower bound of some ranges (e.g. households with toilets) is at a visible distance above zero. We made an exception for bombing intensity (34 percent of the communes did not suffer air attacks), replacing the lowest percentile with zero so that the credible intervals at that point can be distinguished.

We then review results by causal domain in the order of the theoretical exposition. Statistics of the overall model performance are presented in the segment on spatial effects; they are meaningful in comparisons of models with and without commune level spatial term.

War legacy

The Bayesian analysis does confirm the lingering effects of the war legacy that the parametric work revealed. On all three conflict rates, the effect of the linearly modeled landmine and ordnance contaminated area is significantly positive. It is small, however. The rate increase associated with a one-standard deviation change of the contaminated area magnitude is between 5 (domestic violence) and 12 percent (land conflicts).

The effects of the bombing intensity are non-linear. They manifest themselves at or near the extremes of its ranges. In terms of domestic violence and serious crime, communes not bombarded are better off – their predicted rates are 9, resp. 12 percent lower than those of the rest; and this difference is significant. The parametric models had not caught these effects. The effect on land conflicts is significantly positive towards the high extreme; for this group (about 2 percent of the communes, each the target of over 28 million pounds of bombs) we note an approximate 25 percent increase in land conflicts. The parametric model too reports such an effect on land conflicts.

Poverty and resource competition

In the second of the causal domains, poverty and resource competition, the results of the Bayesian and parametric analyses are remarkably different. The Bayesian results are more conservative.

First, poverty severity has a positive effect on all three types of conflict, in both types of models. In the parametric model, the effect is statistically significant for land conflict. In none of the Bayesian models is the coefficient significantly different from zero[4].

[4] The effects on domestic violence and land conflicts just barely miss the significance mark. In the Bayesian estimates of these coefficients, 5.2 percent are below zero for domestic violence, and 5.8 percent for land conflicts. The percentage for serious crime is 14.9.

Second, higher proportions of female-headed households, a measure for absolute destitution, are significantly associated with higher violence rates in all three parametric models. The Bayesian analysis produces non-linear effect curves, with effects decreasing towards the high end – perhaps as a result of the scarcity of male perpetrators. These effects, however, are not significant, with a minor exception: Domestic violence is less pronounced in communes that reported a very low proportion (less than 7 percent) of female-headed households. The predicted rate here is about 12 percent lower than for the rest of the communes.

The third indicator used for this domain, the magnitude of population change, fully proves its usefulness in the Bayesian context – the non-linear character of the effect is clearly apparent. The parametric models returned significant positive effects on all three types of violence. The Bayesian models, by contrast, show steep take-offs in domestic violence and land conflict rates (and, though not in statistically credible manner, the crime rate). The rates take off beyond a threshold, near 25 percent population increase between 1998 and 2002. . In these fast-growing commune populations, the effect on domestic violence is a 17 percent rate increase; for land conflicts, it is 30 percent.

Urbanity

In the urbanity realm, the Bayesian methods overcome the problem that our parametric models encountered. Following the advice of Gillis and Regoeczi (2000) that non-linear effects were to be expected from population density to violence, the parametric models included a squared term. These models could not be executed meaningfully[5]. In the Bayesian models, population density effects are strong, strongly non-linear and moreover remarkably different for the three conflicts. The effects climb steadily in the first quartile, from 0.2 to about 70 persons per sq km. Thence they flatten out, in fact, in the case of land conflicts, at first slouch back. For serious crime, and only for it, a strong positive effect takes off in the fourth quartile, starting roughly at 300 persons per sq km.

Literacy is the other urbanity indicator used here. The final Bayesian models specify the male literacy rate as linear (previous models had failed to show any remarkable non-linear effects). Both the parametric and the Bayesian model coefficients are significantly negative for

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[5] The estimation would not converge with the district random term included. When this term was removed, one of the two density coefficients was significant, and all significant ones were negative – a counterintuitive result.

domestic violence. Under the Bayesian model, an increase from 33.6 percent male literacy (the 1998 census mean) to 50 percent would reduce predicted domestic violence by 1/exp(-0.588 *(0.5-0.336)) – 1, or approx. 10 percent. Neither approach produces any significant associations between literacy and the other two types of violence.

Quality of governance

On the two indicators that measure quality of governance –access to safe water and girls' primary education -, strong non-linear effects abound. These effects persist when the proportion of households with sanitary latrines is included in the models as a control for infrastructure effects.

The effect curves are all, though to different degrees, shaped as inverted U. Safe water produces significant positive effects in the middle range and negative effects at one, resp. both extremes in the domestic violence, resp. the land conflict models. The shape of the effect on serious crime is similar although the credible interval is nowhere outside zero. The parametric models were able to demonstrate effects of safe water provision in one direction only – and they were not consistent (positive on serious crime, negative on land conflicts and domestic violence).

High female school enrolment is associated with reductions in all types of conflict. Significant effects are evident in the highest decile. This contains the communes that reported enrolment upward from 96 percent. A dampening of the land conflict rate is at work at the low end as well – significantly so for the 5 percent communes that reported less than a 50 percent enrolment. In parametric models, higher female enrolment has a significant negative effect only on serious crime.

The non-linear effects in the governance domain are numerous; thus a synoptic table may be helpful. Governance variables are the ones most amenable to policy; an intriguing aspect are the sharp take-offs of their negative (i.e., conflict-dampening) effects.

Table 6 locates thresholds from visual inspection of the "kinks in the curves" (to the nearest decile boundary) and gauges the effect difference from the kinks to the 100 percentile points. We find that five out of six effect curves show a pronounced take-off in the higher ranges, and four of those come with credible intervals outside zero. The effect of girls' education on crime grows negative more gradually, without a sharp kink. The predicted reductions in conflicts are between 16 and 22 percent. Compared to the effects of population change and population density, these are small. We discuss their significance further below.

Spatial effects

To repeat, our models take spatial effects into account in two different manners. District-level influences are modeled through a random term and are interpreted primarily as a part of the measurement error induced by reliance on local key informant estimates. Certainly, there is also a substantive part involved – resulting from the quality of law and order, or the efficacy of land titling. The unobserved aspects vary across the country, but may be relatively uniform within a district. They remain confounded with the measurement error.

At the commune level, we model a structured spatial effect, which we consider genuinely substantive rather than a data issue. Its relative strength can be assessed by comparing the standard deviations of the effects from all the model terms, as in Table 7. For example, in the land conflict model, these quantities read 0.65 for the spatial effect, 0.10 for the district effect, 0.11 for the contaminated area, 0.26 for the population density, but only 0.06 for girls' school enrolment[6].

Across the models, the commune level spatial effects are stronger than any of the other effects. This is a consequence of three factors. To estimate the local effect for each commune, only three observations were available – the event counts of the neighbors in each of three years. Second, the effects capture the influence of all unobserved variables to the extent that they are spatially correlated. One would assume that strong spatial correlation obtains in unobserved war legacy such as the fraction of the local population killed or displaced in the genocide, but also in contemporary conditions such as exposure to natural disasters.

Third, while district membership and commune neighbors were mapped virtually without error, measurement error in the substantive covariates is unknown. Yet, for the same reason (key informant estimates), it may be considerable. If so, the errors attenuate these estimates.

[6] Statistically it would have been more correct to decompose the variance of the linear predictor into the contributions from each model term, but this quick, if naive method seems appropriate for the purpose here.

We tested the plausibility of these assumptions by running models identical but for the commune-level spatial term. We compare them, within each conflict type, on the signature statistic for the overall MCMC model performance – the so-called "deviance information criterion (DIC) based on the saturated deviance" (Spiegelhalter, Best, Carlin et al. 2002: 24). As a measure more easily accessible to readers, we also calculate the Spearman rank order correlations between predicted and observed rates. These statistics are in Table 8. The deviance statistics suggest that the land conflict model performs the best. Its fit statistic increases by 17 percent by including the commune level spatial term; and the rank order coefficients soars from 0.48 to 0.84. This is an indication of overfitting; for, as mentioned, for each commune the spatial effect was estimated from three observations only.

Moreover, visual inspection of the effect curves of the models without the commune level spatial term (not shown for space reasons) reveals that non-linear effects tend to be relatively stronger, and the covariate ranges with effects credibly different from zero are longer. For example, in models that include the commune level spatial effect, the fraction of female-headed households has a credible effect only on domestic violence. In models without, the effect on crime, too assumes a credibly non-linear shape.

All that makes for a twofold interpretation. The spatial terms lend further credence to those covariate effects that "survive" their inclusion in the models. In the above example, communes with rare female-headed households were associated with reduced domestic violence, and this effect stays significant when we control for unobserved spatially correlated influences. It should be noted that the inclusion of the spatial effect actually leads to more reliable conclusions since it accounts for the spatial correlations in the data and therefore leads to realistically wide credible intervals. Excluding the spatial effects gives an overly optimistic impression on the precision of the estimates since spatial correlations effectively reduce the available sample size.

Second, some of the unobserved factors captured in the spatial terms should impact on the three types of conflicts in similar ways. If so they should be strongly and positively correlated. Equivalently, the residual conflict rates in models without the commune-level spatial term should display this correlation pattern.

Table 1: Correlations of spatial effects between conflict types

	Domestic x land	Domestic x crime	Land x crime
Commune level spatial effect	0.49	0.39	0.34
Residual rates in models without that term	0.37	0.22	0.24

This is the case within limits only, as the above table makes clear. The correlations are, given the nationwide coverage, significant, but they are not overly strong. While there is a common spatial component across the three conflict types, domestic violence and land conflicts are more closely related to each other than either of them with serious crime[7]. This similarity pattern obtains, by and large, also for the covariate and year effects.

Discussion

Substantive aspects of human security

Human security in Cambodia, as in other post-war countries, depends on sufficient incentives for peaceful livelihoods to discourage the resumption of violent conflict. That there should be a wealth of significant relationships between socio-economic conditions and conflict rates, therefore, is not surprising.

What is surprising, however, is the relative strength among effects of different factors and the strongly non-linear shape of some of them. Also notable are the location of thresholds below or above which we have noticed steep take-offs, and similarities across conflict types.

Lingering war effects come in two flavors. Those from contamination with explosive remnants of war are linear across its magnitudes and on all conflicts. They are strongest on land conflicts, as one would expect from the land-bound nature of the hazard. By contrast, the effects of the US bombing seem limited to the extremes of intensity. Communes that were never bombarded reported lower domestic violence and serious crime whereas land conflicts

[7] The correlations among residuals are lower than those among the commune level spatial effects because in the model without this term, the district-level random term picks up some of this spatial information. For example, for domestic violence district effect standard deviation increases from 0.106 (with commune level spatial effect) to 0.338 (without).

have responded at the high end. One may speculate that communities with the most severe losses and dislocation find it the hardest to resolve land claims among survivors and returnees.

Although Cambodia as a nation remains deeply marked by the war and genocide experience, the limited effect of the bombing should give pause. It throws up the question (which a cross-sectional study cannot answer) whether the association between historic war intensity and contemporary conflict is diminishing. In neighboring Vietnam, Miguel and Roland (op.cit.) found that the US bombing had no significant impact on local development levels twenty years later. This does not speak to conflict directly, and the historic distance is longer in Vietnam, but with high levels of internal migration and progress in mine clearance, a diminishing effect seems plausible on conflicts in Cambodia too.

The three indicators in the poverty and resource competition domain are faring unevenly. The Bayesian spatial models make it less certain that the severity of poverty aggravates conflicts; still, its effects on domestic violence and land conflicts are not negligible. A significant non-linear signal travels from the proportion of female-headed households, considered shorthand for absolute destitution, to domestic violence, but not to other conflicts. This plausibly hides reverse causality, with higher domestic violence causing more household break-up. These effects are outdone by those of the local population growth in the four years preceding the conflict monitoring period. Population growth was stronger in rural than in urban areas, and thus the conflict-inciting effects of rapid growth reflect competition for resources in rural milieux more than any urbanization processes.

In the urbanity domain, population density strongly affects the propensity for conflict. The effects take different shapes across conflicts. For example, land conflict rates are much lower in sparsely populated rural communes. The rates hit an upward bump in the second density quartile, only to flatten out as we move to higher ranges, where presumably larger settlements offer more livelihoods that are not land based. Only for serious crime does the curve soar in the high-density range, departing from Broadhurst‘s observation that homicide rates in the nineties were lower in urban areas (Broadhurst 2002). The effects of literacy are similarly varied; higher male literacy is associated with a reduction only in domestic violence. We may thus conclude that in Cambodia urbanity, as an institutional complex, is impacting human security in ways that are far from uniform across types of conflict and violence. Intra-rural differences are more important than rural-urban contrasts.

The last of our four causal domains, the quality of governance, is particularly rich in non-linear effects. But this is also the area that calls for the strongest caveats, for two reasons. First, the models do not capture mutual causation between governance and conflict propensity. Second, we assume that the indicators are graded by governance content. The adoption of sanitary latrines is helped by public health campaigns and building ordnances but is essentially left to residential owners. Latrine adoption rates can thus be treated as an infrastructure control. Provision of safe water exceeds the self-help capacity of households and even of most local communities. Girls' primary education ups the ante one more step: it requires not only the organizational continuity of schools but also incentives to retain students. That the effects of safe water and girls' education are stronger than those of toilets speaks for the validity of the governance indicators.

The provision levels at which conflict-reducing effects take off are relevant in a policy perspective. On domestic violence as well as on land conflict, the impact drops starting from relatively high levels of provision, achieved only in approx. one fifth of all communes. In addition, land conflicts show an even more dramatic *increase* at the low end of service provision. This applies also to crime, but faintly so, and even less to domestic violence. One possible interpretation is that as a village or urban neighborhood partners with government and NGO programs, its local politics turns more conflictual over bids to control external resources. Conversely, outside agencies may pick up conflict signals and focus projects on communities with more dramatic conflicts in hopes to resolve them or to mitigate causes.

One of the implications seems to be that domestic violence, land conflict and serious crime will not simply go away in the wake of economic growth or greater provision of public goods, but may need to be addressed through specific interventions. This has long been recognized in violence awareness and survivor assistance programs as well as in the massive land titling endeavor across the country.

Methodology

What do we perceive as the most prominent strengths and weaknesses of this study? And how may subsequent work improve on it?

First, and most basically, it has been an advantage to obtain data from a monitoring body that covers an entire nation, revisits each collection point over several years, and also provides spatial references at a low aggregation level. Measurements were repeated not only in time, but also in substance when we consider the three kinds of conflicts that SEILA has monitored inter-related facets of human security in Cambodia.

Moreover the conflict rates are from a full census of local communities, not a sample survey. Covariates too were measured in all 1,628 communes (with the exception of poverty severity, which was estimated from population census and household sample survey data). The uncertainty of the estimates, therefore, is not the result of sample variance, but of measurement error and model misspecification.

Both are serious. The measurement of conflict, as Barron et al. (op.cit.) pointed out, should use clearer definitions. For example, the serious crime model performs less strongly than domestic violence and land conflict models because the SEILA data conflate crime against persons with property crime, two categories known to respond differently to community conditions. On the explanatory side, key informant estimates were unreliable (literacy) or absent (poverty) so that we went back to substitutes from the latest population census.

The lack of concurrent poverty estimates is particularly regrettable. Our models include poverty levels but omit poverty *change* (Do 2005). One may argue that income growth reduces the propensity for violence (though not necessarily for conflict). If so, the inclusion of poverty change variables would produce strong effects on domestic violence and serious crime, and lesser effects on land conflict. More generally, one can see the importance of omitted variables in the strong spatial effects.

Given the data situation, our analysis remains cross-sectional. This prevents controlling for endogenous effects. Such effects have to be assumed in Cambodia. Notably, areas with high levels of violent crime have deterred the work of development agencies; therefore, governance quality and conflict levels are in a two-way causal relationship. Models neglecting endogenous effects fall behind the basic tenet that poverty drives a cycle of violence, whereby poor societies fall into conflict and the conflict exacerbates the poverty (Gurr, Marshall and Khosla 2001: ; Collier, Elliot, Hegre et al. 2003).

Lines of future work

A number of extensions may be considered depending on additional annual conflict data and updated poverty estimates.

With the data at hand (up to 2004), conflict rates can be analyzed at the village and urban neighborhood level (from which we aggregated them to the commune for this study). For models limited to rural communes, a wealth index may be constructed from asset estimates. This approach would make use of the intra-commune variance. But these units are not geo-referenced, and thus spatially structured terms would remain commune level contexts.

Working at the commune level, the common spatial component present in the three conflicts can be investigated with methods that go beyond the separate estimates in this study, and which have been advanced in joint disease mapping (Held, Natario, Fenton et al. 2005). There is an empirical as well as a theoretical motive. In models without the spatial term, over half of the variance in the residual rates of the three conflicts is shared; this justifies a joint analysis.

It seems attractive also from the idea of an underlying human security map. In this view, the three conflict types are an item sample of a generalized disposition for conflict and violence. In the SEILA format, the local key informants are prompted for separate annual counts, one for each type of violence. Alternatively, the resulting incidence rates can be considered expert judgments that were elicited repeatedly regarding the local severity of the same problem. This puts a better face on the suspected underreporting, but does not change the fact that the rates are correlated in the district as well as with those of adjacent communes. The advantage is in a more continuous risk mapping that makes use of the spatial information, rather than a purely local hot-spot mapping based on some index or cluster analysis.

Assuming that SEILA will continue the conflict monitoring, new analytic opportunities will arise when commune-level poverty estimates are updated. The next population census is planned for 2008. Models of conflict rates would then be able to incorporate poverty change since 1998 as well as the conflict levels estimated for earlier years, with suitable attention to year-wise error components. This should make problems of endogeneity tractable.

With a promise of better reliability, and departing from the SEILA key informant format, conflict estimates might be produced from victimization surveys at the household level. A reliability-driven design puts the concern first that measurement error in the dependent variable, when models are non-linear (such as our count variable models), affects not only "the intercept, but the whole vector of estimated coefficients, and this even if the measurement error is uncorrelated with any of the included variables" (Christin and Hug 2005: 7). Victim data can be collected either through dedicated surveys or as part of living standards or health and demographic surveys, two long-standing international endeavors that would facilitate replication in other countries. The latest living standards survey in Cambodia took place in 2003-2004. Since these traditions have not emphasized community context and spatial modeling, quantities of interest from this SEILA data analysis could be added as commune level background variables.

Conclusion

Working with a narrow concept of human security, one that focuses, in the words of the UN Secretary-General, on "the protection of communities and individuals from internal violence" (as quoted in Mack, op.cit., ibd.), this study harnesses data on different local conflicts to a theoretical model. Bayesian methods ensure at the same time that the associations between socio-economic variables and conflict rates are explored in flexible ways. Spatial models help control for measurement error and the effects of unobserved variables. The results go beyond those of parametric models, notably in the identification of numerous non-linearities, but also in dissipating effects that had appeared significant, if implausible, under parametric assumptions.

Some covariates show distinct thresholds at which the effects on conflicts take off steeply. Most relevant for policy considerations are those that indicate quality of governance. Two findings stand out. Communities that enjoy intermediate levels of service provision tend to have higher levels of conflict than those barely provided for, suggesting interacting struggles for control of outside aid as well as of local resources, particularly land. Second, the thresholds to personal security are high. In 2002-04, four fifths of communes were below the range where our governance indicators are associated with significantly lower domestic violence and land conflict. This suggests that major threats such as homicide, rape and fights over land need to be reduced through dedicated interventions.

These findings have to be read with all the caveats due for cross-sectional analyses. The ongoing nature of the conflict monitoring and the next national population census, planned for 2008, should present opportunities for enhanced studies to address issues of mutual causation. This will help clarify the relationship between the narrowly defined personal security and wider concepts that include economic security. Flexible exploration using Bayesian models and controlling for spatial and temporal dependencies will remain useful while research from other post-war contexts will contribute new analytic ideas.

Although entities in Cambodia were not the only sources of our data (the bombing data is from the US government), their ability to observe conflicts and violence in over 13,000 local communities and to measure a spectrum of socio-economic conditions at that or the next higher administrative units should in itself have a positive impact on human security. The range and scope of these monitoring efforts may have its roots in the United Nations administration of the country in the nineties, but they are now woven into the national fabric through the government's large decentralization program.

Appendices

Estimation, inference, and model selection

We estimated the domestic violence, land conflict and serious crime models separately with BayesX (Brezger, Kneib and Lang 2005a). The final models each ran for 102,000 iterations (2,000 burn-in). Because of knot-spacing requirements, several of the covariates were track-ranked. Auto-correlations rapidly converged below the white-noise level.

Inference from the Bayesian hierarchical model uses Markov Chain Monte Carlo (MCMC) methods. A stochastic iterative process (in the limit) produces samples from the posterior distribution of the unknown parameters conditional on the observed data. From this stochastic iterative process, we can trace the various realizations of parameter θ, say, as $\theta_1, \ldots, \theta_M$. From this sequence, we can estimate the (posterior) mean and variance and produce histograms to estimate the marginal density. From the marginal density, we can estimate percentiles and produce credibility intervals.

Model selection tools were required to address the following two issues.

1. Should covariate x_j be part of the model?
2. If it is part of the model, is the effect on log η linear or semi-parametric?

The first question was answered by the advantage of having models that should remain comparable across types of violence. For this reason, we retained all of the initially included covariates. This was motivated further by workload considerations. Running one Markov chain to estimate one model took several hours. This prohibited an exhaustive search for the "best" model anyway.

To decide the second question, we made extensive use of the Deviance Information Criterion (DIC) (Spiegelhalter, Best, Carlin et al. 2002), which can be considered a Bayesian counterpart to the Akaike information criterion (AIC) and the Bayesian information criterion (BIC). It is particular useful in Bayesian model selection problems where the inference is done using MCMC-methods. The DIC is defined as follows:

$$\mathrm{DIC} = \mathrm{E}(\mathrm{deviance}(\mathrm{parameters})) + p_{\mathrm{eff}} \qquad (4)$$

where the deviance term (minus twice log-likelihood) measures the fit to the data and its expected value enters the DIC. To penalize for the number of parameters, there is a penalty of p_{eff}, which is the *effective number of parameters* in the model. Although this term is defined as

$$p_{\mathrm{eff}} = \mathrm{E}(\mathrm{deviance}(\mathrm{parameters})) - \mathrm{deviance}(\mathrm{E}(\mathrm{parameters})) \qquad (5)$$

its interpretation is not as straightforward as just "counting" the number of parameters, because of interdependencies and penalization.

Our non-linearity tests initially relied on the DIC based on the *saturated* deviance and were later repeated using the DIC based on the *unstandardized* deviance. The results, however, were inconsistent between the two DIC flavors and in either case were implausible under visual inspection of the effect curves. The reason for this can be described in two ways (Kneib 2006): If the model is considered in the negative binomial representation, the scale parameter of the negative binomial distribution differs for models with different predictors.

Therefore the standardized deviance should be evaluated using one fixed value for the scale parameter (similarly to the usual information criteria in Gaussian regression models). On the other hand, if we consider the Poisson formulation of the model including random effects, it is not quite clear whether the random effects are actually parameters of interest that should count in the effective degrees of freedom. This issue has been raised in the discussion of the paper by Spiegelhalter et al., op.cit., as one of the major concerns against the uncritical use of DIC. In general, the effective degrees of freedom rely heavily on the choice of appropriate parameters of interest, which is at least unclear in the present setting. Therefore we relied chiefly on visual inspection, focusing on the ranges with effects that have credible intervals outside zero, as detailed in Table 5 below.

Tables

Table 2: Descriptive statistics of the covariates

CAUSAL DOMAIN / Covariate	Data source	Years data available	Descriptive statistics for 2002 subsample					
			N	mean	sd	skew	min	max
WAR LEGACY								
Contaminated area (sq m, log10(x+1))	Landmine Impact Survey	2002	1,628	2.154	2.934	0.698	0	8.593
Total bombing load (pounds, log10(x+1))	US government	1965-75, aggreg.	1,628	4.061	2.989	-0.519	0	8.549
POVERTY AND RESOURCE COMPETITION								
Poverty head count (p0) [not used]	*World Food Program*	1998	*1,591*	*0.398*	*0.216*	*0.349*	*0.001*	*0.991*
Poverty gap (p1) [not used]	*World Food Program*	1998	*1,591*	*0.122*	*0.101*	*2.006*	*0*	*0.679*
Poverty severity (p2)	World Food Program	1998	1,591	0.054	0.060	3.333	0	0.488
Female headed households (fraction)	Seila	2002-04	1,619	0.161	0.071	1.934	0	0.774
Population growth (log10(pop02/pop98))	Census, Seila	1998, 2002	1,585	0.036	0.078	1.808	-0.424	0.699
URBANITY								
Population density (persons / sq km)	Seila; areas from WFP	2002-04	1,619	1,203	7,622	13.049	0.225	166,000
Male literacy (15+ w. 6+ y schooling) [not used]	*Seila*	*2002-4*	*1,619*	*0.794*	*0.175*	*-1.642*	*0.050*	*1*
Male literacy (fraction 15+ w. 6+ y schooling)	Census	1998	1,591	0.336	0.174	0.296	0.001	0.857
QUALITY OF GOVERNANCE								
Households with toilets (fraction) [infrastr. contr.]	Seila	2002-04	1,619	0.405	0.318	0.424	0	1
Households with safe water (fraction)	Seila	2002-04	1,619	0.122	0.204	2.755	0	1
Girls aged 6-14 in school (fraction)	Seila	2002-04	1,619	0.771	0.170	-1.403	0	1
EXPOSURE VARIABLE								
Households	Seila	2002-04	1,619	1,503	970	3.162	45	15,558
Valid N (listwise, variables used in final models)		2002	1,585					
Communes in Cambodia	Seila	2002	1,628					

Table 3: Descriptive statistics of conflict and violence rates

	Year			
	2002	**2003**	**2004**	**Pooled**
N	1,619	1,617	1,619	4,855
Domestic violence				
Mean [unweighted]	0.0226	0.0206	0.0195	0.0209
Mean [pop-w]	0.0200	0.0185	0.0171	0.0185
Fraction zero	0.0216	0.0167	0.0136	0.0173
Max.	0.2359	0.1967	0.2500	0.2500
Coeff.var.	1.00	0.94	0.97	0.98
Land conflicts				
Mean [unweighted]	0.0118	0.0110	0.0116	0.0115
Mean [pop-w]	0.0109	0.0103	0.0105	0.0106
Fraction zero	0.0679	0.0594	0.0482	0.0585
Max.	0.4869	0.2777	0.1925	0.4869
Coeff.var.	1.45	1.29	1.29	1.35
Serious crime				
Mean [unweighted]	0.0067	0.0059	0.0058	0.0061
Mean [pop-w]	0.0065	0.0057	0.0052	0.0058
Fraction zero	0.1316	0.1670	0.1723	0.1570
Max.	0.1939	0.4221	0.6541	0.6541
Coeff.var.	1.64	2.39	3.16	2.41

Table 4: Spatial MCMC fixed and linear effects

	Spatial MCMC					Parametric	
	Mean	Mean (X-stand'd)	STD	2.5-pc	97.5-pc	Coef.	p-value
Domestic violence							
Constant	0.168		0.102	-0.035	0.368	-5.970	<0.001
Year 2003	-0.092		0.016	-0.123	-0.061	-0.047	0.024
Year 2004	-0.142		0.017	-0.175	-0.109	-0.109	<0.001
Contaminated area	0.017	0.048	0.007	0.002	0.031	0.008	0.030
Poverty severity	0.728	0.044	0.447	-0.160	1.609	0.318	0.143
Male literacy	-0.588	-0.102	0.226	-1.025	-0.133	-0.909	<0.001
Land conflicts							
Constant	-0.841		0.117	-1.075	-0.612	-6.674	<0.001
Year 2003	-0.090		0.020	-0.130	-0.049	-0.039	0.109
Year 2004	-0.055		0.021	-0.097	-0.013	-0.037	0.134
Contaminated area	0.039	0.113	0.009	0.022	0.056	0.019	<0.001
Poverty severity	0.787	0.047	0.501	-0.189	1.763	0.580	0.021
Male literacy	0.132	0.023	0.257	-0.371	0.631	-0.167	0.167
Serious crime							
Constant	-1.381		0.160	-1.684	-1.051	-6.988	<0.001
Year 2003	-0.235		0.031	-0.295	-0.175	-0.180	<0.001
Year 2004	-0.259		0.032	-0.323	-0.196	-0.231	<0.001
Contaminated area	0.036	0.103	0.011	0.015	0.057	0.004	0.486
Poverty severity	0.625	0.037	0.601	-0.533	1.823	0.192	0.556
Male literacy	0.220	0.038	0.321	-0.414	0.841	-0.070	0.638

Note: Spatial MCMC coefficients with a 95 percent credible interval outside zero are highlighted gray. Similarly, coefficients returned by parametric Poisson regression models data are grayed when their associate p-values are smaller than 0.05.

In this and the following tables, the number of observations for all MCMC models is 4,753.

Table 5: Spatial MCMC non-linear effects compared to parametric model effects

	Spatial MCMC –– Effect Diff (max-min)	Covariate ranges with CI outside zero: Negative effects %obs	Range	Positive effects %obs	Range	Parametric Coef.	p-value
Bombing intensity							
Domestic violence	0.39	34	[0]	0		8.5E-4	0.816
Land conflicts	0.45	0		2	[7.45; 8.07]	0.008	0.054
Serious crime	0.33	34	[0]	0		0.005	0.486
Female-headed HH							
Domestic violence	0.12	3	[0.01; 0.07]	0		0.492	<0.001
Land conflicts	0.09	0		0		0.532	0.001
Serious crime	0.14	0		0		0.499	0.014
Population change							
Domestic violence	0.26	0		8	[0.10; 0.70]	0.459	<0.001
Land conflicts	0.54	0		10	[0.09; 0.70]	0.665	<0.001
Serious crime	0.28	0		0		0.345	0.540
Population density							
Domestic violence	0.52	13	[0.21; 21.1]	0		-1.28E-5	<0.001
Land conflicts	1.40	14	[0.21; 22.9]	25	[46.9; 159]	-7.89E-6	0.003
Serious crime	1.39	23	[0.21; 51.6]	22	[335; 1.67E5]	-3.14E-6	0.152
HH with toilets							
Domestic violence	0.24	5	[0.02; 0.03]	0		-0.510	<0.001
Land conflicts	0.23	0		0		-.0.547	<0.001
Serious crime	0.10	0		0		0.114	0.316
HH with safe water							
Domestic violence	0.24	16	[0.85; 1]	19	[0.22; 0.39]	-0.096	0.008
Land conflicts	0.23	10	[0; 0.03] and	38	[0.14; 0.50]	-0.116	<0.001
		19	[0.80; 1]				
Serious crime	0.10	0		0		0.114	0.028
Girls attending school							
Domestic violence	0.15	9	[0.97; 1]	0		0.027	0.731
Land conflicts	0.25	5	[0; 0.50] and	7	[0.89; 0.91]	0.116	0.203
		11	[0.96; 1]				
Serious crime	0.20	14	[0.95; 1]	2	[0.69; 0.70]	-0.283	0.013

Note: The cells giving the effect ranges are grayed if the covariates have any ranges with credible effects. This range may consist of one value only. This is the case of the bombing intensity. One third of the communes were not bombarded; their predicted domestic and serious crime levels are significantly lower.

The fraction of households with access to safe water and the fraction of girls aged 6 – 14 years attending school each have two ranges in which their effects on land conflicts are credibly negative.

Coefficients returned by parametric regression models are grayed when their associated p-values are smaller than 0.05.

The percentage of households with sanitary toilets, although used as a control for infrastructure effects only, appears in this table because it was included in the semi-parametric terms.

Table 6: Synopsis, from visual inspection, of governance covariate graphs

		Domestic violence	Land conflicts	Serious crime
Safe water	"Kink" - nearest decile bound	80	80	70
	Corresp. pc HH with safe water	79%	79%	63%
	Credible interval outside zero?	**Yes**	**Yes**	No
	Effect difference "kink" to max	-0.2	-0.15	-0.1
	Hypoth. drop in conflicts	**22%**	**16%**	11%
Girls in school	"Kink" - nearest decile bound	80	80	20
	Corresp. pc girls in school	92%	92%	69%
	Credible interval outside zero?	**Yes**	**Yes**	**Yes**
	Effect difference "kink" to max	-0.15	-0.15	-0.2
	Hypoth. drop in conflicts	**16%**	**16%**	**22%**

Table 7: Standard deviations of the estimated effects on conflict rates

	Domestic violence	Land conflicts	Serious crime
WAR LEGACY			
Contaminated area	0.048	0.113	0.103
Bombing intensity	0.058	0.077	0.064
POVERTY AND RESOURCE COMPETITION			
Poverty severity	0.044	0.047	0.037
Female headed households	0.023	0.023	0.033
Population growth 1998-2002	0.051	0.093	0.050
URBANITY			
Population density	0.111	0.255	0.285
Male literacy	0.102	0.023	0.038
QUALITY OF GOVERNANCE			
Households with safe water	0.066	0.080	0.031
Girls aged 6-14 in school	0.033	0.058	0.053
CONTROLS			
Year effects			
Year 2003	0.043	0.042	0.111
Year 2004	0.067	0.026	0.122
Spatial effects			
Random (district level)	0.112	0.099	0.127
Spatially structured (commune level)	0.612	0.648	0.783
Infrastructure effect			
Households with toilets	0.050	0.049	0.029

Note: For the semi-parametrically modeled terms, the effects are values of functions; in other words the standard deviations do not depend on the values of the covariates. For the linear terms, the effects are the covariate values multiplied with the coefficients.

Table 8: Comparison of models with and without commune-level spatial term

	Domestic violence	Land conflicts	Serious crime
With commune-level spatial term			
DIC based on the saturated deviance	6466	6535	6430
Rank corr. predicted / observed rates	0.88	0.84	0.75
Without commune-level spatial term			
DIC based on the saturated deviance	5418	5574	5560
Rank corr. predicted / observed rates	0.55	0.48	0.39

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