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A.1 Survey

A.1.1 Selection of counties

My survey covers 242 rural towns and villages in three counties in central and northern Liberia: Lofa, Nimba and Grand Gedeh. Lofa lies in the northwest of the country, and shares a border with both Guinea and Sierra Leone. Nimba is located to the south and east of Lofa, and borders Guinea and Cote d'Ivoire. Grand Gedeh abuts Nimba to the southeast, and borders the southwestern-most districts of Cote d'Ivoire—a fact that plays an important part in my analysis. Nimba is the largest and most densely populated of the three counties with a population of about 462,000 (according to the 2008 census), while Grand Gedeh is the smallest and most sparsely populated, with just 125,000 inhabitants. Lofa, with a population of 276,000, falls in between.

These counties were selected for the survey because they were among the most severely affected by the Liberian civil war, and because they have become priorities for both peacekeeping and state consolidation in the post-conflict years. These three counties also occupy a sort of middle ground between the domains of statutory and customary law in Liberia. In Monrovia, formal institutions tend to dominate informal ones; in the hinterlands—far-flung and sparsely-populated—the opposite is usually true. In Lofa, Nimba and Grand Gedeh, in contrast, formal and informal institutions coexist and compete with one another. These counties thus constitute an important test case for the rule of law in Liberia.

Nimba was the epicenter of opposition to the dictatorship of Samuel K. Doe, which preceded and precipitated the Liberian civil war. After their involvement in a failed coup d'etat in 1985, Nimba's Gio and Mano populations were systematically excluded from the country's political life, and became targets of Doe's "purges" in which hundreds of young Mano and (especially) Gio men were detained, interrogated and executed. The civil war began in Nimba in 1989 when a group of 100 National Patriotic Front of Liberia (NPFL) rebels under Charles Taylor invaded through the border town of Butuo. Taylor's decision to enter through Nimba was highly strategic, as the county's Gio and Mano residents subsequently became some of his most loyal recruits, joining en

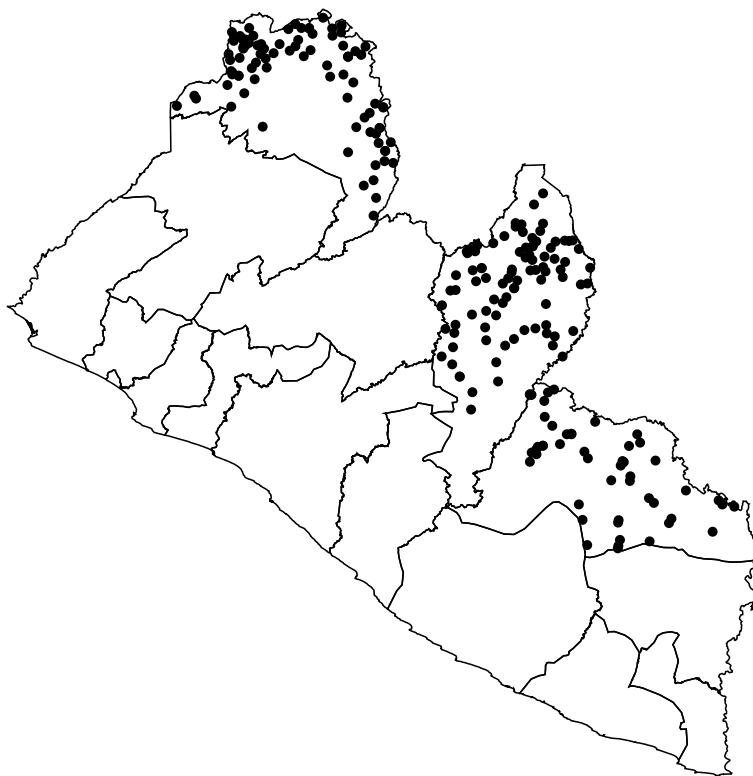
masse after the 1989 raid. After some initial paroxysms of violence, Nimba actually witnessed “very little combat” and was “relatively stable,” remaining under uncontested rebel control for the remainder of the conflict (? , 143, 312).

Lofa, in contrast, was subject to years of virtually uninterrupted violence as rival factions attacked and counter-attacked one another. Lofa has one of the country’s largest Mandingo populations, but is also home to many members of the Lorma and Kpelle tribes. When the NPFL first entered the county, one of its first acts was to slaughter over 500 residents of the predominately Mandingo town of Bakedu (one of the communities in my sample)—an atrocity whose consequences would continue to reverberate throughout the conflict and post-conflict periods. As Ellis (? , 128) explains, “the memory of the Bakedu massacre was one of the chief motives for Mandingo exiles to join Ulimo-K in Sierra Leone and Guinea in 1991.” And indeed, when Ulimo-K invaded Lofa from Sierra Leone in 1992, it brutalized Lofa’s Lorma and Kpelle inhabitants, indiscriminately killing civilians and desecrating traditional religious sites before establishing its de facto capital in the city of Voinjama (another community in my sample).

Finally, in the late 1990s, as rebels began threatening Taylor’s control over the northern counties, his regime began providing material and logistical support to the Lofa Defense Force, a faction that drew primarily from Lorma and Kpelle refugees who had fled to Guinea, and that had emerged in Lofa in 1993 to fight a local war against the predominately Mandingo Ulimo-K. Tensions between these wartime ethnic rivals persisted long after the conflict had officially ended; indeed, Lofa was the site of what was arguably the single most destabilizing incident of collective violence in the post-war period, when accusations of abductions and ritual killings provoked multiple days of rioting between Voinjama’s Lorma and Mandingo inhabitants, resulting in four deaths, dozens of injuries and the destruction of much of the city’s infrastructure.

If Nimba was the epicenter of opposition to the Doe regime, **Grand Gedeh** was the epicenter of loyalty. The county is the ancestral homeland of the Krahn tribe, of which Doe was a member and from which he drew much of his support. While Taylor managed to establish firm territorial control over most of the country, Grand Gedeh remained an exception, witnessing persistent fighting

Figure A.1: Map of communities in sample



throughout the conflict as the NPFL attempted to quell a dogged local resistance. Beyond the Gios and Manos that constituted the bulk of Taylor's forces, the NPFL attracted some additional local support from Grand Gedeh's minority Grebo population, who resented having been long excluded from political power by the county's Krahn majority.

A.1.2 Selection of communities

The 242 communities in my sample constitute the smallest unit of administration in Liberia. Most are villages or small towns. 50 are neighborhoods, called "quarters," within larger towns, with their own quarter chiefs.¹ Figure A.1 shows the geographical distribution of these communities across

¹The sample includes 50 quarters distributed across 36 larger towns. The largest town has 16 quarters, but most have far fewer, with a median of 5. In general we only sampled one or two quarters per town, selected at random.

the three counties in the sample. Importantly, these communities are not representative of Liberia, nor of the counties from which they were sampled. The data were originally collected for purposes of a randomized controlled trial evaluating an alternative dispute resolution (ADR) program. The communities in the sample were selected by government officials and other stakeholders because they were believed to be at disproportionately high risk of violence, and thus as especially likely to benefit from ADR (?).

While this is an important scope condition for my analysis, it is not as restrictive as it may seem. Comparison to a nationally representative survey conducted at the same time by researchers at UC Berkeley's Human Rights Center suggests the 242 communities in my sample were not much more conflicted than the average Liberian town or village, either in these three counties or nationwide (?). For example, 4% of respondents in Vinck et al.'s (2011) survey reported being victims of armed violence in Lofa, 5% in Nimba and 4% in Grand Gedeh, compared to 7% nationwide. In my sample, 1% of respondents reported being victims of armed violence in Lofa, 3% in Nimba and 3% in Grand Gedeh. These rates are comparable across surveys in all counties.

Rates of robbery and burglary are similar as well: in Vinck et al., 12% of respondents reported a robbery or burglary in Lofa, 11% in Nimba and 18% in Grand Gedeh, compared to 15% nationwide. In my sample, 14% of respondents reported a robbery or burglary in Lofa, 25% in Nimba and 13% in Grand Gedeh. Except for Nimba, these rates are comparable across surveys as well. Complaints of witchcraft are similar across surveys as well—indeed, if anything the latter are less common in my sample than in Vinck et al.'s. 19% of Vinck et al.'s respondents reported being victims of witchcraft in Lofa, 21% in Nimba and 14% in Grand Gedeh, compared to 17% nationwide. In my sample, 6% of respondents reported being victims of witchcraft in Lofa, 10% in Nimba and 11% in Grand Gedeh. Of course, it is possible that the communities in my sample are more conflicted along other dimensions. But it is not obvious why this would be the case.

The communities in my sample are similar to those in Vinck et al. in their demographic characteristics as well. 48% of respondents in my sample are men, compared to 50% in Vinck et al. The average respondent in my sample is 41 years old, compared to 37 years old in Vinck et al.

42% of my sample has no education, compared to 35% in Vinck et al.—a difference attributable to the much higher rate of educational attainment among Monrovia residents in Vinck et al.’s sample. (83% of Monrovia residents have at least some primary education.) 13% of my sample is Muslim, compared to 10% in Vinck et al. I interpret the parallels between my sample and Vinck et al.’s as evidence in favor of generalizability, at least within Liberia. (I discuss the issue of generalizability to other countries in the conclusion of the manuscript.)

A.1.3 Sampling frame

My sample consists of a representative sample of 20 randomly-selected residents and four purposively-selected local leaders per community—typically a town chief, women’s group leader, youth group leader and minority ethnic group leader. These positions exist in almost all Liberian communities, and they represent important and distinct cross sections of the Liberian populace. Whenever possible, the same four local leaders were surveyed in each wave of data collection. For communities in which one or more of the four local leaders had died, moved or left office between waves of data collection, the survey tracked the position rather than the individual.

Sampling of residents followed the random walk method. A team of enumerators walked the length of each community and divided it into roughly equal blocks, counted all houses along the “major pathways” separating those blocks, and selected households at even intervals along the major pathways. Respondents were selected at random from among the consenting adult inhabitants of each household. Due to budget and time constraints, in 40 of the 242 communities, only local leaders were surveyed in the third wave of data collection. The 40 excluded communities are very similar to the included ones, and my results are substantively unchanged if I rerun my analysis on the 202 communities surveyed in all three waves of data collection. (In any event, this restriction is only relevant to my analysis of the persistence of UNMIL’s effects on the rule of law.)

A.1.4 Selection of control variables

Prior to the second wave of data collection, I used my qualitative interviews with UNMIL personnel to create a principled, contextually-grounded set of control variables that would allow me to model the selection processes underlying UNMIL patrols, interventions and public works. To further understand these processes, I combined results from my qualitative interviews with a review of the secondary sub-national literature on peacekeeping in Liberia and elsewhere (especially ? and ?), and of the UN Secretary General's periodic progress reports on UNMIL's activities. While there is obviously some slippage between the selection processes described by UNMIL staff and the one implemented on the ground, the mission's selection criteria are, for the most part, knowable and quantifiable.

UNMIL typically does not decide where to patrol, intervene or implement public works projects based on the characteristics of particular individuals. I therefore focus first and foremost on controlling for potential community-level confounders, using my qualitative interviews and a review of the secondary literature to better understand the selection process underlying UNMIL's activities, and therefore to identify the confounders that are likely to be most problematic.

First and most obviously, UNMIL patrols are more likely where the incidence of **crime and violence** is high (MS, Gbarnga, 10/11/2010). The same is true, axiomatically, of interventions to resolve disputes. As Fortna (?, 96-7) explains, across countries, much of peacekeepers' "day-to-day work" involves preventing the escalation of small-scale conflicts into large-scale crises, and UNMIL is no exception. To model this aspect of the selection process, I include dummies for whether there were any major incidents of crime or violence (e.g. murders, riots, incidents of mob justice) in each community in the past year. I also include a continuous variable capturing the proportion of residents who were themselves victims of a crime in the past year.

Second, while in theory UNMIL patrols, interventions and public works can target even "the most remote areas" (KY, Gbarnga, 10/11/2010), in practice they are constrained by **accessibility**. Roads are an especially important constraint in rural Liberia. In an interview with Higate and Henry (?, 77), one UNMIL volunteer explains that "if your area can't be reached by car, you won't

be visited.” Another concurs: “if people come from areas where there are no car-accessible roads, they are unlikely to be visited by UNMIL peacekeepers.” As a proxy for accessibility, I include a measure of the distance (in hours) from each community to the nearest usable road in Liberia’s rainy season, based on estimates gleaned from the local leaders survey. I also include a dummy for whether there is any cell phone network coverage in the community. And because the most accessible communities also tend to be the largest, most urbanized and most developed, I include (1) an estimate for town or village population; (2) a principal components index of 12 household assets (including cell phones, radios, generators and livestock, among others), aggregated from the household to the community level; and (3) an additive index of 14 different social services potentially available in each community (including clinics, schools, wells and latrines).

Third, since the mission’s inception, UNMIL has prioritized restoring the Liberian government’s access to, and control over, areas rich in **natural resources**—especially rubber, but also gold, iron, diamonds, and timber (CC, Monrovia, 10/27/2011). UNMIL was instrumental in reestablishing state authority over the country’s rubber plantations, which, in the aftermath of the civil war, had been overrun by partially re-mobilized remnants of various illegal armed groups (?). To operationalize natural resources, I include a dummy variable indicating the presence of any rubber or timber plantation, or any diamond, gold or iron mine, within an hour’s walk of each community.

Fourth, while UNMIL personnel often conduct patrols on their own, they also implement routine joint patrols with the LNP and, to a lesser extent and in more exceptional circumstances, the AFL and the Bureau of Immigration and Naturalization. The increasing presence of state security forces likely has an independent effect on civilians’ perceptions of those institutions---for better or worse (?). UNMIL also frequently hosts workshops and other outreach and “sensitization” activities, some of which include staff from various local and international NGOs. To disentangle the impact of UNMIL’s activities from that of **police and NGO presence**, I include dummies for the frequency of LNP patrols and NGO visits to each community in the past year.

Finally, while UNMIL has undoubtedly adjusted to the changing landscape of crime, violence

and (in)security in Liberia over the years of its mandate, its initial deployment decisions in the months after the signing of the 2003 CPA continue to have important implications for its operations today—a fact that I exploit in one of my instrumental variables strategies below. To the extent that UNMIL deployments are path dependent, the areas with the most intense **exposure to wartime violence** should also have the most intense exposure to UNMIL today. To account for this possibility, I construct an additive index of 12 different forms of wartime victimization and participation, aggregated from the individual to the community level. I also include estimates for the proportion of returned refugees or internally displaced persons in each community, and the proportion of ex-combatants.

A.1.5 Descriptive statistics

Citizen characteristics

Table A.1 provides descriptive statistics for the residents survey. The average respondent was a 41-year-old farmer with fewer than six years of primary school education. Over 80% of respondents reported being refugees or internally displaced at some point during the conflict; given that 14 years of civil war displaced nearly the entire population, this is unsurprising. Over 60% reported having interacted (or “spent time”) with UNMIL personnel.

Most respondents preferred relying on the state to adjudicate the most serious incidents of crime and violence. This proportion is highest for the two vignettes that include an explicit reference to fatalities (murder and ethnic riots). For mob violence, 59% of respondents preferred relying on informal authorities, compared to just 34% that preferred relying on formal ones. This may reflect the fact that mob violence is itself an informal mechanism for dispute resolution, and thus falls naturally (if still illegally) under the purview of informal authorities. Alternatively, it may suggest that respondents perceived this scenario as less severe than the other two. Strikingly, the proportion of respondents preferring UNMIL never exceeded 15% in any scenario.

Interestingly, while most respondents preferred relying on the state, most also perceived the state as more corrupt and less impartial than its informal and international counterparts. A majority

Table A.1: Citizen characteristics

	Mean	N
Spent time with UNMIL	0.61	4,687
Rely on formal overall	0.56	4,799
Rely on formal for murder	0.64	4,799
Rely on formal for mob violence	0.35	4,799
Rely on formal for ethnic riots	0.58	4,799
Rely on informal overall	0.32	4,799
Rely on informal for murder	0.25	4,799
Rely on informal for mob violence	0.59	4,799
Rely on informal for ethnic riots	0.30	4,799
Rely on UNMIL overall	0.12	4,799
Rely on UNMIL for murder	0.11	4,799
Rely on UNMIL for mob violence	0.06	4,799
Rely on UNMIL for ethnic riots	0.11	4,799
Believes formal is corrupt	0.61	4,368
Believes formal is biased	0.33	4,615
Believes informal is corrupt	0.30	4,419
Believes informal is biased	0.11	4,721
Believes UNMIL is corrupt	0.15	3,642
Believes UNMIL is biased	0.09	4,343
Age	41.39	4,801
Male	0.48	4,801
Years of education	5.55	4,801
Ethnic or religious minority	0.13	4,798
Farmer	0.55	4,801
Returned refugee or IDP	0.84	4,801

of respondents perceived the state as corrupt (61%), compared to 30% for informal authorities and just 15% for UNMIL. Perceptions of bias were less pervasive but no more charitable to state institutions. One third of respondents believed the state discriminates against certain ethnic or religious groups; far fewer perceived bias among informal authorities (11%) or UNMIL (9%). The correlation between preferences and perceptions is near zero,² suggesting that citizens were, on average, equally likely to prefer formal over informal or international security providers, regardless of their perceived fairness or impartiality.

Community characteristics

Table A.2 reports summary statistics for the communities in my sample. Liberia experienced rapid demographic, economic and infrastructural growth in the years after the civil war, and that growth is apparent even in the two-year window captured in the table. The average community had a population of approximately 2,000 residents in 2008 (though this number varies dramatically, with a maximum of 5,000 inhabitants and a minimum of just 20); by 2010, the average population had grown to over 3,000 (with a maximum of 7,250 and a minimum of 30). Between 2008 and 2010, the proportion of communities with cell phone coverage increased from just over half to nearly three-quarters; the distance to the nearest usable road decreased by around eight minutes, or approximately 13% (from just over an hour to just under); and the average number of social services available increased from around six to around seven. Police patrols were common in both periods—around half of communities reported weekly or monthly patrols in both 2008 and 2010—and NGO visits were even more so, reaching about three-quarters of communities.

Liberia became more secure over time as well. The fraction of communities reporting a major incident of interpersonal or collective violence declined from 29% in 2008 to 16% in 2010. Despite this decline, legacies of the conflict still loomed large. On an additive index of 13 different forms of exposure to wartime violence (including beatings, shootings, and forced labor), the average score

²For corruption, the correlation is .07 for the state, -.04 for informal authorities, .01 for UNMIL. The correlations are similar or smaller for impartiality.

Table A.2: Community characteristics

	2008		2010	
	Mean	N	Mean	N
UNMIL patrolled weekly	0.14	229	0.14	229
UNMIL patrolled monthly	0.27	229	0.27	229
UNMIL patrolled occasionally	0.38	229	0.38	229
UNMIL intervened to resolve disputes	0.28	243	0.28	243
UNMIL built or repaired public works	0.19	243	0.19	243
Population	2,033	242	3,115	243
Distance to nearest road (hrs.)	1.05	242	0.92	243
Cell coverage	0.58	242	0.74	243
# of services available in town (0-14)	5.76	242	6.81	243
Any natural resources w/in 2 hr. walk	0.64	242	0.71	243
Weekly or monthly visits by police	0.55	242	0.45	243
Weekly or monthly visits by NGOs	0.71	242	0.73	243
Town wealth index (PCF)	-0.02	243	-0.03	243
Town index of wartime violence (0-13)	4.29	243	4.76	243
% displaced or refugee during war	0.80	243	0.84	243
# of ex-coms in town	25.67	242	51.48	243
Any major incident of violence	0.29	242	0.16	243

was over four in both 2008 and 2010, with minimum scores of one and two, respectively. In other words, not a single community in the sample reported being entirely unaffected by the conflict.

Even among these rural communities, UNMIL managed to establish a remarkably pervasive presence. In 2010, local leaders reported weekly UNMIL patrols in about 14% of communities. (Unfortunately I did not measure exposure to UNMIL in 2008.) Monthly patrols were more common (27% of communities), and occasional patrols even more so (37% of communities). Less than one-quarter of communities reported no patrols at all—a figure that is all the more remarkable given that the average community in the sample was located nearly an hour from the nearest usable road. UNMIL intervened to resolve disputes in slightly more than one-quarter (28%) of communities, and constructed public works projects in slightly less than one-fifth (19%).

A.2 List experiment

A.2.1 Item counts in treatment and control groups

Table A.3 reports the distribution of list experiment item counts in the treatment and control groups. A “semi-sensitive” item was included among the control items in order to minimize the risk of floor and ceiling effects. Ceiling effects occur when respondents’ true answers are affirmative for all items (including the sensitive one); floor effects occur when respondents’ true answers are negative for all items. In either case, treatment group respondents may fear that answering truthfully will reveal their response to the sensitive item (?). The distribution of item counts suggests that my inclusion of a semi-sensitive item successfully minimized this risk.

A.2.2 Design effects

Table A.4 reports results from Blair and Imai’s (?) proposed test for detecting list experiment design effects. Each row reports the estimated proportion of respondents that answered n items affirmatively in the treatment and control groups. The test estimates the probability that inclusion

Table A.3: Local leaders' responses to list experiment

	Control	Treatment
Item count		
0	36%	12%
1	47%	37%
2	15%	36%
3	1%	13%
4		2%
Direct questions (control group only)		
Call the police	41%	
Do nothing	26%	
Call UNMIL	14%	

Notes: Responses to a list experiment in which local leaders were read a vignette describing a hypothetical burglary and asked to count the number of potential responses they believed were likely to occur in their community. The list experiment uses Corstange's (?) modified design, in which the control group is asked direct questions for each control item on the list. Control items include "call the police," "just leave it alone and do nothing about it" and "take it to the UNMIL people." The sensitive item is "call for sassywood or sand cutter," a Liberian colloquialism for trial by ordeal.

Table A.4: Test for list experiment design effects

	Control	Treatment
$\Pr(y_i = 0)$	0.12 [0.02]	0.24 [0.03]
$\Pr(y_i = 1)$	0.13 [0.13]	0.34 [0.03]
$\Pr(y_i = 2)$	0.02 [0.02]	0.14 [0.02]
$\Pr(y_i = 3)$	-0.01 [0.01]	0.02 [0.01]
Bonferroni-corrected p -value	0.27	

Notes: Results from Blair and Imai’s (?) test for list experiment design effects. Each row reports the estimated proportion of respondents that answered n items affirmatively in the treatment and control groups. Standard errors are in brackets. The null hypothesis is no design effect.

of the sensitive item affected respondents’ answers to the control items. The null hypothesis is no design effect. I fail to reject the null, meaning that the assumption of no design effects is likely satisfied.

Originally, my list experiment included two additional hypothetical scenarios as well: a missing person and a mysterious death. Unfortunately, these incidents proved sufficiently severe that almost no control respondents admitted to “doing nothing” when asked directly, and Blair and Imai’s (?) test rejects the null of no design effect. For this reason, and because missing persons and mysterious deaths are not crimes (and therefore do not technically require reporting to the police), I exclude them from my analysis.

A.3 Interviews

I complement my survey data with 42 open-ended qualitative interviews conducted over the course of 15 months of fieldwork in Liberia. 19 of these interviews were conducted with UNMIL per-

sonnel; six with representatives of local and international NGOs; six with government officials, including police officers, magistrates, and court clerks; and 11 with civilians in and around Gbarnga, Bong County. All but two of these interviews were conducted in person between June 2010 and November 2012; the remaining two were conducted via phone and email in October 2013. In-person interviews were conducted in Bong, Lofa, Nimba and Grand Gedeh counties, as well as in Monrovia, the capital city. Civilian respondents were randomly sampled; all others were selected through referral (i.e. snowball sampling). The goal throughout was to interview respondents who could speak knowledgeably about UNMIL's activities in Liberia.

A.4 Instrumental variables

A.4.1 Effects on citizens' preferences using proximity to UNMIL bases as instrument

Drawing on ?, I use the locations of UNMIL bases as a source of as-if random variation in the intensity of exposure to UNMIL at the individual level.³ As Mvukiyehe and Samii note, and as I discuss in the manuscript, before peacekeeping operations deploy, costly siting decisions must be made on the basis on highly incomplete information about potential host communities. The quality of this information is likely to be especially low in rural areas. Initial siting decisions also depend on the idiosyncratic preferences of troop-contributing countries, as expressed at UN headquarters in New York (?). This may explain why peacekeepers tend to deploy to the most conflicted areas sub-nationally, but only after a considerable delay (?).

For this identification strategy, I calculate the proximity of each community in my sample to the nearest UNMIL base established by the end of the mission's first full year of deployment (2004), after which new and better information should have become available. I then use that calculation as

³My approach draws on but departs from Mvukiyehe and Samii, who compare Liberian clans (one administrative unit above the community level) that did and did not host bases, using matching to create samples of comparable "treatment" and "control" units.

an instrument for individual-level exposure to UNMIL in 2010.⁴ Following Mvukiyehe and Samii, I minimize any residual selection effects by including control variables drawn from the same data sources that UNMIL consulted at the time of deployment, including a rapid needs assessment conducted by the UN Office for the Coordination of Humanitarian Affairs (OCHA) in early 2004.⁵

This identification strategy hinges on two assumptions. First, conditional on covariates, I assume that UNMIL's initial siting decisions were as-if random, such that any other community could have just as easily been chosen to host a base. Second, I assume that conditional on those same covariates, the only mechanism through which proximity to an UNMIL base might have affected Liberians' attitudes was the higher "dose" of peacekeeping to which they were subsequently exposed. Based on UNMIL's deployment maps,⁶ the most obvious threat to both of these assumptions is the mission's preference for establishing bases in county capitals and other large cities and towns. In addition to controlling for population and distance to the nearest usable road, I mitigate this threat by restricting my analysis to communities located within 5km of *either* a base *or* a city that did *not* host a base in 2004, using UNMIL's own deployment maps to identify locations that might have been considered as viable alternatives.⁷ My results are robust to wider and narrower bandwidths as well.

⁴Because my analysis is just-identified for one endogenous regressor, I can only instrument for individual-level exposure. In principle I could use proximity to the nearest base as an instrument in 2012 as well, but the first-stage relationship is weak. This may be because UNMIL began dismantling some of its bases between 2010 and 2012, limiting opportunities for engagement with civilians living in the vicinity.

⁵Specifically, I control for the number of households in each community; dummies indicating the presence of schools or health clinics; a dummy indicating whether the community is accessible in the rainy season; and an estimate for the number of incidents of wartime violence in the community between 1997 (the first year data is available) and 2003 (the end of the civil war). All but the last of these controls are gleaned from the OCHA rapid needs assessment; the last is gleaned from the Armed Conflict Location and Event Data Project (ACLED).

⁶Available as annexes to the UN Secretary-General's progress reports, downloadable at <http://www.un.org/en/peacekeeping/missions/unmil/reports.shtml>.

⁷UNMIL's deployment maps are very sparse. I assume that if a city is listed on the map, then the UN must have considered it sufficiently large and strategically important to warrant the mission's attention, and potentially to consider it during initial siting decisions.

Table A.5: First-stage relationship between proximity to the nearest UNMIL base and citizens' reports of exposure to UNMIL

Proximity to nearest UNMIL base	Spent time with UNMIL		
	0.01	0.01	0.01
	[0.00]***	[0.00]***	[0.00]***
Observations	948	1,250	1,699
<i>F</i>	13.40	12.20	19.77
District FE	N	N	N
Community controls	Y	Y	Y
Individual controls	Y	Y	Y
Proximity to nearest city	4km	5km	6km

Notes: Coefficients from OLS regressions. Estimates are weighted by the inverse probability of sampling. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.5 reports the first-stage relationship between citizens' reports of "spending time" with UNMIL personnel in 2010 and proximity to the nearest UNMIL base that was established by the end of 2004. In column 1 I restrict my sample to communities located within 4km of *either* an UNMIL base *or* a city that did *not* host an UNMIL base in 2004, using UNMIL's own deployment maps to identify locations that might have been considered viable alternatives. In columns 2 and 3 I restrict my sample using 5km and 6km bandwidths, respectively. Proximity is measured in km, and is operationalized as the negative of the distance to the nearest base, such that higher values indicate closer proximity. The table reports coefficients from linear probability models with individual- and community-level controls, inverse probability weights, and standard errors clustered at the community level. (My results are substantively unchanged if I use a logit or probit estimator instead.)

Because establishing a base requires substantial fixed costs, these decisions were very sticky, resulting in a strong first-stage relationship even six years after initial siting decisions were made. Regardless of the bandwidth I use, my first-stage results suggest that proximity to the nearest

Table A.6: Effects on citizens' preferences using proximity to nearest UNMIL base as instrument

	Rely on informal	Rely on formal	Rely on UNMIL
Spent time with UNMIL	-0.34 [0.16]**	0.33 [0.18]*	0.01 [0.10]
Observations	1,249	1,249	1,249
District FE	N	N	N
Individual-level controls	Y	Y	Y
Community-level controls	Y	Y	Y
Proximity to nearest city	5km	5km	5km

Notes: Coefficients from two-stage least squares regressions using proximity to the nearest UNMIL base as an instrument for exposure to UNMIL in 2010. Estimates are weighted by the inverse probability of sampling. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

UNMIL base is a sufficiently strong instrument for citizens' interactions with UNMIL personnel. Survey respondents were, on average, 1 percentage point more likely to report spending time with UNMIL for every 1 km closer they lived to the nearest UNMIL base. A standard "rule of thumb" is that strong instruments must have first-stage F -statistics of at least 10 (?); my instrument exceeds this threshold at all three bandwidths.

Table A.6 reports my second-stage results. Each column reports estimates from a separate two-stage least squares regression, with the same controls and inverse probability weights as in my other analyses.⁸ Standard errors are again clustered by community. Consistent with the results in the manuscript, I find that exposure to UNMIL increased reliance on formal authorities and decreased reliance on informal ones. Within this sub-sample of urban communities, residents who reported interacting with UNMIL as a function of distance to the nearest base were 33 percentage points more likely to prefer relying on formal institutions, and 34 percentage points less likely to

⁸To avoid collinearity with distance to UNMIL bases, I omit district fixed effects from these analyses.

prefer relying on informal ones. The former effect is only weakly statistically significant, but is substantively large and statistically different from the latter. Residents who reported interacting with UNMIL were no more or less likely to prefer relying on UNMIL itself.

Of course, this 5km bandwidth is somewhat arbitrary—wide enough to ensure statistical power and a strong first-stage relationship, but narrow enough to exclude communities that are obviously outside the range of UNMIL’s initial siting decisions. Table A.7 replicates my analysis, restricting my sample to communities located within 4km (columns 1-3) or 6km (columns 4-6) of the nearest city instead. My point estimates are nearly identical at these wider and narrower bandwidths, suggesting that my results are not an artifact of the particular bandwidth I use.

A.4.2 First-stage relationship between proximity to Para and citizens’ reports of exposure to UNMIL

Table A.8 reports the first-stage relationship between citizens’ reports of “spending time” with UNMIL personnel in 2012 and proximity to the Ivorian border town of Para. I restrict my sample to communities located within 15km, 20km and 25km of the Ivorian border in columns 1, 2 and 3, respectively. Proximity is again measured in km, and the table again reports coefficients from linear probability models with individual- and community-level controls, inverse probability weights, and standard errors clustered at the community level. Regardless of bandwidth, proximity to Para is a sufficiently strong instrument for interactions with UNMIL personnel. The average 2012 survey respondent was 3 percentage points more likely to interact with UNMIL personnel for every 1 km closer they lived to Para. My first-stage F -statistics exceed the “rule of thumb” threshold of 10 at all three bandwidths.

Table A.7: Effects on citizens' preferences using proximity to nearest UNMIL base as instrument, robustness to alternate buffers

	Rely on informal	Rely on formal	Rely on UNMIL	Rely on informal	Rely on formal	Rely on UNMIL
Spent time with UNMIL	-0.35 [0.14]**	0.27 [0.16]*	0.08 [0.10]	-0.38 [0.14]***	0.26 [0.14]*	0.13 [0.10]
Observations	947	947	947	1,698	1,698	1,698
District FE	N	N	N	N	N	N
Community-level controls	Y	Y	Y	Y	Y	Y
Individual-level controls	Y	Y	Y	Y	Y	Y
Proximity to nearest city	4km	4km	4km	6km	6km	6km

Notes: Coefficients from two-stage least squares regressions using proximity to the nearest UNMIL base as an instrument for exposure to UNMIL in 2010. Columns 1-3 and 4-6 report results using 4km and 6km buffers, respectively. Estimates are weighted by the inverse probability of sampling. Standard errors, clustered by community, are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.8: First-stage relationship between proximity to Para and citizens' reports of exposure to UNMIL

Proximity to Para	Spent time with UNMIL		
	0.03	0.03	0.03
	[0.01]***	[0.01]***	[0.01]***
Observations	943	1,384	1,556
<i>F</i>	28.30	34.79	31.90
District FE	N	N	N
Community controls	Y	Y	Y
Individual controls	Y	Y	Y
Proximity to Ivorian border	15km	20km	25km

Notes: Coefficients from OLS regressions. Estimates are weighted by the inverse probability of sampling. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.4.3 Effects on citizens' preferences using proximity to Para as instrument, using alternate bandwidths

In the manuscript, when using proximity to Para as an instrument, I restrict my sample to communities located within 20km of the Ivorian border. Again, this bandwidth is wide enough to ensure statistical power, but narrow enough to include only communities that are likely to share similar characteristics due to their proximity to the border. Table A.9 replicates this analysis, restricting my sample to communities located within 15km (columns 1-3) or 25km (columns 4-6) of the Ivorian border instead. My point estimates are again nearly identical at these alternate bandwidths.

Table A.9: Effects on citizens' preferences using proximity to Para as instrument, robustness to alternate buffers

	Rely on informal	Rely on formal	Rely on UNMIL	Rely on informal	Rely on formal	Rely on UNMIL
Spent time with UNMIL	0.24 [0.16]	0.38 [0.17]**	-0.62 [0.14]***	0.20 [0.15]	0.40 [0.14]***	-0.60 [0.13]***
Observations	943	943	943	1,556	1,556	1,556
District FE	N	N	N	N	N	N
Community-level controls	Y	Y	Y	Y	Y	Y
Individual-level controls	Y	Y	Y	Y	Y	Y
Proximity to nearest city	15km	15km	15km	25km	25km	25km

Notes: Coefficients from two-stage least squares regressions using proximity to Para as an instrument for exposure to UNMIL in 2012. Columns 1-3 and 4-6 report results using 15km and 25km buffers, respectively. Estimates are weighted by the inverse probability of sampling. Standard errors, clustered by community, are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.10: Correlation between individual- and community-level exposure to UNMIL

	Spent time with UNMIL
Weekly patrols	0.26 [0.05]***
Monthly patrols	0.24 [0.04]***
Occasional patrols	0.16 [0.03]***
Interventions	0.10 [0.02]***
Public works	0.01 [0.04]
Observations	4,387
District FE	Y
Community controls	Y
Individual controls	Y

Notes: Coefficients from OLS regressions. Estimates are weighted by the inverse probability of sampling. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.5 Robustness checks and ancillary analyses

A.5.1 Correlation between individual- and community-level proxies for exposure to UNMIL

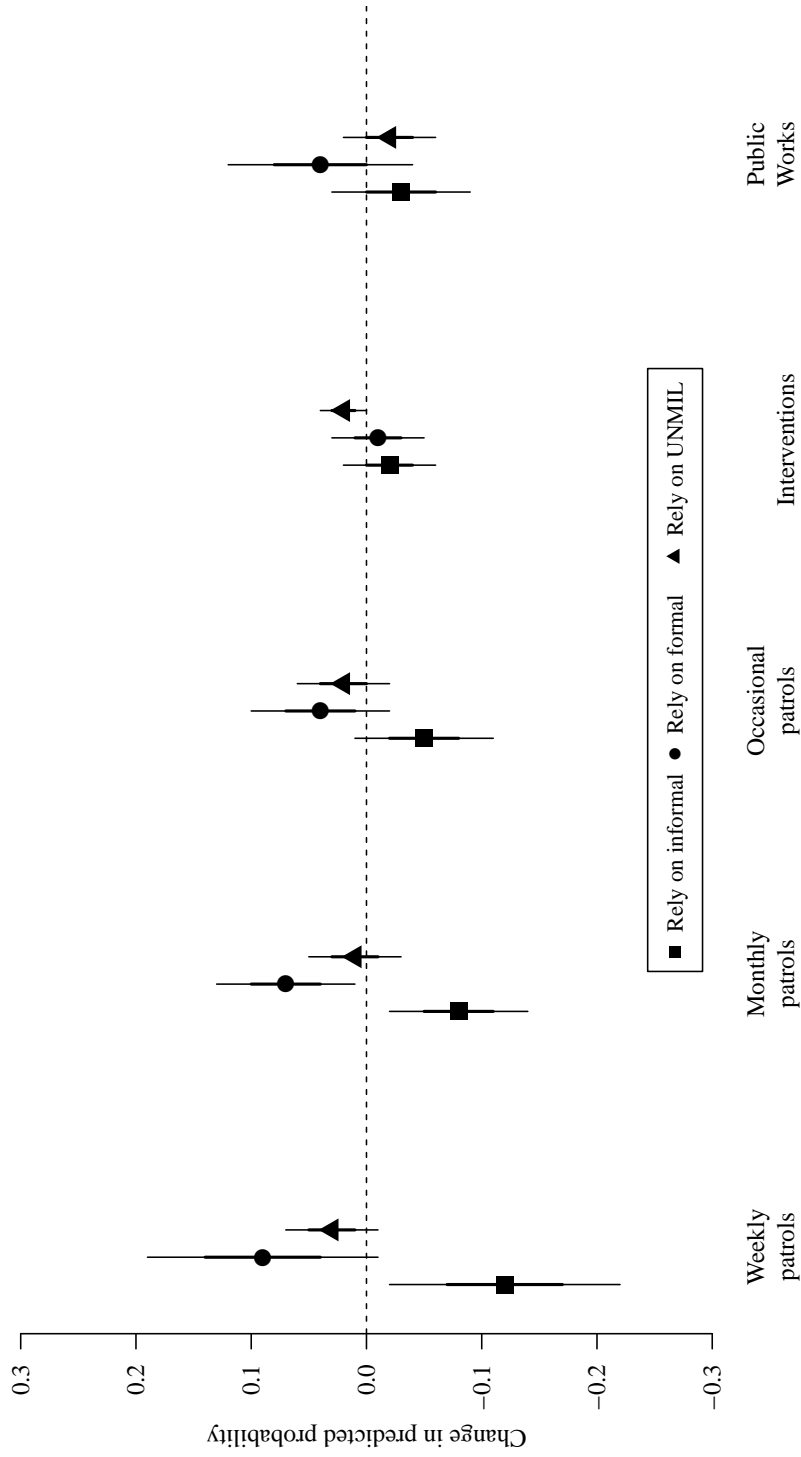
Table A.10 reports the correlation between community- and individual-level proxies for exposure to UNMIL. The former are derived from my survey of local leaders, the latter from my survey of residents. Residents were much more likely to report interacting with UNMIL personnel in communities whose leaders reported UNMIL patrols or interventions in the previous year. In communities that reported weekly patrols, residents were 26 percentage points more likely to report interacting with UNMIL. The correlation is smaller for less frequent patrols, smaller than that for interventions, and smaller still (and statistically indistinguishable from zero) for public works.

These discrepancies make sense. For many civilians, patrols are the most visible reminder of the mission's presence (?). As a Military Observer explained to me, during patrols, "we sit with [civilians], eat with them, interact with them, and spend most of the time with them just mingling." These interactions are indispensable in that they allow peacekeepers to "guarantee the local population that the UN is on the ground" (MS, Gbarnga, 10/11/2010). In contrast, while interventions to resolve disputes may involve direct (and potentially quite intense) contact between peacekeepers and disputants themselves, they are less conducive to interactions between peacekeepers and community members more broadly. Construction sites provide even fewer opportunities for interaction, and citizens may mistakenly attribute public works to the government or other NGOs.

A.5.2 Effects on citizens' preferences, disaggregated by hypothetical scenario

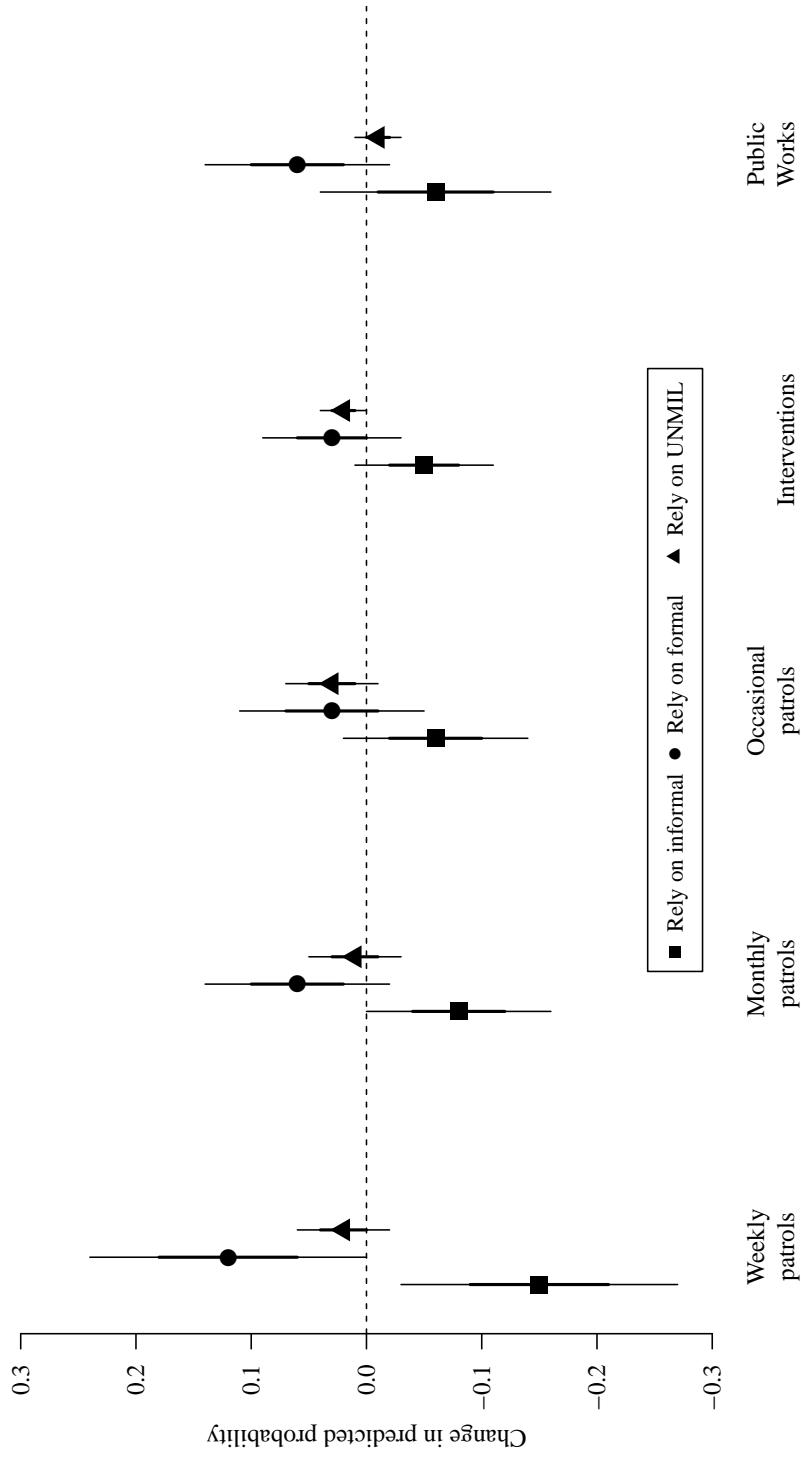
In the manuscript I test the effects of exposure to UNMIL on citizens' modal preference over potential security providers in three hypothetical scenarios of interpersonal (murder) and collective violence (mob violence and ethnic riots resulting in fatalities). In Figures A.2, A.3 and A.4 I

Figure A.2: Effects on citizens' preferences using local leaders' reports of exposure to UNMIL, murder only



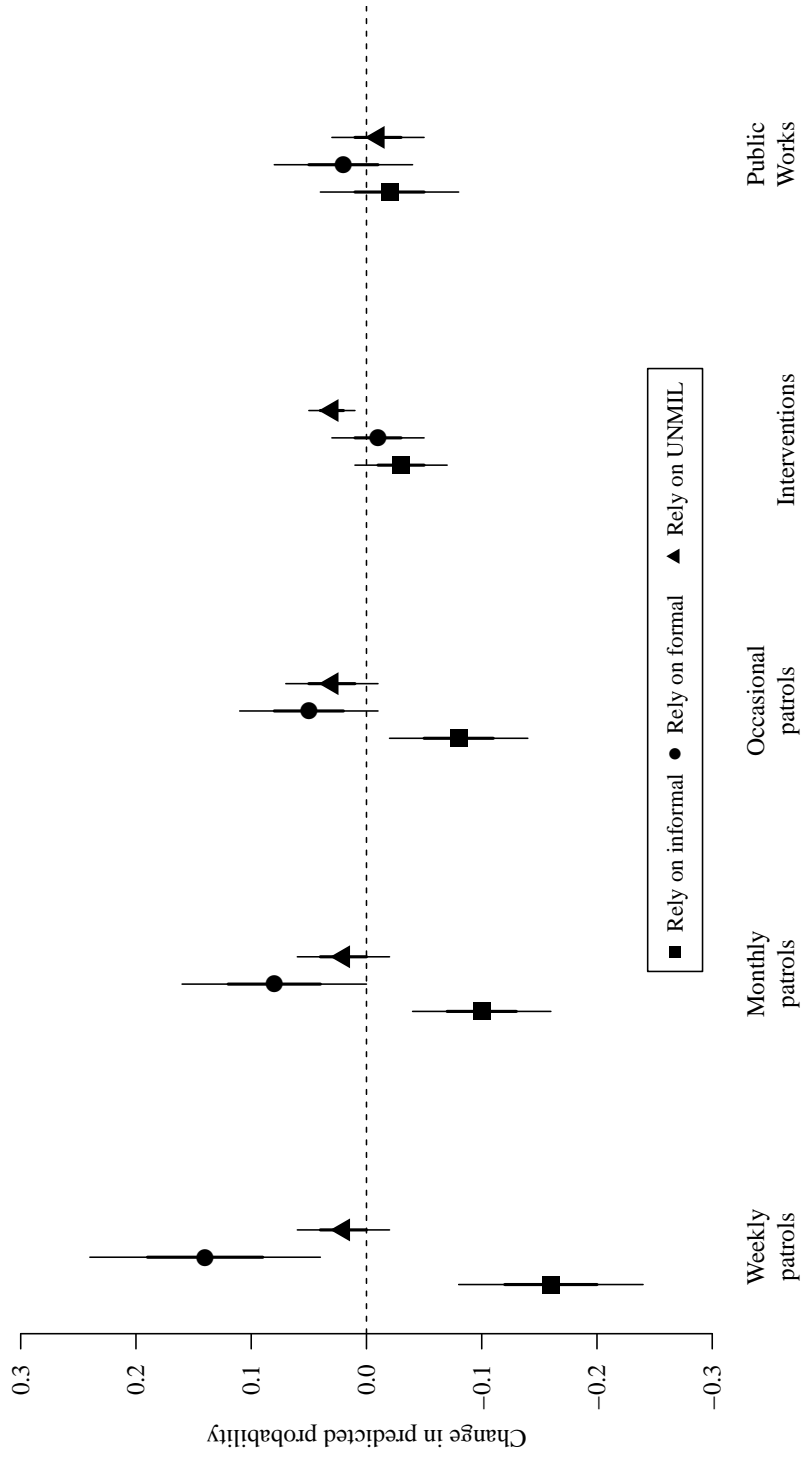
Notes: Marginal effects and 90 and 95% confidence intervals from multinomial logit regressions, holding all controls at their means. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community.

Figure A.3: Effects on citizens' preferences using local leaders' reports of exposure to UNMIL, mob violence only



Notes: Marginal effects and 90 and 95% confidence intervals from multinomial logit regressions, holding all controls at their means. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community.

Figure A.4: Effects on citizens' preferences using local leaders' reports of exposure to UNMIL, ethnic riots only



Notes: Marginal effects and 90 and 95% confidence intervals from multinomial logit regressions, holding all controls at their means. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community.

replicate this analysis, disaggregating by hypothetical scenario. The results are similar across specifications: residents of communities that reported UNMIL patrols were more likely to prefer formal institutions, and less likely to prefer informal ones. The more frequent the patrols, the stronger the effect. Residents of communities that reported interventions were more likely to express a preference for UNMIL, though this effect is statistically indistinguishable from the effect on preferences for the state. Residents of communities that reported public works projects were *less* likely to express a preference for UNMIL, though again, this effect is statistically indistinguishable from the effect on preferences for the state.

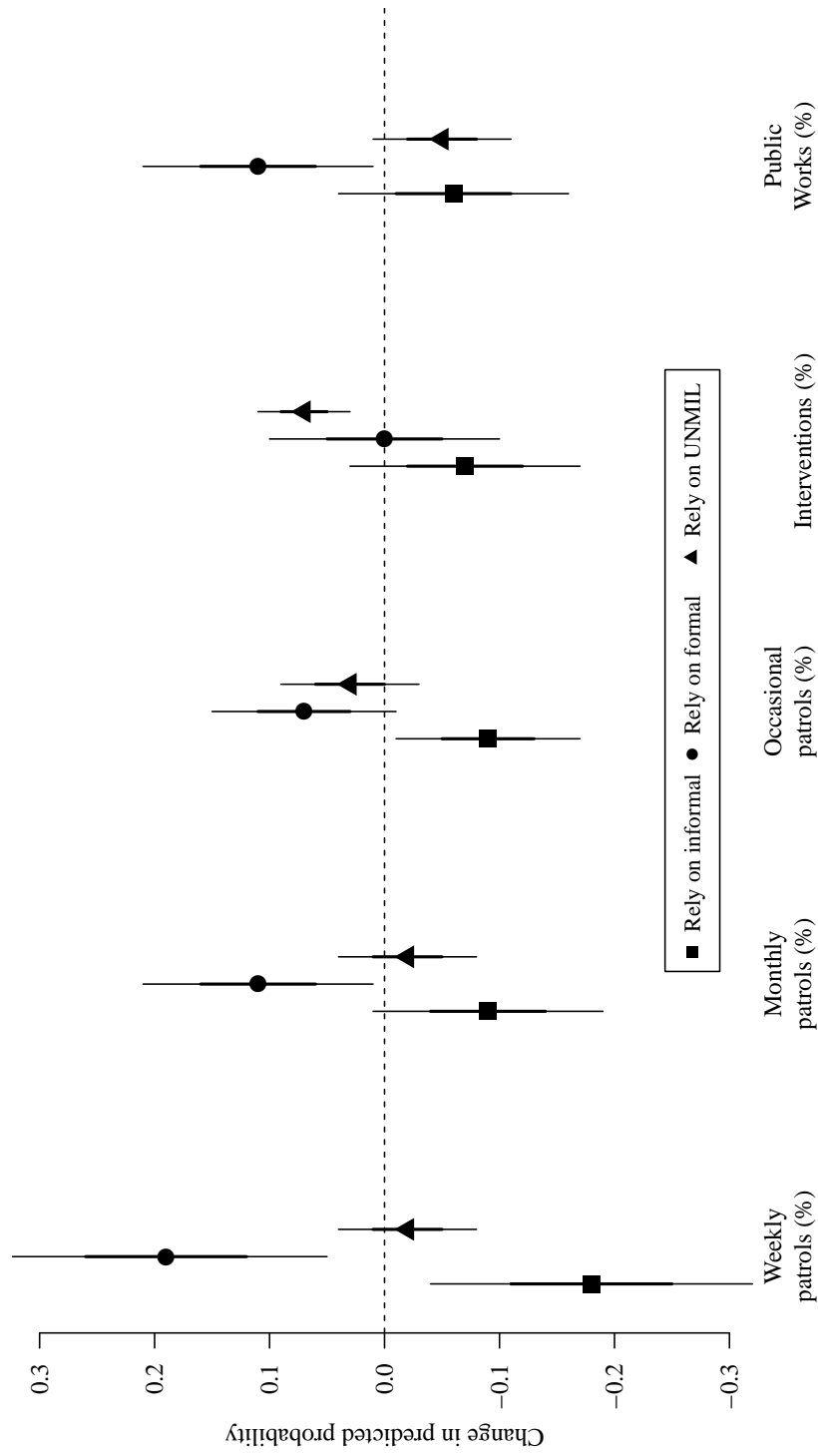
A.5.3 Effects on citizens' preferences, weighting by degree of agreement in local leaders' reports

In the manuscript I use a survey of local leaders to operationalize three different types of exposure to UNMIL: patrols, public works, and interventions to resolve impending or ongoing disputes. To minimize measurement error, I code dummies for the modal response across the four local leaders in each community. As an alternative, in Figure A.5 I code the proportion of local leaders in each community that reported each type of exposure, then use those proportions as independent variables. My specification is otherwise identical to the one in the manuscript, with individual- and community-level controls, district fixed effects, inverse probability sampling weights, and standard errors clustered by community. The results are virtually identical to those in the manuscript, providing further evidence that my findings are not artifacts of measurement error in the independent variables.

A.5.4 Correlation between birthplace and proximity to Para

The killing of seven UNOCI peacekeepers in Cote d'Ivoire was a tragic and highly disruptive event. My instrumental variables strategy assumes that the only mechanism through which the ambush might have affected Liberians' attitudes towards state laws and institutions was through the higher

Figure A.5: Effects on citizens' preferences using local leaders' reports of exposure to UNMIL



Notes: Marginal effects and 90 and 95% confidence intervals from multinomial logit regressions, holding all controls at their means. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community.

Table A.11: Correlation between birthplace and proximity to Para

	Born in community		
Proximity to Para	-0.01 [0.01]	-0.01 [0.01]	-0.01 [0.01]
Observations	966	1,418	1,597
District FE	N	N	N
Community controls	Y	Y	Y
Individual controls	Y	Y	Y
Proximity to Ivorian border	15km	20km	25km

Notes: Coefficients from OLS regressions. Estimates are weighted by the inverse probability of sampling. Standard errors, clustered by community, are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

“dose” of peacekeeping to which they were subsequently exposed. Tables A.11, A.12 and A.13 empirically explore three possible violations of this excludability assumption.

One possible violation is that the ambush precipitated an influx of Ivorian refugees into Liberia, which, in turn, may have affected citizens’ preferences over potential security providers (though the likely direction of this effect is unclear). Unfortunately I do not have data on the population of Ivorian refugees living in the communities in my sample. I do, however, have data on the proportion of residents that were born in each community—a proxy for migration. Since refugees are, by definition, migrants, this is an imperfect but still informative proxy. It will only be misleading if communities near Para hosted more Ivorian refugees but *fewer* migrants from other locations, and if the latter difference offsets the former. (In this case, there will appear to be a zero or negative correlation between migration and proximity to Para, despite a positive—but unobserved—correlation between displacement and proximity to Para.) But I see no reason to suspect this is the case.

In Table A.11 I find that residents living near Para were no more or less likely to be migrants than those living further away. This is perhaps unsurprising, since the Liberian government closed the border with Cote d’Ivoire just four days after the attack, and did not reopen it until November of that year. Moreover, violence during the Ivorian civil war occurred throughout the Cote d’Ivoire’s

southwestern departments; while thousands of Ivorian refugees *did* cross the border during the conflict, I am unaware of any evidence to suggest that they congregated in the region surrounding Para. The border communities in my sample share similar demographic, economic and political profiles; I see little reason to suspect that communities near Para were more plausible hosts than those near other points along the border.

A.5.5 Correlation between interpersonal and collective violence and proximity to Para

A second possible exclusion restriction violation is that the ambush near Para exacerbated violence in proximate Liberian communities. Again, this seems unlikely given the border closure, and given that Para was not especially strategically important—indeed, as I discuss in the manuscript, the district in which Para is located did not suffer any other incidents of violence in the first half of 2012, and only two more in the second half. Nonetheless, in Table A.12 I test the correlation between proximity to Para and residents’ reports of interpersonal (rape or murder) and collective violence (ethnic riots or violent strikes or protests) in 2012. Again, I find that residents living near Para were no more likely to report these incidents than those living further away.

A.5.6 Correlation between police presence and proximity to Para

A final possible excludability violation is that the ambush provoked an increase in police as well as peacekeeper presence near Para. Liberian security forces did intensify patrolling and other activities along the border in response to the attack, but their capacity remained severely constrained and their presence extremely limited. Table A.13 tests the correlation between proximity to Para and local leaders’ reports of LNP patrols in their communities. To estimate the frequency of these patrols, I take the modal response across the four leaders in each community, following the same procedure I used to estimate the frequency of UNMIL patrols. (Unfortunately I do not have data on military patrols, but given that *all* of Liberia’s security forces were involved in the response,

Table A.12: Correlation between interpersonal and collective violence and proximity to Para

	Interpersonal violence	Collective violence	Interpersonal violence	Collective violence	Interpersonal violence	Collective violence
Proximity to Para	0.00 [0.01]	-0.00 [0.01]	-0.00 [0.01]	-0.00 [0.01]	-0.00 [0.01]	0.00 [0.00]
Observations	966	966	1,418	1,418	1,597	1,597
District FE	N	N	N	N	N	N
Community controls	Y	Y	Y	Y	Y	Y
Individual controls	Y	Y	Y	Y	Y	Y
Proximity to Ivorian border	15km	15km	20km	20km	25km	25km

Notes: Coefficients from OLS regressions. Estimates are weighted by the inverse probability of sampling. Standard errors, clustered by community, are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.13: Correlation between police presence and proximity to Para

	Weekly or monthly police patrols		
Proximity to Para	-0.02	0.00	0.00
	[0.02]	[0.02]	[0.02]
Observations	59	88	97
District FE	N	N	N
Community controls	Y	Y	Y
Individual controls	N	N	N
Proximity to Ivorian border	15km	20km	25km

Notes: Coefficients from OLS regressions. Standard errors are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

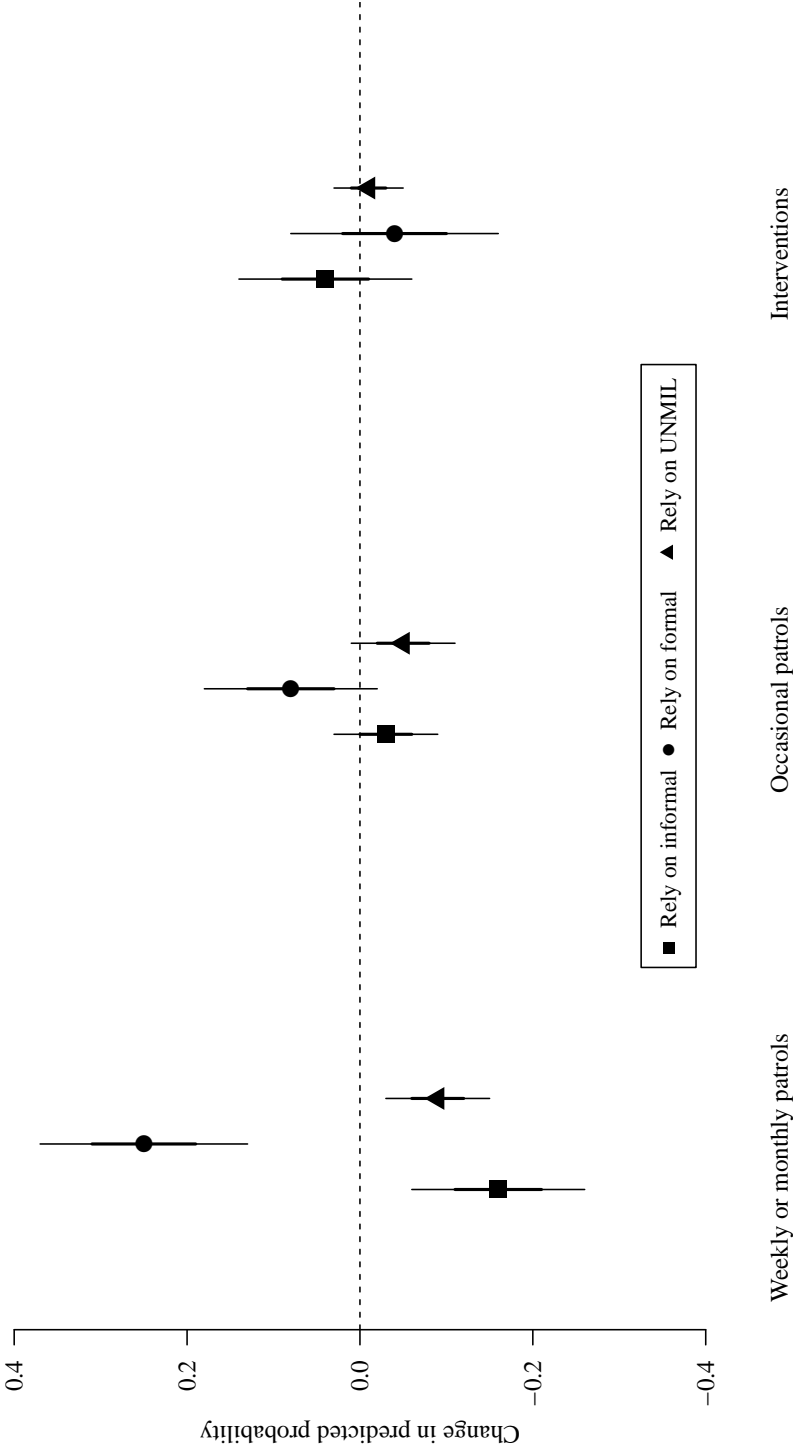
the correlation with police patrols should be equally informative.) I find that communities located near Para were no more likely to report LNP patrols than those located further away.

Together, the results in Tables A.11, A.12 and A.13 lend additional credence to the excludability assumption. While the exclusion restriction is ultimately untestable, and while other violations are possible, these three are the most obvious. My nulls do not conclusively prove that excludability holds, but they provide strong suggestive evidence that it does.

A.5.7 Persistence of effects on citizens' preferences and perceptions after two years

How likely are my results to persist even after UNMIL's withdrawal? While I cannot answer this question directly—the last round of surveying occurred in early 2013, when UNMIL was still present in Liberia, albeit at a smaller size than in previous years—I can provide some suggestive evidence. In Figure A.6 I test whether exposure to UNMIL in 2010 continued to affect citizens' attitudes two years later, focusing on the subset of communities that reported no further exposure to UNMIL of any kind in 2012. Due to sparseness in the data when subsetting in this way, for this specification I exclude public works and combine weekly and monthly patrols into a single

Figure A.6: Persistence of effects on citizens' preferences using local leaders' reports of exposure to UNMIL, exposed in 2010 only



Notes: Marginal effects and 90 and 95% confidence intervals from multinomial logit regressions, holding all controls at their means. Exposure to UNMIL is measured in 2010; citizens' preferences are measured in 2012. Sample is restricted to communities that report no exposure to UNMIL in 2012. Estimates are weighted by the inverse probability of sampling. Standard errors are clustered by community.

indicator.

The results in Figure A.6 are consistent with—and in some respects even stronger than—those in the manuscript. Interestingly, in the medium term and in this subset of communities, exposure to UNMIL patrols actually *reduced* citizens' preference for UNMIL as a purveyor of security and justice, both in absolute terms and relative to citizens' preference for the state. This provides further evidence that UNMIL did not induce dependence on the mission's presence; indeed, if anything the opposite appears to be true.

Table A.14 extends this analysis by testing the effects of exposure to UNMIL in 2010 on citizens' perceptions of state corruption and bias in 2012, again focusing on the subset of communities that reported no further exposure to UNMIL. I find some suggestive evidence that UNMIL interventions aggravated perceptions of state bias in the medium term in this subset of communities, but this effect is only weakly statistically significant, and is specific to interventions. These analyses should be interpreted with some caution, since UNMIL may have decided to discontinue patrols, interventions and public works in particular communities for reasons that are endogenous to citizens' perceptions. With this caveat in mind, my results suggest that the adverse effects of UNMIL's presence dissipated over time while the beneficial effects persisted, even after UNMIL personnel were no longer physically present.

Table A.14: Persistence of effects on citizens' perceptions using local leaders' reports of exposure to UNMIL, exposed in 2010 only

	Believes formal is corrupt	Believes formal is biased	Believes informal is corrupt	Believes informal is biased	Believes UNMIL is corrupt	Believes UNMIL is biased
Weekly or monthly patrols	0.08 [0.05]	-0.05 [0.06]	-0.14 [0.10]	0.02 [0.04]	-0.00 [0.05]	-0.02 [0.07]
Occasional patrols	0.04 [0.05]	-0.04 [0.05]	-0.03 [0.07]	0.08 [0.03]**	-0.06 [0.05]	-0.02 [0.05]
Interventions	-0.03 [0.05]	0.07 [0.04]*	-0.06 [0.04]	-0.00 [0.04]	-0.01 [0.03]	-0.02 [0.04]
Observations	909	944	933	974	790	872
District FE	Y	Y	Y	Y	Y	Y
Community-level controls	Y	Y	Y	Y	Y	Y
Individual-level controls	Y	Y	Y	Y	Y	Y

Notes: Coefficients from OLS regressions. Estimates are weighted by the inverse probability of sampling. Exposure to UNMIL is measured in 2010; citizens' perceptions are measured in 2012. Standard errors, clustered by community, are in brackets. *** p<0.01, ** p<0.05, * p<0.1