The Subtle Micro-Effects of Peacekeeping: Evidence from Liberia*

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Abstract

We use original survey data and administrative data to test a theory of the micro-level impacts of peacekeeping. The theory proposes that through the creation of local security bubbles and also through direct assistance, peacekeeping deployments contribute to economic and social revitalization that may contribute to more durable peace. This theory guides the design of current United Nations peacekeeping operations, and has been proposed as one of the explanations for peacekeeping's well-documented association with more durable peace. Our evidence paint a complex picture that deviates substantially from the theory. We do not find evidence for local security bubbles around deployment base areas, and we do not find that deployments were substantial contributors to local social infrastructure. In addition, we find a negative relationship between deployment basing locations and NGO contributions to social infrastructure. Nonetheless, we find that deployments *do* seem to stimulate local markets, leading to better employment possibilities and substantially higher incomes. The result is something of a puzzle, suggesting that more work needs to be done on other types of direct assistance by peacekeeping contingents—e.g. the impact of mission procurement and routine spending by those associated with the mission. Also, the findings with respect to NGO activities suggest that this is an important factor that past case studies and cross-national studies have not taken into account sufficiently.

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1 Introduction

Multidimensional peacekeeping is integral to strategies by the international community to help establish stable and self-sustaining peace in countries coming out of civil war. Traditionally, peacekeeping was limited to military and security functions such as monitoring ceasefire agreements or setting buffer zones between the belligerents and mostly intervened in the context of interstate wars. In the last two decades, however, peacekeeping saw a dramatic expansion to 'non-military' functions such more peacekeeping operations were now tasked with revitalizing the economy, rebuilding infrastructures, fostering democracy, promoting human rights, among other things. In practice, these broader, non-military activities are undistinguishable from traditional development activities. Peacekeeping operations that have both the military and civilian components are generally referred to as 'integrated' or 'multi-dimensional' peacekeeping operations and typically take place in the context of civil war.[1] These operations are undertaken on the theory that military means alone cannot lead to self-sustaining peace unless accompanied by robust efforts to address the structural factors responsible for the outbreak of civil war in the first place as well as the economic and political consequences of war that might leave societies vulnerable to renewed conflict (Boutros-Boutros Ghalis 1995; Doyle and Sambanis 2006).[2] These multidimensional peacekeeping operations exhibit variation in terms of their size, scope and resources.[3] One feature common to most of them, however, is their involvement in the social, economic and political aspects of the host country.

But to what extent do these multi-dimensional peacekeeping operations reach their objectives? Recent quantitative studies have established that these operations are effective in making peace

[1]The distinction between peacekeeping and peacebuilding goes beyond 'military' vs. 'non-military' dichotomy. See Galtung (1975) and Call and Cook (2003) for a discussion of the different perspectives on these concepts. Through this paper, however, we use peacekeeping as a short hand for multidimensional peacekeeping. But we will occasionally use 'peacebuilding activities,' when emphasizing the non-military activities carried out within the framework of multidimensional peacekeeping operations.

[2]Peacekeeping operations typically take the lead in carrying out these peacebuilding activities. However, other international actors, including bilateral donors, NGOs and relief and development agencies, have been playing a growing role as well.

[3]The largest multidimensional peacekeeping operation, the United Nations Mission in the Democratic Republic of Congo (MONUC in its French acronym), counted in its ranks more than 20,000 international peacekeepers and hundreds of international and national civilian staff at its as well as an annual operating budget of nearly 1.5 billion US dollars.

much more likely to last after civil war, presumably by restoring security and fulfilling other non-security goals such as economic revitalization or democratization (Doyle and Sambanis 2000, 2006; Gilligan and Sergenti 2008; Fortna 2008a).[4] This conclusion is widely shared within policy circles. Indeed, practitioners are increasingly confident about the positive role of peacekeeping in building peace after civil war. For instance, in her recent testimony to the House Foreign Affairs Committee, Susan Rice, the United States Ambassador to the United Nations, claimed that many countries are more peaceful and stable today due to UN peacekeeping. She attributed this outcome to the peacebuilding activities undertaken within the framework of peacekeeping: "today's UN operations do much more than just observe cease-fires. They provide security and access so that humanitarian aid can reach the sick, the hungry, and the desperate. They help protect vulnerable civilians and create conditions that will allow refugees to return home. And they help emerging democracies hold elections and strengthen the rule of law."[5]

Despite the consensus in the empirical literature and the growing confidence of policymakers about the positive role of peacekeeping in helping to build peace after civil war, we actually have little insight into peacekeeping's peacebuilding efforts. Nor do we have a good understanding of why peacebuilding outcomes vary across geography, even in the context of one country. Why, for example, do some areas regain security faster, while others continue to be trapped into violence for years after civil war? Or why do some areas succeed in revitalizing their economies soon after civil war while others remain sluggish for years? Unfortunately examples of these variations are frequent in war-torn countries. The current literature does not provide adequate answers.

Part of the problem is that existing quantitative studies of peacekeeping effectiveness have been carried out at the mission and country levels, focusing on the question of whether peacekeeping operations have succeeded in preventing the recurrence of another war. The implicit assumption in this literature has been that once peacekeeping realizes gains (in the security or other realms) at the country level, these would trickle down to individuals and communities. This is certainly the

[4] However, different scholars use different criteria to assess peacekeeping effectiveness. Some (see, for example, Walter 2001; Fortna 2008a) define success minimally in terms of preventing the risk of another war. Others (see, for example, Doyle and Sambanis 2000, 2006) include democratization as an additional criterion. Still others (see, for example, Paris 1997, 2004) define effectiveness more broadly to include political and economic liberalization.

[5] http://www.state.gov/p/io/rls/rm/2009/126740.htm

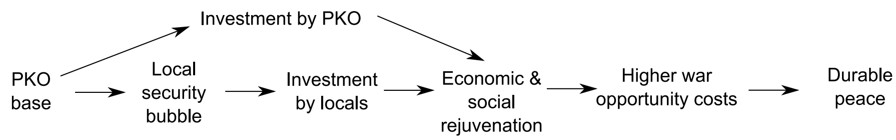
impression we get from studies on credible commitment (see, for example, Walter 2001; Fortna 2008a), which attribute lasting peace to the ability of peacekeeping to make commitments more credible or provide security guarantees to warring factions. If peacekeepers can build trust and confidence between belligerent factions (for example, by getting the, to agree on powersharing and get weapons of out of the hands of their rank-and-file), the reasoning goes, security will automatically follow in the country as a whole. Yet, the debilitating experiences in the DRC and Haiti belie this reasoning. While overall security conditions in the country improved after the advent of peacekeepers, some communities continued to experience violence and in some cases the situation even worsened than before the arrival of peacekeepers. As a number of scholars have pointed out, failure of peacebuilding at the local can ultimately doom the international peacebuilding efforts at the national level (for instance, Austessere 2010 makes this argument in the case of the Democratic of Congo).

A related problem is that the current literature seldom examines non-security peacebuilding processes.[6] By definition, multidimensional peacekeeping operations do more than just (re)-establish peace and security in war-torn societies. They also tend to have a substantial civilian component whose mission is to rehabilitate the social, economic and political infrastructures left in ruins by civil war. This component typically encompasses a broad range of activities spanning from repatriation of refugees and other displaced populations to revitalization of local economies to livelihood and employment creation to reconstruction of physical infrastructures to provision of political development assistance or human rights promotion, among others, to name a few.[7] Some of these programs activities target the more immediate needs of the local population while others

[6]A handful of empirical studies have looked at the impact of peacekeeping on democratization focusing on cross-country comparisons (e.g., Wantchekon 2004; Fortna 2008b; Bueno de Mesquita and Downs 2006; Watts 2008), while a few others (see, for example, Woodward 2002; Cooper 2005; Ballentine and Nitzschke 2005; Wennman 2005; Felbab-Brown 2009) have provided descriptive accounts possible linkages between peacekeeping and local economies.

[7]For a comprehensive listing and a detailed description of each of common peacebuilding components, see the Handbook on United Nations Multidimensional Peacekeeping Operations accessible at http://www.peacekeepingbestpractices.unlb.org/Pbps/library/Handbook on UN PKOs.pdf. There are many descriptive studies of peacekeeping operations. These include the "Lessons Learned" reports conducted within the UN Department of Peacekeeping Operations (UNDPKO) itself. While extremely useful, these tend to be limited to descriptions of "key" episodes in a peacekeeping mission. They do not attempt to measure the general impact of peacekeeping activities. An exception is the recent study by Humphreys and Weinstein (2007), which examined the disarmament, demobilization and reintegration (DDR) component of the peacekeeping mission in Sierra Leone. Our study follows in the example set by that study, although we cover different types of activities and peacebuilding outcomes.

Figure 1: **A theory of local peacekeeping and durable peace**



aim to address the long-term developmental issues (Kumar 1997 and 1998; Chopra 1998; Russett and O'Neil 2001; Stedman et al. 2002; Gueli and Huyssteen 2007). Moreover, these peacebuilding activities, especially the longer-term activities, are often undertaken with the explicit aim to transform the social, economic and political structures of war-torn societies (at least with respect to the wartime status quo, if not with respect to pre-war conditions) (Paris 2004; Pouligny 2005, 2006; Kumar 1997; Del Castillo 2008). These transformations are presumed to be an essential prerequisite to political stability and lasting peace because they are supposed to reduce the likelihood of renewal of violence. Indeed, these peacebuilding activities are a central presumption in what we understand as the implicit theory that motivates integrated peacekeeping interventions. Yet, very few studies have attempted to examine systematically the effectiveness of these peacebuilding interventions or the patterns of their outcomes across geography.[8]

Nonetheless, the literature provides some insights, which form the basis of our peacebuilding hypotheses. The basic theory is sketched out in Figure 1. We contend that a disaggregated approach may offer a nuanced view of peacebuilding dynamics and outcomes. We propose that there is substantial variation in peacebuilding outcomes at the local level and these outcomes may be a function of exposure to peacekeeping deployments as well as the various non-military activities under the auspices of peacekeeping operations. More specifically, we suggest two hypotheses of how peacekeeping might provide the building blocks of durable peace in the target country. The first hypothesis is that peacekeeping deployments create a security environment that enables autonomous socioeconomic and political recovery processes. The operating mechanism is through improving people's perceptions of security. In this view, peacekeeping deployments improve socioeconomic

[8]A handful of studies have examined some of these peacebuilding outcomes, without reference to peacekeeping (see, for sample, Bellows and Miguel [2007] on Sierra Leone and Humphreys and Weinstein [2009] on Liberia). To our knowledge, only Mvukiyehe and Samii (2009, 2010) attempted to examine peacebuilding outcomes undertaken within the framework of peacekeeping in Cote d'Ivoire and Liberia.

and democratic outcomes and such improvements should be decreasing in distance to deployment. An alternative hypothesis is that socioeconomic and political improvements are a result of direct assistance (e.g. material assistance, sensitization on human rights issues or democracy) provided through peacekeeping operations.

Understanding peacebuilding processes is crucial. Not getting peacebuuilding right can lead to the reversal of peace the processes, costing innocent lives and resources. as experiences from Angola to Cambodia to Rwanda have shown (Orr 2002). We advance the study of peacekeeping and peacebuilding in at least three ways. First, intermediate peacebuilding outcomes are potential mechanisms at work in peacebuilding. Logically, if we do not find that peacekeeping affects some intermediate outcome, then we begin to cast doubt on the claim that such outcomes are integral to the mechanism through which peacekeeping prolongs peace. Of course, there are limits to what we can infer. A finding of positive micro-level effects in conjunction with sustained macro-level peace within a single country does not allow us to infer that the micro effects and macro peace are related. Such inferences require adding other macro-level comparisons to the analysis. This micro-level analysis can thus inform future macro-level analysis. Second, intermediate outcomes are more under control of peacekeeping (insofar as they are direct effects of specific programs and activities) than is the end outcome (i.e. overall peacebuilding). The latter is likely to be influenced by other factors outside control of peacekeeping. Thus, from a policy perspective, intermediate outcomes may make it possible to determine what about peacekeeping works and what does not. Third, we contribute to developing micro-level methods for studying peacekeeping impacts. The micro-level quantitative approach allows us to construct, with rigor, a nuanced picture of what peacekeeping operations actually do inside a country.

We use a micro-level approach to test these hypotheses within the context of the United Nations Mission in Liberia (UNMIL). We use original survey data, conflict event data, and other socio-economic variables to measure micro-effects of peacekeeping in a number of ways. This is still a work in progress, but at present we find that the results are quite surprising. For the most part, we do *not* estimate any effects indicative of the types of mechanisms that are presumed to operate at the micro-level. Most important to us is the lack evidence that peacekeepers have any substantial impact on *local* security, as should be manifested in a number of behaviors, such as resettlement, investment in one's community, and economic and social revitalization. We also find evidence that rather than creating a local environment to facilitate the work of NGOs, NGO efforts seem to occur in places where peacekeepers are *not* based. Perhaps this is due to intentional complementarity, or some kind of crowding out. Finally, with respect to mechanisms, we do not find that deployment base localities are well served by peacekeepers in terms of their contribution to building social infrastructure (wells, schools, and health posts). What we *do* find is that peacekeeping base locations are sites of substantially greater market vitality. Occupations and incomes suggest that base locations have much more vibrant labor markets. However, there is no associated increase in the vibrancy of community life: participation in self-help organizations does not not increase, and social club participation is actually lower. The results are intriguing. It may be that forms of direct assistance other than contributions to social infrastructure are at work, for example by way of procurement or even routine expenditure by those associated with the peacekeeping deployment.

The paper proceeds as follows: the next section provides a bit of background to the Liberian civil war and UNMIL's intervention. Section three describes our analytical methods for identifying peacekeeping effects. Next, we present findings with respect to intermediate peacebuilding outcomes. We conclude by discussing how we can improve the study of peacekeeping with micro-level analysis.

2 Background to UNMIL Intervention

Liberia, a small coastal country in Western Africa, was embroiled in a 14-year civil war that claimed the lives of 200,000 people and displaced more than a million more into neighboring countries.[9] The causes of this civil war were multiple and complex (Amos 2005). Armed rebellion started in 1989 when the self-proclaimed National Patriotic Liberation Front (NPLF) launched attacks in Nimba County, from neighboring Ivory Coast. The conflict escalated when the Kran-dominated government forces retaliated against civilian populations from the Mandingo and Gio

[9] http://www.un.org/en/peacekeeping/missions/unmil/background.shtml

tribes of the region. The rebel forces led by Charles Taylor, a former government employee, quickly overran much of the countryside and a splinter faction led by Prince Johnson capture and executed the dictator Samuel Doe. This, however, only plunged the country into further chaos and violence, as neither of the different faction was able to gain the upper hand.

From the outset, the Economic Community of West African States (ECOWAS) undertook several peace initiatives and established a Military Observer Group (ECOMOG) to support these efforts (Adebajo 2002; Amos 2005). This civil war saw many twists and turns as several ceasefires were signed and violated by the warring parties. With the support of the newly established United Nations Observers' mission in Liberia (UNOMIL), ECOMOG's brokered several peace agreements between the warring parties, which resulted in the 1997 presidential elections that brought Charles Taylor to power. The window of peace, however, was short-lived, as two new armed factions, the Liberians United for Reconciliation and Democracy (LURD) and the Movement for Democracy and Elections in Liberia (MODEL), emerged in 1999, vowing to overthrow Charles Taylor's government purportedly due to the endemic corruption and the inadequacy of promised political and security reforms. The situation came to a head in 2003 when Charles Taylor was forced to step down under pressure from the United States. Meanwhile, several political actors and members of civil society were involved in peace talks in Accra, Ghana, in an attempt to put together a Transitional Government that would be tasked with completing the peace process and organizing elections. These new developments paved the way for the establishment multidimensional peacekeeping operation, the United Nations Missions in Liberia (UNMIL) comprised of 15,000-strong international peacekeepers and hundreds of international and local civilian personnel. The mission was tasked with, among other things, to support the implementation of the comprehensive peace agreements and help rebuild the social and economic infrastructures left in ruin by the conflict. The political transition culminated in fairly successful legislative and presidential elections in 2005 and the country is now gearing towards its second postwar elections in 2011.

3 Causal identification

Our aim is to estimate the causal effect of the location of peacekeeping bases on security and economic outcomes.[10] In order to do so, we need to attend to two sources of potential bias. The first source of potential bias is confounding due to unmeasured covariates, which may cause omitted variable bias. The second source of potential bias is spill-over effects. Here we discuss the methods and assumptions needed to mitigates these threats to the valid estimation of local peacekeeping impacts.

3.1 No unmeasured confounding

Peacekeepers' base locations are not randomly chosen. Therefore any attempt to estimate the causal effects of peacekeeping basing will depend on the plausibility of an assumption of "no unmeasured confounding." This assumption cannot be tested, although we can perform sensitivity analysis to see how sensitive are our results to violations of the assumption.

We assume no unmeasured confounding after matching and regression adjustment. We use coarsened exact matching (Iacus et al, 2009), which strictly limits the amount of confounding due to possible direct effects and interaction effects of covariates to within the amount of variation that remains within the coarsening cells. We exact match on coarsened pre-deployment community characteristics to construct sets of comparable communities that do and do not host deployment bases. The coarsening implies that some imbalance will remain on the uncoarsened covariates in matched sets. Such imbalance may introduce omitted variable bias in our estimates of peacekeeping impacts, although the bias should be small in that the matching reduces the scope for such bias considerably. To remove this small residual bias, we use regression after matching. Regression essentially extrapolates and interpolates over the small regions where covariates do not overlap. In doing so it provides synthetic "exact" matches (Rubin and Thomas, 2000). The fact that we have already matched reduces considerably the scope for introducing specification bias in our attempt to remove residual omitted variable bias. King and Zeng (2006) have found that matching, if done well, tends to make the specification bias negligible in which case the gains in bias reduction from

[10]We use the words "impact" and "causal effect" interchangeably.

the regression adjustment should outweigh any bias introduced by specification error.

To merely match and regress is not sufficient, in our view, to make the causal story persuasive. Like Sekhon (2009), we believe that a convincing causal story requires a *verifiable* source of exogeneous variation. The matching and regression are there to merely "clean-up" the imperfect natural experiment that the exogenous variation provides. In our case, we need something that can explain why communities that are the *same* in terms of potential security and economic outcome trajectories could *differ randomly* in whether they host peacekeeping bases.

We claim that the "fog of peacekeeping" combined with stickiness of basing decisions provides such a natural experiment. By the fog of peacekeeping, we mean that when peacekeepers are deployed, the information available to them for making basing decisions is very limited. Only rough information about conflict vulnerability and local infrastructure is available. Such information is able to narrow the range of possible basing locations somewhat, but not completely. Our identifying assumption is that the variables that we take into account in the matching produces a set of communities that on average, were equally plausible candidates to receive bases. Once the range of basing locations has been narrowed based on available conflict and infrastructure data, locations are selected as if by chance. By "stickiness" we mean that once a base is established, it is highly unlikely that it will be moved. This is because a base requires costly fixed investments. Therefore, even if new information should become available suggesting that there is a more optimal location to establish a base, it is unlikely that the base will be moved. If base allocations are exhausted, it is unlikely that a new base will be established there either. This stickiness is borne out if one consults maps of deployment locations [maps will be inserted.].

3.2 No spill-overs

Suppose we have matched two communities, A and B, and claim that in expectation, the two communities should have the same security and economic outcome trajectories were no peacekeepers to be based there. Suppose now that A is host to a peacekeeping base. We want to be able to make two claims in addition to the claim about common baseline conditions that we have already made. First, we want to claim that the outcome trajectory for A is the same that B would have were it to host a base. Second, we want to claim that the outcome trajectory for B given that only A hosts a base is the as the outcome trajectory for B were A *not* to host a base. This is part of the "stable unit treatment value assumption" in the causal inference literature (Rubin, 1978). It says that outcomes for a unit are determined *only* by the treatment assignment *for that unit*.

This claim may be violated in a number of ways. For example, it may be that the peacekeeping base in A causes people to start investing there. News of this may travel to B, raising the confidence of people there and inducing them to start investing in their own community at the same rate. If we based our judgment of impacts on differences between the two communities, we would end up saying that there are none. That would not be a valid causal estimate. Or, it could be that the base in A creates a local security bubble that attracts resources from people in B who want to invest in the conditions created in A. If we based our judgment of impacts by looking at the difference in resources being invested locally in a community, we would end up saying that there are large effects. However, if we were to put a base in B in addition to the one in A, then we should not expect the resulting increase in investment in B to equal what was previously the difference between A and B. Indeed, all else equal, if the amount of resources in A and B are equal, then the difference between resources in A and resources in B in the first scenario should be double the difference between resources in B in the first scenario and resources in B in the second scenario.

These are two examples of spill-over effects. There is no way to remove the inferential threats from spill-overs completely. The best we can do is to try to minimize them. To minimize the threats from direct spill-overs, we only matched communities that were relatively far apart from each other geographically. Given the difficulties of moving from place to place in Liberia, we felt that this would accomplish a lot. There is still the possibility that information about peacekeepers' activities could spread, creating spill-overs over arbitrary distances. This is something that we cannot control in our analysis. Our hunch is that these kind of spill-overs would bias our estimates toward the null: if word of peacekeepers doing good in one place travels to another, we expect that the result will be a positive reaction in those other places, muting the local impact of peacekeepers relative to the other places. Our data cannot speak to these possibilities. Perhaps the best thing to do would be look int othese possibilities with more in-depth probing of conditions in various communities. We leave this as something for further research.

We also need to state a scope condition to make our causal analysis coherent in the face of potential spill-overs. We only claim to measure local impacts for a peacekeeping operation that was able to establish as many bases as were established in Liberia. We accept that our estimates do not necessarily characterize what would happen were more or fewer bases to be established. Also, we only claim to measure impacts on a country that has resource mobility that resembles the circumstances of our sample. Our estimates do not necessarily characterize what would happen in places where this is different. Whether or not these scope conditions are restrictive depends on how unusual you think are the conditions associated with UNMIL and the Liberia context. That too is a judgment call beyond what our data can tell us.

4 Sample design

We used a simulation based power analysis to determine the sample size necessary for a household survey with at least 80% power to detect non-negligible treatment effects (e.g. 0.1 differences in proportions) with 95% confidence under moderate levels of intra-cluster correlation and treatment effect heterogeneity.[11] Based on this, we determined that a sample of approximately 25 base and 25 non-base communities with sample sizes of about 15 respondent households per community would meet power requirements to estimate effect sizes of policy relevance. The precise number of communities was determined through the coarsened exact matching procedure.

"Community" in this study refers to the administrative unit of the "clan," which is the third tier administrative unit in Liberia below county and district, but above village. Clans contain clusters of villages that are linked on the basis of traditional ties, and therefore circumscribe domains of routine economic and social interaction. "Clan" in this context should not be confused with a family unit. It refers specifically to a geographic area. On average, a clan contains about 700-1000 households (the average size for a household is between 5 and 6 people). In Monrovia, there are no clans, but rather administrative blocks that the Liberia Institute of Statistics and Geo-Information Services (LISGIS) has demarcated and that have approximately the same population as clans. LISGIS lists 673 clans and 165 administrative blocks, and these 838 units partition the entirety of

[11]The power analysis followed recommendations in Gelman and Hill (2007).

Liberia's territory.

We study outcomes in communities in Liberia outside the capital of Monrovia. The reason for excluding Monrovia is that it is, due to population density and economic vibrancy, a case apart from the rest of the country. Conditions there really ought to be studied in a separate analysis.

We matched communities on the following covariates, all of which we anticipated as potential confounders:

- Whether the community is in the coastal region, set as a binary covariate.
- Whether the community is in the Kru speaking region, set as a binary covariate.
- Proximity to the major road network, set as a binary covariate as "beside road" or not.
- Wartime conflict exposure, set as a three level covariate for low, medium, or high.
- Number of households, set as a three level covariate for small, medium, or large.
- Number of schools per household, set as a three level covariate for none, few, or many.
- Number of health posts per household, set as a two level covariate for none or some.

The covariate coarsening was determined by inspecting the histograms of the variables to see how communities naturally separated. This ensures that the coarsening reflects qualitative differences that are likely to be important in the real world.

The ethnicity, region, and road network measures were taken from pre-deployment era public domain maps posted to the online University of Texas Perry-Casteneda Library Map Collection. The conflict exposure data were pulled from the Peace Reasearch Institute of Oslo's Armed Conflict and Event Location Date file (2009 release). The population and social infrastructure data were drawn from a data file produced as part of a rapid baseline assessment conducted by the United Nations Office for the Coordination of Humanitarian Affairs (OCHA) in early 2004. This was early enough in the deployment period that population figures and infrastructure could not yet have been affected by deployments. These data were consulted by UNMIL itself in assessing baseline conditions, making them quite appropriate for the purposes of this study, although they did not provide the exclusive basis on which deployment base decisions were made.

We aim to estimate a type of "local average treatment effect," particularly an "effect of the treatment on the treated" communities for treated communities that can be matched.[12] By "treated" we mean that a community hosted a peacekeeping base. Therefore, the objective for the matching algorithm is to find non-base communities ("controls") that resemble each of the base communities ("treated"). Coarsened exact matching does so by creating stratification cells. Recall that the matching algorithm matches on coarsened versions of these covariates and all interactions. Thus, the covariate values permit up to 432 possible strata. Communities in Liberia occupy a total of 154 of these strata. That is, most combinations of covariate values do not characterize any communities. There are 44 communities that hosted peacekeeping bases. They are distributed over 28 of the strata. Among the 44 peacekeeping base communities, 26 of them occupy strata that are also occupied by non-base communities. We are thus able to match 26 out of the 44 base communities. The matched sets occupy 19 strata. There are a total of 25 non-base communities in these strata. Thus, we are matching 33 base communities to 29 non-base communities. The 11 base communities in the 8 strata that contain no non-base communities are thus excluded from the analysis. Therefore, the effects that we are estimating are limited to base communities that we could match. This maximizes internal validity, but it may come at a cost to external validity if the set of "matchable" communities is not representative. This is something that we leave for future consideration. In a few strata there are more base communities than non-base communities, and in a few the opposite is true. Because we are estimating the effect of the treatment on the treated, we weight the control communities in these cells so that the sum of weights for the control communities equals the number of treated communities in those cells. This ensures that the sample is weighted to the distribution of base communities over "matchable" strata. (Recall that the unit of analysis is the community.) These weights are used in all of the analyses below.

Households were selected at random within each of the 51 selected communities through a multistage process. Enumeration teams were supplied with maps of villages within communities from LISGIS. Three villages in each community were selected at random and the community level sample target was divided equally over the villages. Households in the selected villages were then

[12]For an updated discussion of these estimands and the primacy of internal validity in observational studies, see Imbens (2010).

listed and then selected at random from the list to fulfill the village sample target. The baseline target sample size within each community was 15. The survey contained another component that involved community-level group activities with survey respondents. These are being analyzed as part of the separate study. In the group-activity communities, higher sampling targets were set because the activities required more than 15 participants. For the most part, non-response was infrequent enough that sampling targets were met. In six communities, however, the non-response rate was higher than 33% (implying a sample size of 10 or less for communities with baseline sample targets).

Data were collected in December 2009 through January 2010 by an enumeration team managed by a private Liberian research firm, Subah Bellah Associates. We trained the enumerators extensively on the questionnaires, interview techniques, and human subjects protection principles. Because we could not follow all of the teams to all of the field sites, we monitored progress by having enumeration teams regularly report global positioning satellite coordinates to ensure that enumeration was taking place in the correct locales.

In addition to the OCHA data and the household surveys, we obtained community level outcome measures from surveys with local chiefs and from the 2008 Liberian census data file, provided by LISGIS.

5 Outcome measurement

The unit of analysis is the community, and so some care needs to be taken in constructing outcome measures. The reason is that while the communities that we have sampled are *fixed* relative to their proximity to deployments, individuals and households are not. People relocate, being pushed by insecurity and pulled by opportunity. To focus on outcomes at that adhere to individuals or households rather than communities may be fallacious. For example, suppose we wanted to study whether peacekeeping base proximity affects the likelihood of violent victimization. Suppose then that we simply ask individuals whether they had been victimized, and then compare the responses of those residing in deployment base versus non-base communities at the time of fieldwork. We may find that respondents in deployment base communities report victimization at a higher rate than in non-base communities. But this may be because they relocated to the base communities from elsewhere due to the security of the base community relative to their home community. Higher rates of victimization responses in deployment communities may reflect the fact that these places are *more* secure, not less. If one failed to take mobility into account, one might draw the wrong conclusion.

Keeping these perils in mind, we use a set of indicators that we think should capture community-level conditions as *reflected* in the behavior of individuals. These are listed below. With respect to security, we have (data sources in parentheses),

- Victimization by looting or physical attack, or fear of such victimization, in current community (household survey).[13]
- Settlement patterns, including permanent out-migration rates and in-migration of conflict victims (OCHA, household census, and household survey).
- Number of schools, health posts, and wells built either by the community itself or by NGOs (chief survey). The idea here is that the security bubble allows these projects to occur.

The security bubble hypothesis proposes that the first outcome should be substantially lower and the next two outcomes substantially higher in deployment base communities.

With respect to direct assistance, we have,

- Number of schools, health posts, and wells built post-war by UNMIL (chief survey).

The direct assistance hypothesis proposes that these should be substantially higher in the deployment base communities.

With respect to the ultimate economic and social revitalization outcomes, we have,

- Having a productive livelihood (household survey).
- Personal monthly income (household survey).

[13]We have demonstrated in previous work that incidences of victimization by armed groups in the post-deployment period were extremely rare (Mvukiyehe and Samii, 2010). Therefore, we only focus on isolated criminal violence.

- Levels of alimentary consumption in terms of meals eaten in the previous day (household survey).

- Higher order consumption in terms of investment in household repairs (household survey).

- Participation in community organizations such as a rotating credit group (*Susu*), farmers self-help group (*Koo*), producers' cooperative, or social club (household survey).

There are hypothesized to be positively associated with deployments, whether due to security of direct assistance.

The UNMIL deployment period began after the UN Security Council issued its mandate for the operation in September 2003. UNMIL did not complete full deployment into its base locations until the end of 2004, however. The deployment basing data that we have is rather coarse, only having been updated quarterly or semi-annually, and the time-based data that we have from our surveys is even coarser, being only yearly. Given this, we need some way to demarcate "before" and "after" deployment. Any choice may carry some biases: setting a cut-off too early risks attributing outcomes to deployment bases even though they took place prior to establishing the base, and vice versa for setting a cut-off too late. Based what we learned in the field, the subjects of our study could have received some information about base locations prior to their actually being physically established, although this would probably only have been at most a few weeks prior. We do not think that there is scope for outcomes to be affected by the anticipation of the establishment of a base many months prior to physical establishment, but there may have been anticipation effects within a month or so of deployment. Thus for our yearly data, the question is whether to designate 2004 as a pre-deployment or post-deployment year. Because there is some scope for anticipation and because our deployment timing data suggest that by mid-year 2004, 32 out of 45 bases had been established to full strength, we designate 2004 as the first year in the post-deployment period.

In the household survey data, two variables—income and meals—exhibited substantial amounts of missingness (18% and 16% of observations, respectively). It is not entirely clear from where this missingness arose—it is some combination of non-response, "don't know" responses, and enumerator error. Listwise deletion for missingness at those rates can result in substantial bias (Samii

2010), and so we used multiple imputation to fill those values. We used the MICE package for R (van Buuren and Groothuis-Oudshoorn, 2010). For the income variable, we used predictive mean matching to impute values of log(income + 1). (Since income is highly skew, the transform greatly improves the performance of the imputation algorithm and any other method for that matter that relies on linear specifications.) For the meals variable, we used predictive mean matching to impute untransformed values. For the analysis of these two outcomes, we use standard imputation combination rules (King et al, 2001), which are implemented with Stata 11's MI suite. The other household survey outcome variables exhibited very low rates of missingness (less than 5%), and so we simply omit observations with missing values on those outcomes to simplify the analysis.

6 Estimation and inference

When computing parameter estimator variances from the household survey data we take into account features of the study design as well as community-level clustering. Our design stratifies communities by the matching cells. This provides some efficiency gains. However, we also cluster units within communities to allow for arbitrary intra-community correlation between households. We assume no inter-community correlation conditional on the covariates that we include in our estimates. The stratification and clustering are taken into account in the variance estimates. All estimation was done in Stata version 11.

When working with outcomes the rely exclusively on the census or chief survey data, there is no sampling-based uncertainty about the outcome values. Judging "significance" can come merely from judging the *substantive* significance of the measured differences, as sampling-based standard error are undefined for such data. We could obtain p-values by applying the identifying assumption of random assignment within matching strata (Rosenbaum, 2002) and then using the appropriate permutation distributions. This is a controversial application, and one that we do not use at this time.[14]

For interpretability, we regularly convert logistic regression coefficients into exponentiated co-

[14]Other alternatives would be to reference some stochastic "super-population" or to use a Bayesian framework that assumes stochastic population parameters. This would require introducing more unverifiable assumptions into the analysis, and we choose not to make that move, at least not yet.

efficients, which provide odds ratios and relative risks. When, we do so, the standard errors are reported on this scale as well.[15] The null hypothesis is indicated by an exponentiated coefficient equalling one, positive effects are reflected in coefficients larger than one, and negative effects in coefficients between zero and one. Because confidence intervals are asymmetric on the exponentiated scale, we present p-value for ready assessment of statistical significance.

7 Descriptive statistics

This section will contain some descriptive statistics, including summary of balance on community covariates and a characterization of the demographic characteristics of the household sample. We will also include a map of respondent locations and deployment base locations. These tables and figures have not yet been constructed. We apologize for that.

[15]Stata computes odds ratio standard errors via the delta method.

8 Results

8.1 Security bubble hypothesis

Recent victimization by looting or physical attack, or fear of such victimization

Estimates from a design-adjusted logistic regression are presented in Table 1. Surprisingly, we find that PKO bases are associated with 71% *higher* odds of experiences or fears of victimization. This is contrary to the security bubble hypothesis.

Settlement patterns

Estimates are presented in Table 2. The key results are parts V and VII of the table. Base communities have a negligibly lower average out-migration rate, and the average rate of in-migration by conflict victims is slightly *higher* in non-base communities. The results do not provide support for the security bubble hypothesis.

Community and NGO social infrastructure projects

Table 3 shows results of linear regressions of the number community and NGO social infrastructure projects, as reported in the chief surveys. As discussed above, no standard errors are computed, as this is population level data. The significance of the results is to be determined by the substantive significance of the estimates. The results show that bases are not regularly associated with more community and NGO infrastructure projects. In fact, the most striking result is that NGO facilitated projects appear to be consistently *less* frequent in base communities. At least for one type of infrastructure, wells, the difference is quite substantial.

8.2 Direct assistance hypothesis

We test the direct assistance hypothesis by estimating covariate adjusted differences in the frequency of UNMIL-facilitated social infrastructure projects in base and non-base communities. The estimates show that UNMIL-facilitated social infrastructure projects were quite rare overall, and there is little in the way of differences in base and non-base communities in this regard.

8.3 Economic and social revitalization

Having a productive livelihood

Results are presented in Table 5. The outcome is a four-category typology of livelihoods:

1. no job or only causal labor
2. agriculture and husbandry or regular but unskilled labor
3. skilled labor, small business, or soldier/police
4. professional or student

We set the baseline category to the second category; substantively, this is sensible since this can be considered the "default" livelihood for the vast majority of Liberians. Interestingly, we find that deployment base communities host a substantially larger share of skilled labor, small business, and soldier/police livelihoods: the likelihood of having such a livelihood versus the baseline outcome is 73% higher in deployment base communities. Unpacking this at a finer grain of detail, we find that the differences are across a number of occupational types, including carpentry, small business owners, soldier/police, and tailoring. This is demonstrated in Table 6, which shows the full breakdown of job types across the base and non-base communities. The fact that so many commercial activities are represented in greater proportion in base communities may be indicative of a local market stimulation effect. That is, it does not seem that the differences are due, say, to the mere increased presence of police or soldiers accompanying the peacekeepers. The only other occupation type that exhibits substantial variation is for students: they are much more prominent as a share of the population in non-base communities. Of course, local custom in countries like Liberia is for unemployed but nonetheless ambitious young adults to refer to themselves as "students" as they are awaiting an employment opportunity. We do not discount that possibility, which would in fact provide more indication of differences in the level of labor market vibrancy across the two communities, with base communities having more vibrant markets.

Personal monthly income

Design-adjusted OLS and quantile regressions of log (income + 1) are shown in Table 7. Deployment bases are associated with substantially higher income. In fact, the OLS estimates show that the average income more than *double* for those in base communities. The quantile regressions show differences in the 25th percentile, median, and 75th percentile of the log-income distributions. Quantile regressions are useful, because the highly skew and heteroskedastic nature of income, even after the log transformation, means that OLS can mask important patters. The quantile regressions show that the income boost is strongest for those at the lower end of the income spectrum, increasing the 25th percentile of the income distribution almost six-fold, and then increasing median and 75th percentile incomes by factors of about 2 and 1.5, respectively. This is a tremendous income boost, and is consistent with our findings about labor market stimulation above.

Higher order and alimentary consumption

Results for consumption levels are in Table 8. On average, we see that both home repairs and number of meals eaten is actually lower in base communities, however the estimates are imprecise and do not provide compelling evidence of meaningful effects on consumption levels.

Participation in community organizations

Results are presented in Table 9. We see that on average, deployment base communities exhibit higher rates of participation credit and producers organizations, however the estimates are quite imprecise and thus do not present compelling evidence of positive effects (p-values of .24 and .41, respectively). The evidence for *negative* effects of social club participation is rather strong, however. Residents of base communities are 44% less likely to participate in social clubs (p-value 0.06). This is may be indicative of some sort of social dysfunction in deployment base communities.

9 Initial Conclusions

The results thus far paint a picture that diverges from the expectations of the theory outlined above, at least in terms of mechanisms, but does demonstrate postive impacts on market vitality. This presents a puzzle. What might be going on? There are a few possibilities. It may be that we are not capturing forms of direct assistance that are going on, for example procurement by the operation and routine expenditure by those associated with the deployment. In addition, there are intriguing possibilities that may be associated with the negative relationship between NGO activities and peacekeeping deployments. With respect to security provision and the rehabilitation of community social life, could it be that NGOs focus their work where peacekeepers are *not* present and that in fact the assistance provided by NGOs *outperforms* that which is provided by peacekeepers on these fronts? This possibility raises questions about the cross-national findings about the effectiveness of peacekeeping: are NGO activities a confounding factor? We think this is a fruitful area for further research. Finally, it may very well be that we are measuring something close to the real non-effects peacekeeping at the micro-level in terms of security provision. As discussed in the introduction, there is no contradiction between the lack of micro-level security effects and the finding that peacekeeping is associated with more durable peace—the mechanism may very well be at the macro-level.

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Tables

Table 1: **Logistic regression of crime-related victimization**

Variable	Odds ratio	(OR s.e.)	p-value
PKO base	1.71	0.21	0.00
Kru region	0.74	0.13	0.10
Coastal region	0.72	0.15	0.14
Distance to road network	0.05	0.05	0.01
2004 Households	1.00	0.00	0.00
2004 Schools per 100 households	0.99	0.00	0.00
2004 Health posts per 100 households	0.93	0.06	0.31
Medium level conflict	2.21	1.17	0.15
High level conflict	2.24	1.21	0.15

N = 846, Global F-test p-value, 0.00

Standard errors account for matching strata and community level clustering.

Table 2: **Settlement patterns**

	PKO base communities	Non-base communities
I. Average number of households per clan, 2004 (OCHA)	2,198	1,602
II. Average number of households per clan, 2008 (census)	1,762	1,358
III. 2009-2010 household survey results		
a. Prewar inhabitants who never left	53%	44%
	(4)	(5)
b. Prewar inhabitant displacees/migrants who returned pre-deployments	1%	1%
	(<1)	(1)
c. Displacees/migrants from other clans who arrived pre-deployments	13%	15%
	(2)	(2)
d. Prewar inhabitant displacees/migrants who returned post-deployments	4%	7%
	(1)	(2)
e. Displacees/migrants from other clans who arrived post-deployments, nonvictims	14%	15%
	(2)	(3)
f. Displacees/migrants from other clans who arrived post-deployments, victims	16%	18%
	(2)	(2)
IV. Estimated household distribution per clan		
a. Prewar inhabitants who never left	925	593
b. Prewar inhabitant displacees/migrants who returned pre-deployments	16	18
c. Displacees/migrants from other clans who arrived pre-deployments	228	204
d. Prewar inhabitant displacees/migrants who returned post-deployments	72	94
e. Displacees/migrants from other clans who arrived post-deployments, nonvictims	245	210
f. Displacees/migrants from other clans who arrived post-deployments, victims	275	239
V. Estimated percentage of 2004 households who outmigrated and did not return by 2010[a]	54%	56%
VI. Estimated ratio of post-deployment in-migrating households to 2004 households, non-victims[b]	0.11	0.13
VII. Estimated ratio of post-deployment in-migrating households to 2004 households, non-victims[c]	0.13	0.15

[a]Subtracts IV.a, IV.b, and IV.d from I and then divides by I.

[b]Divides IV.e by I.

[c]Divides IV.f by I.

Percentage point standard errors shown in parentheses.

Table 3: **Linear regressions of infrastructure projects by communities and NGOs**

Variable	Community wells	NGO wells	Community primary schools	NGO primary schools	Community health posts	NGO health posts
PKO base	0.06	-3.27	0.74	-0.53	-0.77	-0.59
Kru region	0.73	-6.84	0.32	-1.85	-0.89	-0.04
Coastal region	0.80	0.71	-0.10	-0.73	-0.71	-0.26
Distance to road network	5.11	-25.38	0.99	-7.01	-1.03	0.35
2004 Households	0.00	0.00	0.00	0.00	0.00	0.00
2004 Schools per 100 households	-0.04	-0.04	0.00	0.01	0.01	-0.01
2004 Health posts per 100 households	-0.28	1.04	0.08	0.41	-0.61	0.01
Medium level conflict	0.32	0.99	1.24	0.19	0.51	0.64
High level conflict	0.90	1.58	0.68	1.10	-1.08	0.43
Constant	-0.18	10.27	-0.86	1.85	3.04	0.19

N= 48

Table 4: **Linear regressions of infrastructure projects by the UN**

Variable	UN wells	UN primary schools	UN health posts
PKO base	-0.54	-0.09	0.02
Kru region	0.65	0.02	0.03
Coastal region	-0.93	-0.42	0.07
Distance to road network	-4.67	0.05	0.70
2004 Households	0.00	0.00	0.00
2004 Schools per 100 households	-0.01	0.00	0.00
2004 Health posts per 100 households	0.17	0.04	-0.01
Medium level conflict	1.61	0.43	-0.01
High level conflict	1.28	0.45	-0.08
Constant	-0.59	-0.10	-0.12

N=48

Table 5: **Multinomial logistic regression of livelihood types**

Variable		**No job vs. agriculture**	**Skilled labor vs. agriculture**	**Professional vs. agriculture**
PKO base	*RRR*	1.08	1.73	0.58
	(s.e.)	(0.38)	(0.38)	(0.15)
	p-value	0.82	0.02	0.04
Kru region	*RRR*	1.62	1.28	0.84
	(s.e.)	(1.05)	(0.37)	(0.25)
	p-value	0.46	0.40	0.56
Coastal region	*RRR*	0.69	0.84	0.64
	(s.e.)	(0.58)	(0.30)	(0.28)
	p-value	0.66	0.63	0.32
Distance to road network	*RRR*	0.32	2.14	0.01
	(s.e.)	(1.27)	(2.91)	(0.04)
	p-value	0.78	0.58	0.09
2004 Households	*RRR*	1.00	1.00	1.00
	(s.e.)	(0.00)	(0.00)	(0.00)
	p-value	0.04	0.00	0.23
2004 Schools per 100 households	*RRR*	1.00	0.99	1.02
	(s.e.)	(0.01)	(0.02)	(0.00)
	p-value	0.89	0.61	0.00
2004 Health posts per 100 households	*RRR*	0.72	0.91	0.95
	(s.e.)	(0.14)	(0.10)	(0.09)
	p-value	0.09	0.40	0.62
Medium level conflict	*RRR*	0.78	1.43	1.28
	(s.e.)	(0.90)	(0.96)	(0.60)
	p-value	0.83	0.59	0.61
High level conflict	*RRR*	0.97	1.09	1.09
	(s.e.)	(1.26)	(0.81)	(0.65)
	p-value	0.98	0.90	0.88

N = 842

Global F-test p-value, 0.02

"RRR" stands for relative risk ratio.

Standard errors account for matching strata and community level clustering.

Table 6: **Distribution of livelihood types across base and non-base communities**

Category	**Livelihood type**	**PKO base communities**	**Non-base communities**
4	Civil servant	1.8%	0.7%
	Doctor or nurse	0.8%	0.9%
	Student	6.7%	14.4%
	Teacher	2.7%	3.6%
3	Carpenter	1.6%	0.7%
	Craftsman	0.2%	0.6%
	Taxi or driver	1.3%	1.1%
	Small business	13.7%	8.2%
	Soldier or police	1.0%	0.3%
	Tailor	0.9%	0.0%
	Technician	0.3%	0.9%
2	Husbandry	0.0%	0.6%
	Crop farming	51.9%	56.8%
	Factory	2.6%	0.3%
	Fisherman	1.0%	0.0%
	Petty trading	6.0%	3.6%
	Rubber tapping	1.2%	0.9%
1	Casual labor	0.7%	0.5%
	Without occupation	5.8%	5.7%
	Porter	0.0%	0.2%

N = 842

F, 2.00; p-value, 0.03.

Table 7: **OLS and quantile regressions of log(income+1)**

Variable		**OLS**	**25th Percentile**[a]	**Median**[a]	**75th Percentile**[a]
PKO base	*Coef.*	1.00	1.78	0.71	0.42
	(s.e.)	(0.31)	(0.39)	(0.20)	(0.25)
	p-value	0.01	0.00	0.00	0.10
Kru region	*Coef.*	1.98	4.68	1.33	0.78
	(s.e.)	(0.62)	(1.15)	(0.26)	(0.39)
	p-value	0.00	0.00	0.00	0.05
Coastal region	*Coef.*	-0.61	-1.19	-0.31	0.03
	(s.e.)	(0.45)	(0.64)	(0.25)	(0.37)
	p-value	0.19	0.08	0.22	0.92
Distance to road network	*Coef.*	9.60	14.14	7.63	5.45
	(s.e.)	(2.68)	(4.03)	(1.60)	(2.16)
	p-value	0.00	0.00	0.00	0.01
2004 Households	*Coef.*	0.00	0.00	0.00	0.00
	(s.e.)	(0.00)	(0.00)	(0.00)	(0.00)
	p-value	0.41	0.33	0.17	0.88
2004 Schools per 100 households	*Coef.*	-0.02	-0.05	-0.01	-0.01
	(s.e.)	(0.02)	(0.02)	(0.01)	(0.01)
	p-value	0.45	0.01	0.49	0.20
2004 Health posts per 100 households	*Coef.*	-0.38	-0.68	-0.24	-0.19
	(s.e.)	(0.15)	(0.21)	(0.07)	(0.10)
	p-value	0.02	0.00	0.00	0.06
Medium level conflict	*Coef.*	-0.65	-0.01	0.00	-0.10
	(s.e.)	(0.82)	(0.83)	(0.47)	(0.56)
	p-value	0.44	0.99	0.99	0.86
High level conflict	*Coef.*	-1.46	-1.61	-0.52	-0.68
	(s.e.)	(0.93)	(1.38)	(0.47)	(0.66)
	p-value	0.13	0.26	0.28	0.30
Constant	*Coef.*	5.16	1.95	5.70	7.47
	(s.e.)	(1.00)	(1.63)	(0.48)	(0.61)
	p-value	0.00	0.25	0.00	0.00
Each regression is estimated from 5 imputation-completed datasets with 850 observations each.					
Global F-test p-value		0.00	0.00	0.00	0.00

[a]Standard errors and t-statistics for the quantile regression estimates are an approximation. Software limitations prevented us from directly estimating bootstrapped standard errors on the weighted data. Thus, we multiplied the analytic iid standard errors by 1.1, which equals the average ratio of bootstrapped to analytic iid standard errors for the unweighted regressions; p-values were computed using average multiple imputation degrees of freedom. For more details, contact the authors.

Table 8: **Logistic and ordered logistic regression of consumption**

Variable		**Logistic regr. for household repairs**	**Ordered logistic regr. for meals**
PKO base	*OR*	0.80	0.84
	(s.e.)	(0.13)	(0.17)
	p-value	0.179	0.39
Kru region	*OR*	1.85	5.04
	(s.e.)	(0.59)	(2.18)
	p-value	0.062	0.00
Coastal region	*OR*	0.61	0.74
	(s.e.)	(0.15)	(0.22)
	p-value	0.057	0.32
Distance to road network	*OR*	1.98	78.62[a]
	(s.e.)	(2.70)	(151.15)
	p-value	0.618	0.03
2004 Households	*OR*	1.00	1.00
	(s.e.)	(0.00)	(0.00)
	p-value	0.026	0.01
2004 Schools per 100 households	*OR*	0.98	1.01
	(s.e.)	(0.01)	(0.01)
	p-value	0.008	0.32
2004 Health posts per 100 households	*OR*	1.03	1.11
	(s.e.)	(0.08)	(0.10)
	p-value	0.679	0.23
Medium level conflict	*OR*	1.35	0.65
	(s.e.)	(0.64)	(0.56)
	p-value	0.53	0.62
High level conflict	*OR*	1.08	1.00
	(s.e.)	(0.58)	(0.91)
	p-value	0.89	1.00
N		817	850 on 5 imputation completed datasets
Global F-test p-value,		0.01	0.00

Standard errors account for matching strata and community level clustering.

[a]The range of this variable is .00025 to .27450, which explains the bizarre coefficient. It is not due to separation. Running the regression with a standardized version of the variable does not change any of the other results.

Table 9: **Logistic regression of community participation outcomes**

Variable		**Rotating credit**	**Social clubs**	**Producers coop.**
PKO base	*OR*	1.23	0.66	1.14
	(s.e.)	(0.21)	(0.14)	(0.18)
	p-value	0.24	0.06	0.41
Kru region	*OR*	1.33	1.73	1.17
	(s.e.)	(0.35)	(0.53)	(0.33)
	p-value	0.29	0.08	0.58
Coastal region	*OR*	0.56	0.59	1.15
	(s.e.)	(0.17)	(0.17)	(0.41)
	p-value	0.06	0.08	0.69
Distance to road network	*OR*	94.18[a]	2.84	8.90
	(s.e.)	(210.18)	(5.71)	(13.23)
	p-value	0.05	0.61	0.15
2004 Households	*OR*	1.00	1.00	1.00
	(s.e.)	(0.00)	(0.00)	(0.00)
	p-value	0.53	0.65	0.91
2004 Schools per 100 households	*OR*	1.00	1.00	0.99
	(s.e.)	(0.01)	(0.01)	(0.02)
	p-value	0.35	0.61	0.61
2004 Health posts per 100 households	*OR*	0.88	0.93	0.99
	(s.e.)	(0.07)	(0.06)	(0.12)
	p-value	0.15	0.31	0.92
Medium level conflict	*OR*	2.13	0.87	0.66
	(s.e.)	(1.74)	(0.29)	(0.20)
	p-value	0.36	0.68	0.17
High level conflict	*OR*	1.45	0.74	0.67
	(s.e.)	(1.28)	(0.32)	(0.25)
	p-value	0.68	0.50	0.29
N		816	844	840
Global F-test p-value		0.01	0.04	0.61

Standard errors account for matching strata and community level clustering.

[a]The range of this variable is .00025 to .27450, which explains the bizarre coefficient. It is not due to separation. Running the regression with a standardized version of the variable does not change any of the other results.