Delivering Justice to the Poor: Theory and Experimental Evidence from Liberia*

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Abstract

Can progressive, statutory legal reform improve the lives of the poor in places where formal legal institutions have limited reach? We develop and test a simple model of forum choice that highlights the tradeoff the poor and socially disadvantaged face between the repressive aspects of customary law and the formal system's high costs and punitive approach to justice. Using new survey data on over 4,500 legal disputes in rural Liberia we find that, consistent with our model, plaintiffs facing a disadvantageous pairing under the custom—e.g., women suing men—are more likely to choose the formal system and are relatively happier when they do. We apply these insights to a randomized trial of a legal aid program designed to overcome this tradeoff, by offering *pro bono* mediation by community paralegals trained in the formal law. Plaintiffs offered legal aid are significantly more satisfied with case outcomes, pay fewer bribes, and report large material gains in terms of household and child food-security. Furthermore, both demand for and impacts of the program are greater for plaintiffs facing poor odds in the customary system. Our results suggest that there are large socioeconomic gains to be had from improving access to the formal law, by making its institutions more competitive with the organizational forms of the custom.

Keywords: conflict, forum shopping, legal institutions, Liberia, rule of law
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1 Introduction

A broad body of evidence developed in recent years suggests that the quality of formal legal institutions is a primary driver of economic growth (Acemoglu et al., 2001; Rodrik et al., 2004), and that extending access to, for example, formal property rights (Besley, 1995; Field, 2005, 2007; Galiani and Schargrodsky, 2010), credit market institutions (Jappelli et al., 2005; Pinheiro and Cabral, 2001) and the judiciary (Chemin, 2009a,b, 2010) can have significant welfare benefits. Yet most people in the developing world have little, if any, contact with such formal legal institutions—their marriages, property ownership, debts and even crimes are instead governed by informal customs and traditional leaders, i.e., customary law (CLEP, 2008; de Soto, 2000; Harper, 2011). Furthermore, when formal and customary law come into conflict, the poor often choose to seek justice under customary rather than formal law (Isser et al., 2009; Moult, 2009; Sharma and Sen, 2008).

This forum shopping behavior presents a *prima facie* challenge to models of legal reform that either ignore customary law altogether or assume plaintiffs will seek out formal justice if impediments are removed (Golub, 2003). It implies that formal legal reforms intended to raise the status of women, protect children, or guarantee the economic rights of ethnic minorities will oftentimes be dead letters—i.e., *de jure* rules with little effect on *de facto* outcomes.

In this paper, we present a simple framework for understanding the forum shopping decisions of plaintiffs in rural Liberia. The crux of the model is a trade-off between the more progressive content of the formal law—protecting the rights of women, minorities, and the socially disadvantaged—and the lower costs and less punitive remedies provided by the customary system.

Using new household survey data on over 4,500 disputes in rural Liberia, we show that forum-shopping behavior conforms to the basic theoretical predictions. The relative social position of plaintiffs and defendants predicts both where disputes are taken, and the parties' satisfaction with the outcomes. For instance, women suing women are very unlikely to go the formal system, while women suing men are much more likely to do so. Male defendants who are taken out of the customary system, where they may be privileged, report deep dissatisfaction with formal verdicts. Opposite patterns hold with male plaintiffs and female defendants.

These results suggest potential gains from the provision of low-cost, remedial justice that incorporates the progressive features of the formal law. In the second half of the paper we present the results of a randomized controlled trial of a legal empowerment intervention run by The Carter Center in Liberia and designed around the basic principles highlighted in the model and observational data analysis.

The NGO program provided *pro bono* mediation and advocacy services by means of community paralegals trained in the formal law. Trial participants were recruited through public information meetings about the rule of law and dispute resolution conducted in 76 villages across four of Liberia's fifteen counties. The sample was drawn from individuals who came forward to request

free legal assistance for legal problems or disputes of any type, including debt disputes, land disputes, criminal acts of which they were either accused or victims, and a host of family and custody-related disputes. Half the participants were randomly selected to receive three months of assistance from community paralegals trained in mediation and legal advocacy.[1]

Overall, we find significant impacts on legal and socioeconomic outcomes on the study population. Legal aid yields a large, statistically significant increase in the proportion of clients who report that their case outcome was fair, who are satisfied with the result and feel it left them better off, and who report a good relationship with the other party after the resolution of the case. The program also produced a 10 percentage-point reduction in the share of clients who paid a bribe during the treatment period.

Our findings relate to a growing literature examining the design of development programs in fragile and post-conflict environments.[2] For instance, the World Bank and other development agencies have invested heavily in "community-driven development and reconstruction" (CDD/R) programs which aim aim to increase social cohesion and reduce violence by replacing indigenous local institutions with *de novo* organizational forms that are more democratic and representative. A growing body of evidence from large-scale randomized controlled trials of this approach have shown mixed and often disappointing results in achieving lasting institutional change (Beath et al., 2012; Casey et al., 2012; Coleman and Lopez, 2010; Fearon et al., 2009).

Other efforts have focused on reforming rather than replacing customary institutions, for instance through curriculum interventions. Blattman et al. (2013) find a significant reduction in unresolved land disputes and property destruction due to community-level training in alternative dispute resolution in Liberia, but mostly null effects on economic behavior, community-level violence, and norms about legal dispute resolution. Staub and Pearlman (2009) and Paluck (2009) find positive effects from curricular interventions on survey measures of social cohesion in Rwanda, but provide no evidence of changes in behavior or real-world outcomes.

Relative to these other common programmatic models among development organizations, the legal empowerment intervention examined here seeks to "pull" rather than "push" social change by expanding the scope of meaningful institutional choice faced by rural Liberians. This emphasis on facilitating choice and competition between institutions differs from programs attempting to either reform or replace customary institutions.

In the remainder of the paper, we lay out our conceptual framework for thinking about the trade-offs faced by plaintiffs who have suffered some legal harm in rural Liberia. Section 2 provides background on customary and formal law in Liberia. Section 3 lays out a formal game-theoretic model of the interactions between plaintiffs, defendants, and customary and formal judges. The

[1] Treatment villages were themselves randomly assigned to participate in the intervention, and will be compared to randomly assigned control villages as part of a broader study beyond the scope of this paper. The comparison will provide greater insight into the longer-term, community-level impacts of this type of intervention.

[2] See King et al. (2010) and Mansuri and Rao (2013) for systematic reviews.

empirical analysis is then divided into two parts. First, in Section 4 we use baseline, observational data on 4,500 disputes to test several predictions of the model. Second, in Section 5, we present the results of a randomized impact evaluation of a legal aid program designed to overcome the trade-offs discussed in the model. Section 6 concludes.

2 Context

Liberia has one of the poorest populations in the world, ranking 182 out of 187 countries in the 2011 Human Development Index. Decades of unrest and civil war have led to "an almost unanimous distrust of Liberia's courts, and a corresponding collapse of the rule of law" (ILAC, 2003). Formal courts are hard to access, expensive, and slow; few justice practitioners are legally literate; and the laws and procedures of the formal system are alien to most Liberians (Isser et al., 2009).

In contrast, the customary system is both accessible and culturally acceptable, but operates under patriarchal and communal norms rather than the notions of individual rights enshrined in Liberian statutory law (ICG, 2006). Recent anthropological research documents a range of customary practices that violate international standards, such as *sassywood* (trial by ordeal), as well as local laws and practices that run contrary to generally accepted notions of women's rights and the rights of vulnerable groups (Isser et al., 2009; Pajibo, 2008).

Since the end of the civil war in 2003, the formal law is on the march. The national government has passed progressive legislation reforming the content of the formal law (e.g., banning trial by ordeal, criminalizing statutory rape, etc.) and pushed to extend the reach of formal courts into areas previously under customary jurisdiction. Rapid changes in statutory law and in the allocation of judicial and administrative responsibilities have created widespread confusion about the substance of the law, the proper passage of appeal, and the rights and responsibilities of different actors in the justice system (Isser et al., 2009).

Liberians thus have to negotiate a confusing legally dualistic system that offers starkly different choices in terms of the costs and quality of justice provided. To understand these choices further, we turn briefly to the history of legal dualism in Liberia, and its contemporary manifestation.

2.1 Legal dualism

The history of customary law and legal dualism in Africa is well-documented in anthropological scholarship, with scholars largely agreed that these laws were formed out of struggle between fiercely competing groups. In his influential work *Citizen and Subject*, Mahmood Mamdani describes the judicial system in colonial Africa as a deeply bifurcated institution, and equates legal dualism with a "deracialized legal apartheid" that restricts formal rights to a select few while relegating the rural poor to the "decentralized despotism" of customary rule.

While Mamdani's stylized view is starker than most (Berry, 1975; Chanock, 1989; Moore, 2011), it resonates peculiarly with Liberia's justice system, which though outside the orbit of British colonialism, displayed many of the hallmarks of discriminatory segregation:

> At Liberia's founding, the state established the dual system to ensure that statutory law would govern 'civilised' people—Americo-Liberians and missionaries—while customary law would regulate 'natives'. The non-Christian, indigenous Africans, who were considered 'uncivilised', could not use the statutory system, and chiefs could not adjudicate cases to which a 'civilised' person was party. State-sponsored customary law was the compromise between the government's attempt to coopt the traditional sphere and villages' desire to maintain their autonomy. Although the constitution, statutory laws and common law of the formal legal system now govern all Liberians, the archaic Rules and Regulations Governing the Hinterland still refer to the adjudication of cases for 'civilized people' and 'natives' (ICG, 2006, p. 7).

At present, Liberian statutory law applies, in principle, to all Liberians. The statutory system comprises, for the most part, a vertical hierarchy of statutory courts, including Justice of the Peace (JP) courts, magistrates' courts, circuit courts, and the Supreme Court. They are supported in their workings by public attorneys, specialized institutions such as land commissioners to arbitrate land matters, and the police. Yet through the 'Hinterland Regulations', statutory law explicitly recognizes the dual nature of the legal system in the form of a parallel, idiosyncratic customary system administered by local chiefs. The most direct provider of customary justice is the town chief, who is the *de jure* leader of the community. Outside the village, the town chief is the lowest rung in a vertical hierarchy of chiefs of increasing degrees of formal recognition: the general town chief, the zone chief, the clan chief, and finally the paramount chief. Chieftaincy is recognized and receives some support from the state, and is regulated by state-appointed district commissioners and county superintendents.

Although the two systems are parallel in principle, the boundary between them is complex and contentious. Isser et al. (2009) stress the role of individual agency in navigating the dual system, noting that "cases may jump from the customary chain into the formal one and vice versa at nearly any point, due to the assertion of authority by a member of one or the other chain, or by choice of one of the litigants." This conceptualization differs notably from recent theoretical work on the issue (e.g. Aldashev et al., 2012) that sees the relationship between the two systems as a unidirectional hierarchy, with the formal system on top acting as the *de facto* court of appeal. Our survey data shows, for instance, that of disputants who visited more than one forum, the proportion that first chose a customary forum and then a formal is similar (in fact slightly less) than the proportion that chose a formal forum first and then reverted to the customary. This suggests both that there is considerable leeway to shop between forums, and that either forum can be treated as a 'court of appeal'.

To further complicate matters, in reality there exist "a broad range of actors who have no legally or socially recognized roles in formal, state-backed-customary, or even community-based-customary justice institutions become involved in, and are perceived to be able and likely to influence, the resolution of cases ranging from the most trivial to the most serious" (Isser et al., 2009, p. 23-24). Within the village, such actors include village elders, who advise and regulate the town chief's decisions, as well as family heads, women's leaders, youth leaders, secret society leaders, religious leaders (pastors and *imams*), and heads of social institutions (savings clubs, markets, unions, etc.). Outside the village, these include national legislators, deputy ministers, immigration officers, city mayors, diplomatic bodyguards, and the police—the latter serving both as "gatekeepers" for decisions over forum choice, as well as mediators or adjudicators of the dispute itself.

To summarize, while in Liberia the system is dualistic both *de jure* and to a large extent *de facto*, it is porous and there is no strict hierarchy between customary and formal, providing genuine opportunity to shop between forums. We now turn to the determinants of this forum choice.

2.2 Tradeoffs

We contend that disputants choose between the customary and formal system based on rational tradeoffs salient to their situation. While the customary system is preoccupied with reconciliation and social harmony, in contrast, magistrates—backed by police and prisons—are better equipped to mete out punishment, which provides little or no material or social gain to plaintiffs. Liberians consistently complain that the formal system is rarely capable of enforcing redressive measures, instead providing punishment without compensation. A punishment meted out by the formal system is "regarded as a source of added forms of victimization even of those it determines to be in the right and innocent (through the battery of fees that are imposed in the process), and as the source of accentuated conflict that is ultimately detrimental to all—victims, perpetrators and the community at large" (Isser et al., 2009, p. 49).

In addition, Liberians experience "a bewildering array of fees associated with the formal system, including registration fees, gas money for police investigators, requirements that victims pay the cost of food for the detained accused, lawyers' fees, bribes, and indirect costs such as money for transportation and time spent away from livelihoods" (Isser et al., 2009, p. 3). Perhaps most importantly, the laws and procedures of the formal system are alien to most Liberians. Formal legal literacy is low, even more so because the formal law has rapidly changed in the years after the civil war (ICG, 2006). Finally, local cultural norms emphasize that community members should not seek recourse from outside institutions, and those who approach formal institutions face the threat of social sanction and possible expulsion from their own communities.

Yet while the customary legal system comes with many benefits, it remains largely unencumbered by the set of egalitarian norms and rights enshrined in the formal law. Thus customary

law may recognize the right of a husband to beat or demand sexual intercourse from his spouse, limit land rights for widows, ethnic minorities, or persons born outside the village, and so forth. For instance, among the Kwa-speaking people in Liberia, sharing a kola nut is a popular form of dispute resolution based on forgiveness, where the perpetrator offers provide kola nuts, cane juice (a local alcoholic drink) or a chicken or goat to the aggrieved party. The aggrieved party is under tremendous social pressure to accept the offering "[i]n most instances... as a result of social coercion" (Pajibo, 2008, p. 16).

Furthermore, the system "utilizes a range of practices that violate international standards, most prominently, trial by ordeal and practices that violate women's rights" (Isser et al., 2009, p. 3). Though trial by ordeal (*sassywood*) is illegal, most ethnic groups use it to settle disputes involving property theft, witchcraft/sorcery, or death. The practice is primarily used to identify the perpetrator of a crime, but in itself constitutes "cruel and inhuman punishment". In a typical case:

> The alleged perpetrator is made to imbibe a mixture or brew made from indigenous plants. If he or she regurgitates the brew, this constitutes a not guilty verdict. Failure to do so demonstrates guilt and the person will be banished from the village (in the case of murder), scorned, shamed and (in the case of theft of property) made to make restitution. Another sassywood method involves the use of a red-hot metal that is brought into contact with the alleged perpetrator's person (in most cases the leg). If the alleged perpetrator withdraws from the heat, he or she is ruled guilty. If you were not guilty, then you would not get burned (Pajibo, 2008, p. 17).

In the case of conflict between groups, the party that initiated the conflict is typically held responsible, and to prevent future conflict "is made to give away a beautiful unmarried woman from their group to the other group, to be married to the chief warrior of the second group"(Pajibo, 2008, p. 17). Cases of rape are resolved with similar pragmatism: "In most rape cases, the fine may include a he-goat, a black rooster, 50 pounds of rice and three gallons of palm oil." Among the Bassa, the second-largest ethnic group in Liberia, "the fine is only a white chicken and a white kola nut." Among the Mende, "[i]n some instances, the rapist is made to marry his victim" (Pajibo, 2008, pp. 20, 22).

The Liberian customary system exhibits many of the repressive features catalogued in other contexts (see e.g. Wojkowska, 2006). Customary systems tend to emphasize peace and social order, often at the expense of violating individual rights and freedoms. Furthermore, customary justice providers are "often selected on the basis of who they know or who they are related to" and "may abuse their power to benefit those who they know or who are able to pay bribes" (Wojkowska, 2006, p. 21). The accused may not always have the chance to be heard or adequately represented, and unequal power relations may simply be reinforced given the system's vulnerability to elite capture (DIHR, 2010) and its "biases towards patriarchy" (Pajibo, 2008, p. 17). For example,

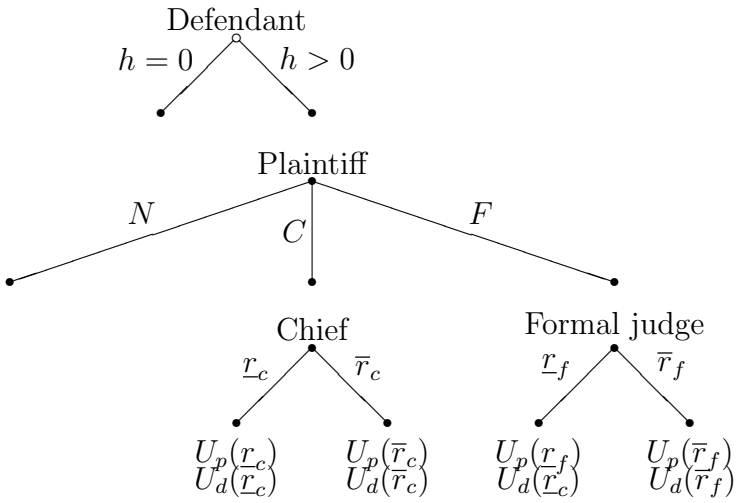

Figure 1: Game Tree

Moult (2009) notes that women in South Africa take cases to traditional authorities in part because of "power relations within their community and economic dependence on their husbands", and questions "whether informal systems can or will stop forms of violence against women" (Moult, 2009, pp. 9, 19).

The above discussion yields two main stylized facts, which we take as the starting points for the formal modeling exercise in the next section:

1. Despite offering recourse to more progressive, rights-based law, the formal legal system is more costly to access, and less able to provide redress to aggrieved parties or victims of crime. The punitive justice it metes out is at odds with the local emphasis on social reconciliation and harmony.

2. The customary system, though cheaper and more culturally acceptable, is highly susceptible to elite capture and operates under patriarchal, gerontocratic and communal norms that often violate the individual rights of the socially disadvantaged.

The community paralegal intervention studied in this paper attempts to address these tradeoffs, by providing an alternative delivery mechanism for the formal law that bypasses the institutional constraints of the formal system, offering lower-cost access and emphasizing reconciliation and redress over punishment.

3 A model of forum shopping

To motivate our theoretical setup, we distinguish two separate approaches towards conceptions of legal pluralism in academic and policy debates. The first view maintains a strict hierarchy between the systems, seeing the custom as a distinctly second-best alternative to the formal law.

Initiatives promoting 'access to justice' often equate justice with formal law, and implicitly assume that agents make a constrained choice between forums, where financial costs and ignorance of the law are the most commonly cited obstacles to the formal system. This approach is what Golub (2003) terms 'rule of law orthodoxy' in development thinking. This orthodox approach focuses on the promulgation of new laws and reforming formal institutions, often taking for granted the supremacy of the judiciary and central role of trained lawyers.

A second view, with a long pedigree among Western scholars of African law (Adinkrah, 1991; Allott, 1968), contrasts the punitive, "zero-sum, winner-take-all model of justice" of formal courts with an often romanticized view of customary law in which "a high value is placed on reconciliation and everything is done to avoid the severance of social relationships" (Stevens, 2000).

We organize these competing thoughts in a simple economic model of forum shopping that allows for individual agency by plaintiffs in choosing forums, as is implicit in policy debates about access to justice, while also allowing for the positive features of customary justice stressed by many legal anthropologists. In the model, individuals trade off the social bias of the customary system with its relative efficiency and 'remedial' approach towards justice. This incorporates basic insights from the law and economics literature (Aldashev et al., 2012; Becker, 1968; Polinsky and Shavell, 2007) as well as contemporary anthropological work (Isser et al., 2009).

The model developed below attempts to capture the tradeoffs listed in Section 2—between the formal system's punitive approach and high costs, and the customary system's bias against the socially disadvantaged. The small number of recent papers examining the workings of legal dualism in a developing-country context have focused primarily on the strategic actions of judges rather than disputants. Notably, Aldashev et al. (2012) highlight the strategic actions of customary justices attempting to retain power in the shadow of the formal law. In the extreme, they posit that progressive legal reforms may backfire by encouraging customary institutions to impose stronger penalties on individuals who exit. Similarly, Sterck and Aoust (2012) demonstrate how competition between forums may contribute to rent-seeking and bribery. As these issues are beyond the scope of our empirical analysis, here we focus exclusively on the strategic choices of individual plaintiffs.

3.1 Setup

We model three stages of a dispute between a plaintiff and a defendant, and the strategic verdicts of a customary chief and a formal magistrate. The timing of the game is as follows. Defendants and plaintiffs begin with identical utility endowments u_0. First, the defendant (D) chooses whether or not to inflict some harm $h \in [0, u_0]$ on the plaintiff (P). We conceive of harm broadly, to encompass both crimes and economic losses resulting in civil disputes.

Second, in response to this harm, the plaintiff chooses whether to carry the case to either the chief (C), the formal magistrate (F), or neither (N). Finally, the chosen judge offers a judgment, or legal remedy (r), which is essentially an offer to redistribute resources from the defendant to

the plaintiff. Judges' decisions are final and, thus, judges lack any ability to commit to deviations from their ex post optimal remedies.[3] We assume that all parties possess full information about each other's utility functions and the structure of payoffs.

In addition to the forum shopping decision, there are just two choice variables in the model to consider, denoted by roman letters: h denotes harm inflicted on the plaintiff by the defendant, and r_j denotes the remedy granted by judge j to the plaintiff. Subscripts i and j index the disputants and judges (or forums), respectively. The exogenous parameters of the model that will determine players' strategies are denoted by Greek letters: β_j denotes the bias of judge j; $\beta > 1/2$ denotes pro-defendant bias; and ϕ_j measures 'leakage' in the judge's remedy, with $\phi > 0$ implying the plaintiff's utility from r_j is less than the cost to the defendant. β can be conceived of simply as an endowment of personal characteristics (sex, wealth, power, ethnicity) that the custom is more likely to reward. ϕ can be conceived of as a cost to access the formal court paid by the plaintiff that does not accrue to the defendant. Conversely, but also consistent with our model, ϕ may capture the punitive nature of remedies in the formal system, in which the cost borne by the defendant (say physical punishment) does not deliver material gain to the plaintiff.

The core conceit of the model rests on two key assumptions about institutional differences between the customary and formal courts, which derive from the stylized facts listed at the end of Section 2.2. Our first basic assumption relates to judges' preferences or biases.

Assumption 1 *The custom is biased against certain identifiable social and demographic groups.*

Judges choose remedies r to maximize social welfare, u_j, subject to their own biases. Biases, denoted by $\beta_j \in [0, 1]$, may be pro-defendant ($\beta_j > 1/2$) or pro-plaintiff ($\beta_j < 1/2$). In the empirical analysis the direction of the bias will hinge on disputant characteristics. In accordance with the full information assumption, players also know each judge's biases in advance of making decisions about inflicting harm or choosing a forum. Judges are primarily concerned with rectifying inequalities between the disputants:

$$\max_{r_j} u_j = (1 - \beta_j) \ln u_p(r_j) + \beta_j \ln u_d(r_j) \tag{1}$$

Assuming *ex ante* equality, this amounts to repairing harms inflicted by defendants on plaintiffs. All other things being equal, judges prefer peace to conflict, and reparation to impunity. We assume that imposing remedies is costless to judges.

Our second basic assumption is technological, relating to the remedies at the judges' disposal.

Assumption 2 *Formal justice is 'leaky'—i.e., when a formal court rules in favor of a plaintiff, the utility lost by the defendant does not fully accrue to the plaintiff.*

[3] In this basic setup we do not allow either the plaintiff of defendant to appeal this initial verdict, thus precluding strategic interaction between the judges.

This inefficiency comes both from the overall higher costs borne by the plaintiff in accessing the formal system, and the punitive nature of formal justice that is less able to provide redress. It is reflected in the structure of the payoffs to the two disputants. Defendants derive benefit from the harm h, and experience the full disutility of the remedy r_j in both systems:

$$u_D = \begin{cases} u_0 + h & \text{if } j = N \\ u_0 + h - r_j & \text{otherwise} \end{cases} \tag{2}$$

In contrast, punitive formal remedies involve an additional cost to the plaintiff that represents the 'leakiness' of the formal system—representing the inability of the judge to fully compensate the plaintiff for the harm done.[4] This follows directly from Assumption 2.

$$u_P = \begin{cases} u_0 - h & \text{if } j = N \\ u_0 - h + r_C & \text{if } j = C \\ u_0 - h + r_F - \phi & \text{if } j = F \end{cases} \tag{3}$$

The key assumption is $\phi > 0$, i.e., the formal system is more costly or less efficient at delivering justice to plaintiffs. Note that the simple sum of utilities $u_P + u_D$ is greater in the customary sector ($2u_0$) than in the formal ($2u_0 - \phi$). We now determine the equilibrium of the game through backward induction.

3.2 Solving the model

Stage 3: Judges make decisions. In the third and final stage of the game, the chosen judge sets his or her optimal remedy, $r_j^*(h, \beta_j, \phi_j)$, by solving the maximization problem in equation (1), yielding:

$$r_C^* = h + (1 - 2\beta)u_0 \tag{4}$$

$$r_F^* = h + \frac{\phi}{2} \tag{5}$$

Because their decisions cannot be appealed, both formal and customary judges choose their unconstrained optimum. Given full information of the judges' preferences, the plaintiff would not consider credible any more favorable signal from either judge.

In the customary system, the chief's bias affects the remedy he hands out. In the formal system, the plaintiff is fully compensated for the harm done, but only partly compensated for the verdict's 'leakiness'. Since judges make decisions independently, 'leakage' does not impact the chief's decision ($\frac{\delta r_C^*}{\delta \phi} = 0$), and bias does not impact the magistrate's decision ($\frac{\delta r_F^*}{\delta \beta} = 0$). A more

[4]This could be seen simply as the costs of travel or contesting a case in court, the deadweight loss due to punitive rather than redressive justice, or a range of other social and financial costs associated with taking a case to the formal system.

biased chief imposes a smaller penalty on the defendant ($\frac{\delta r_C^*}{\delta \beta} = -2u_0$), and the formal remedy increases with the 'leakiness' of the formal system ($\frac{\delta r_F^*}{\delta \phi} = 1$). In both systems, greater harm necessitates a higher remedy ($\frac{\delta r_C^*}{\delta h} = 1$ and $\frac{\delta r_F^*}{\delta h} = 2$) .

Stage 2: Plaintiff chooses forum. In the second stage, a forward-looking plaintiff with knowledge of the judges' remedies chooses $j \in (N, C, F)$ by comparing her utility in each potential forum. This is equivalent to comparing the remedy she would receive from the chief, r_c^*, and the remedy from the magistrate accounting for 'leakage', ϕ. Remedies in turn depend on β and ϕ (equations 4 and 5). Comparing the utilities implied by these options and simplifying, the thresholds at which P switches between systems are given by

$$\phi = 4u_0\beta - 2u_0 \tag{6}$$

$$\phi = 2h \tag{7}$$

$$\beta = \frac{1}{2} + \frac{h}{2u_0} \tag{8}$$

Equation 6 determines the choice between F and C, equation 7 between F and N, and equation 8 between N and C. Figure 3 plots each of these conditions in (β, ϕ) space. Each region displays P's ordering of utilities for the combination of conditions that define the region. Given the structure of the game, P will choose her first-best choice of forum in every instance. The shaded regions correspond to conditions under which P's choice is the customary system, the formal system, or none. Greater bias (β) reduces the likelihood of reporting to the chief, while higher 'leakiness' (ϕ) reduces the likelihood of reporting to the magistrate.

Figure 3 also shows how P's choice set changes with changes in h and u_0. As $h \to u_0$, the 'non-reporting' region shrinks—all else equal, higher levels of harm raise P's willingness to report the crime, but does not change the relative merits of the two systems. In contrast, as $u_0 \to 0$, the chief becomes more attractive relative to both the magistrate and non-reporting, suggesting that on average, individuals with lower initial endowments end up in the customary system.

The above discussion generates a number of testable predictions.

Prediction 1. *Plaintiffs are more likely to report the dispute ($j = C, F$) when harm h is high.* Thus we would expect, e.g., violent crimes or land disputes to be reported more often than 'minor' disputes.

Prediction 2. Ceteris paribus*, disadvantaged plaintiffs—i.e., those with lower utility endowments (u_0)—are more likely to report to the customary system.*

This implies that in the empirical analysis we expect women to be more likely to use the customary system, all other things being equal. The model suggests that the most important factor to hold constant for this to be true is the identity of the other party to the dispute. Prediction (2) is driven by resource endowments of the plaintiff alone, which we proxy with his/her demographic

and socio-economic characteristics. Prediction (3) is driven by levels of bias in the customary system, which are determined by the *gap* between the wealth or privilege of the plaintiff and defendant.

Prediction 3. *As pro-defendant bias in the customary system (β) increases, the probability of reporting declines, and the probability of carrying the case to the formal system increases.*

Thus we would expect, for example, a female plaintiff to face greater bias in the customary system when pursuing a case against a male defendant, and consequently be more likely to take a male defendant to the formal system than a female defendant, or else not report the dispute at all. Similarly, we expect poor or otherwise disempowered plaintiffs to be more likely to take wealthier and more powerful peers to the formal sector. The source of this prediction can be seen by comparing the solutions to the judges' maximization problem in Equations 4 and 5.

Stage 1: Defendant chooses harm. In the first stage, D will choose a level of harm that gives him the greatest utility conditional on his knowledge of P's future forum choice. We can rank D's utilities from each forum in a similar manner to P's. Figure 4 overlays a partial ranking of D's utilities onto Figure 3. Depending on the exogenously determined values of β and ϕ, D will be located at some combination of (ϕ, β) in one of three relevant regions X, YY' or ZZ'. Before we examine D's choice of h, it is worth noting that the only region where D could end up in the formal system is ZZ', and in this region his utility is strictly less than if he were in N or C. In contrast, since P always chooses the forum that maximizes her utility, at the margin we expect P to be indifferent between forums. Thus we can make another prediction:

Prediction 4. *The customary system provides greater aggregate welfare than the formal system, in that the sum of the utilities of P and D are higher. Furthermore, for ϕ above some threshold (i.e., a costly, inefficient, or punitive formal sector), D is strictly worse off if P chooses the formal sector.*

Note that we make no unambiguous prediction about the utility of the plaintiff in the formal versus the customary system. Plaintiffs rationally choose their forum conditional on exogenously determined β and ϕ as such, we would expect utility-maximizing agents to be indifferent at the margin.

But prediction (4) suggests clearly that the subjective satisfaction of defendants would be lower in the formal system. Furthermore, the combination of (a) being taken to the formal system, and (b) demographic characteristics that suggest the defendant would have received a favorable outcome in the customary system should produce a strongly negative outcome in defendants' eyes.

We return now to D's choice of harm. In the world of pro-plaintiff bias, X, where $\beta < \frac{1}{2}$, D's choice of h will not influence P's forum choice—P will always choose the chief and D will always receive $u_D(r_C^*) = 2\beta u_0$. Thus if $\beta < \frac{1}{2}$, D's choice of harm is irrelevant, and P always chooses $j = C$.

If bias is pro-defendant ($\beta > \frac{1}{2}$), D's choice of h will allow him to determine the region he occupies, by moving the $\phi = 2h$ and $\beta = \frac{1}{2} + \frac{h}{2u_0}$ lines along the $\phi = 4u_0\beta - 2u_0$ line—an increase in h will reduce the "no reporting" region $Y'Z'$.

In this world, if D finds himself in region YY', his choice of h will determine whether—for a given combination of (ϕ, β)—P will choose the chief or not report at all. By setting a low h, D can increase the size of the "no reporting" region sufficiently to ensure that P does not report, and vice versa. However, no matter what he does he cannot get his first-best choice because P's interests are dramatically opposed to his. In sub-region Y, where $j = C$, the chief's bias towards D is not high enough to make up for the loss of the remedy transferred to P, so $u_D(r_N^*) > u_D(r_C^*)$. Conversely in Y', where $j = N$, the chief's bias is high enough to more than make up for the remedy, so $u_D(r_N^*) > u_D(r_C^*)$. Thus while D can influence P's forum choice, no matter what level of h he sets he will always get suboptimal utility.

Finally, assume that D is in region ZZ'. His choice of h will determine whether—for a given combination of (ϕ, β)—P will choose the magistrate or not report at all. In both subregions Z and Z', $u_D(r_N^*) > u_D(r_F^*)$, i.e. that D always prefers non-reporting. This implies that D will choose a low enough h to expand the "no reporting" region so that he ends up in region Z', where $j = N$. Simply put, the chief's high pro-defendant bias means P will never go to the customary system, and the higher the harm D commits, the more likely P is to report him to the magistrate—which makes D strictly worse off. So D chooses low harm and P chooses $j = N$.

One takeaway from this discussion is that D never sets h such that P will choose the formal system—D avoids the formal system at all costs. This is consistent with our story that the formal system provides lower aggregate welfare than the customary, since taking D would entail a large loss of utility. In addition, though our model is extremely simple, it reflects quite well the empirical reality in Liberia where only four percent of disputes ever make it to the formal system—and most of those are "high harm" disputes such as murder, rape, and land (Table 1). The only circumstance where D prefers F to C is when bias is pro-plaintiff and ϕ is low—but pro-plaintiff bias means P always picks the chief.

While the formal system will only be chosen in rare circumstances, our model highlights its role as an effective deterrent to harm. In the absence of the formal system, D has no incentive to set low harm—in region X, he is trapped in the customary system, and in region YY' though he can influence forum choice he cannot optimize his utility. But when the formal system casts its shadow in region ZZ', D actively reduces harm in order to prevent P from taking him to the magistrate. Thus our relatively simple model also captures one of the basic insights from Aldashev et al. (2012)—that the formal system, though rarely used, plays a crucial role in determining social welfare outcomes.

3.3 Introducing legal aid

We consider now the predictions of the model for the randomized impact evaluation. Specifically, we introduce into the model an informal legal-aid program which, on the one hand, is based on statutory legal principles (i.e., exhibiting low bias, β) and, on the other hand, is *pro bono* and focuses on non-punitive resolution of disputes (i.e., low leakage, ϕ). This description is in line with our *pro bono* paralegal program, which is described more fully in Section 5.1.

Clearly, such a program would strictly dominate the F for both P and D, since neither has to bear the costs associated with the formal system. Similarly, P would now prefer the customary system if and only if she is in region X, where bias is pro-plaintiff. D would have the exact opposite preferences. It is worth noting that in this simple setup, the result is identical whether we consider legal aid to be a new forum L, with $\beta = \frac{1}{2}$ and $\phi = 0$ that strictly dominates F, or simply a resource transfer to P equal to ϕ that effectively sets formal system 'leakiness' to zero.

This generates the following three predictions for the randomized evaluation.

Prediction 5. *Take-up: A disproportionate share of disputes taken to the program would, absent legal aid, not have been taken to any forum.*

The model is not unambiguous on this point, but prediction (5) emerges from a combination of the theoretical model and what we know about actual forum-shopping behavior in the observational data – i.e., a parameterized version of the model. Of cases taken the customary system, legal aid will only be attractive when β is low. For cases taken to the formal system, it depends on whether we conceptualize legal aid as a substitute for the formal system (i.e., as a competing forum L) or as an input into it (i.e. as a resource transfer ϕ). In the first case, *pro bono* legal aid is always a better option, so the number of cases going to the formal system would go down. In the second, legal aid lowers formal system costs, thereby increasing reporting to the formal system. Thus predictions on formal system reporting are ambiguous. In contrast, there is a large number of disputes not reported, and *pro bono* legal aid lowers the two hurdles to reporting in the model: bias and cost.

Turning from take-up patterns to the effect of the program conditional on take-up, the model generates two predictions about treatment effects on the treated.

Prediction 6. *Treatment effects: Legal aid will increase plaintiffs' payoffs.*

Prediction 7. *Heterogeneous treatment effects: Plaintiffs facing disadvantageous bias in the customary system will benefit more from legal aid.*

Prediction (6) is fairly obvious and in no way unique to our theoretical setup. The primary function of empirical testing is (a) to establish the magnitude of the potential welfare gains at stake here, and (b) to explore, beyond the narrow predictions of the model, the scope of impacts, i.e, on case outcomes, subjective satisfaction measures, material well-being, etc.

In contrast, prediction (7) provides a testable implication of our modeling framework. The

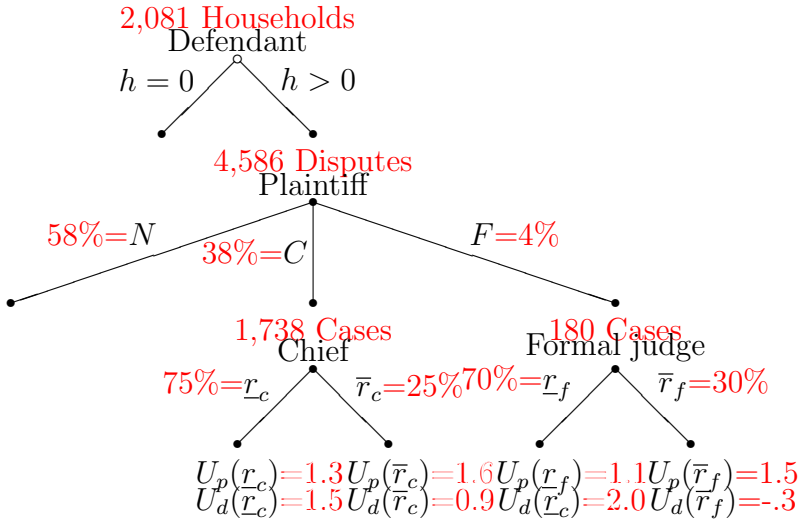

Figure 2: Game Tree & Empirical Proxies

depiction of the trade-offs faced by disadvantaged social groups in making forum-shopping decisions help to highlight who stands to gain the most from *pro bono* legal aid.

4 Evidence of forum shopping from observational data

To test the empirical empirical predictions of the model, we use data drawn from two rounds of an original household survey conducted by the authors in August 2008 and February 2009.[5] The full sample includes 2,081 households spread across 176 communities in five Liberian counties: Bong, Grand Gedeh, Lofa, Maryland and Nimba, shown in Figure 5. Together these counties account for nearly two-fifths (38 percent) of the population of Liberia, and more than half (56 percent) of the population outside Monrovia. First-stage sampling of communities within each county was based on random probability-proportional-to-size (PPS) sampling from the full list of communities in the 2008 Census of Liberia. In the second stage, a simple random sample of households was drawn within each selected community using in-field randomization.[6] As the primary purpose of the survey was to look at inter-household disputes, survey respondents were typically the household head, their spouse, or if neither was present, a representative adult member able to answer questions on behalf of the household.

Disputes are the basic unit of analysis in much of what follows, yet the boundary of what qualifies as a 'dispute' was left deliberately vague, and respondents were free to report disputes as they defined them. Thus it is important to analyze the incidence of and response to disputes within well-defined sub-categories of crimes and civil cases. In total, the 2,081 households in our final

[5]These survey rounds also constitutes the baseline for our community-level randomization.

[6]In the initial design of the survey we had anticipated that legal disputes would be rare events, requiring the need to screen respondents and over-sample those with disputes relevant to the study. However, a pilot survey conducted in July 2008 showed widespread incidence of crime and conflict across all communities.

estimation sample – restricted to those with full socioeconomic data on both parties to any dispute – reported 4,586 separate disputes, with 98 percent of households reporting at least one dispute. Disputes were solicited through a 60-90 minute interview focusing on respondents' experience of a wide range of crimes and conflicts, including assault, sexual violence, murder and theft, as well as disputes involving land, debt, property, and family. Respondents were asked whether any member of their household had been affected by each dispute type, and if so, whether the other party was another household member, another member of the community, an outsider to the community, or unknown. Intra-household disputes were treated similarly to disputes between households. Details were collected for each dispute reported within the past year, including the forums visited, the time and costs incurred, and details of the judgment including reported subjective satisfaction.

Limited socioeconomic and demographic information is available for both parties (plaintiff and defendant) to each dispute, including sex, occupation, relationships to powerful figures, and ethnicity. However, we were constrained to soliciting this information from only one party to a given dispute— privacy and ethical considerations, e.g. the possibility of worsening or reigniting a previous dispute, as well as concerns about encouraging biased or censored response prevented us from tracking down the adversary to collect full dyadic data. Accordingly, subjective views about fairness or satisfaction with case outcomes are only reported in the first person by party being interviewed. In the analysis we include dummies wherever appropriate for whether the respondent was the plaintiff or defendant in the dispute.

4.1 Mapping theory to data

In this section we begin with some descriptive statistics, then attempt to map the data to the key variables in our model.

The core hypothesis of the theoretical model concerns forum choice. The raw data includes disputes taken to dozens of different forums on a fairly continuous spectrum, from 'family head' or 'elders' at the customary extreme, to police and magistrates at the formal extreme. For most of the analysis, forums are grouped into just three options corresponding to the theoretical model: 'no forum' if the respondent reports that the case was not taken to any third party; 'formal', which is limited to justices of the peace, magistrates, police and other military/government officials; and 'customary', which encompasses all other forums, including town, clan and paramount chiefs, as well as elders, family leaders and secret societies.[7]

Using these broad categories, 38 percent of disputes were taken to the customary system, while just 4 percent were taken to the formal system. In addition, 58 percent of disputes were not reported to any forum, and were either resolved by the disputing parties themselves or left unresolved. Table 1 disaggregates these patterns by dispute category. In line with Prediction 1

[7]While we made every effort to communicate to respondents that 'reporting' taking the dispute to *any* third party for mediation or adjudication, it is possible that respondents did not fully understand reporting to the lowest rungs of the customary system, artificially increasing the number of disputes that were taken to 'no forum'.

from the theoretical model, there is a clear tendency for violent crimes to be taken to the formal system (25.8 percent of murders, 21.2 percent of rapes and cases of sexual abuse) while the civil cases that dominate the sample are very rarely taken to the formal system (1.5 percent of the debt disputes and 1.4 percent of the family or marital disputes, which together comprise almost two-thirds of the sample).

Table 2 examines the relationship between forum shopping and plaintiff characteristics. We examine each of the four demographic measures at our disposal, namely whether the respondent is male, wealthy (based on a dummy for whether the household head has any non-farm employment), powerful (based on a dummy for whether the disputant is or is not related to a local leader), or drawn from the largest ethnic group in the community. In line with Prediction 2 from the model, the socially disadvantaged *on average* are significantly more likely to take their case to the customary system—women more so than men (42 percent versus 37 percent), farmers more so than non-farmers (38 percent versus 35 percent), and the 'powerless' more so than the powerful (40 percent versus 29 percent). It is worth noting that this table is silent on Prediction 3, regarding bias, since we consider bias as being disadvantaged *relative to the other party* in the dispute.

Table 3 presents favorable response rates for self-reported subjective evaluations of five justice outcomes: 'fairness', 'satisfaction', 'winning', 'willingness to return to the forum', and 'respect received'. 'Satisfaction' and 'respect' are measured on a five-level Likert scale ('very satisfied', 'somewhat satisfied', etc.); 'winning' is measured on a three-level scale measuring in whose favor the verdict was given ('my favor', 'neutral', or 'other party's favor'); and 'fairness' and 'willingness to return' are binary variables ('yes' or 'no') measuring whether the respondent felt the decision was fair and whether they would be willing to bring another dispute to the forum. The table summarizes the relevant favorable response for each of these measures (respondents answering 'yes', 'my favor', and 'somewhat satisfied' or 'very satisfied', as appropriate), as a percentage of all disputes resolved in customary forums and formal forums, respectively. Thus 92 percent of respondents who had a dispute resolved in a customary forum thought that the outcome was fair, compared to 85 percent of respondents at formal forums. It is worth noting that in all measures, people appear to be happier with the customary system, in line with Prediction 4 regarding the customary system's relative efficiency.

Figure 2 reproduces the theoretical game tree from Figure 1, overlaying descriptive statistics from the dispute database. Starting at the bottom of the figure, our model equates justice with the utility (u_P and u_D) that it generates. In the empirical analysis, utility is proxied by self-reported evaluations of justice outcomes: notably the five measures of fairness, satisfaction, 'winning', willingness to return, and respect received summarized in Table 3. For brevity, we generate an aggregate index of all these subjective measures of justice, based on the first principal component taken from a factor analysis.

Remedies, r_C and r_F, in rural Liberia are difficult to quantify. Monetary compensation is rare, especially in the customary system. Instead, remedies are often in-kind or focus on loss of stature or

reputation through mandated apologies. We generate an aggregate measure of remedies, indicating whether any physical punishment was meted out, any material compensation was incurred, and whether either party issued an apology. The average values of this PCA index for plaintiffs and defendants, respectively, in cases where any remedy or punishment including apology was ($\bar{r}_j$) or was not ($\underline{r}_j$) incurred are listed at the bottom of Figure 2.[8]

Our analysis relies heavily on an empirical measure of customary judges' bias (β). While we do not observe biases directly, we posit that the chief's bias in a given case will be determined by the characteristics of the plaintiff and defendant, reflecting the hegemony of certain social and economic groups. In particular, we hypothesize that bias will favor disputants who are male, employed, powerful, and from the dominant ethnic group. Characteristics that work in one's favor are coded as positive values in defining β. Both the plaintiff's and defendant's characteristics are clearly relevant. β equals the difference between defendant and plaintiff characteristics, such that $\beta > 0$ is pro-defendant bias. For example:

$$\beta(\text{Sex}) \equiv \text{male dummy for defendant} - \text{male dummy for plaintiff.}$$

As coded, this implies that we expect customary judges to be more likely to side with the defendant when, say, a woman sues a man ($\beta = 1 - 0 =$ pro-defendant bias).

Moving up the game tree, the core hypothesis of our model concerns forum choice, and we present the relative take-up rates for different forums as described in Table 1. Finally, at the very top of the game tree, the level of harm, h relates to the incidence and severity of losses incurred by the plaintiff. Harm is not observed directly. (Monetary losses are measured when the respondent is the plaintiff, but are applicable for only a subset of disputes.) Instead, we control somewhat crudely for variation in the severity of harm using dispute-type dummies, covering 19 different categories of dispute. Thus we analyze the relationship between, for instance, disputant characteristics and forum choice by comparing land cases to land cases, thefts to thefts, and so on, but do not control for variation in severity of harm within these dispute categories.[9]

[8] As noted earlier in Section 4, we interviewed only one party to each dispute. While the objective characteristics underlying the bias measures referred to earlier (sex, ethnicity, etc.) are solicited for both the plaintiff and defendant from a single respondent, this is clearly not appropriate for subjective evaluations of justice. Thus justice outcome data is available for either the plaintiff or defendant for a given case, not both. However, we can speak about both plaintiffs and defendants in the aggregate, as we interview plaintiffs in some cases and defendants in others.

[9] A special case occurs when the level of harm is zero, and no dispute is observed. As noted in Figure 2, the estimation sample covers 2,081 households. Because these households comprise a representative sample their respective communities, it is possible to examine the endogenous decision to inflict harm by defendants, by relating the probability of victimization to household characteristics. Due to limits of time and space, this analysis is not included here.

4.2 Testing the model

The fundamental premise of our modeling framework is that plaintiffs exercise agency in choosing a forum to hear their case, and that these choices are made strategically to maximize plaintiffs' own welfare, possibly at the expense of defendants. An extreme alternative hypothesis would be that agents are bound by laws or norms to one system or another: legal dualism as legal apartheid. At the other extreme (more in keeping with our rational choice approach but taking its logic further than we feel is warranted), one might speculate that rational forum shopping and strategic behavior by judges could lead to an equilibrium where judgments are indistinguishable between forums, something analogous to the race to the middle in a Hotelling model.

This section econometrically tests the predictions of our model, implicitly weighing it against these alternative approaches. As detailed below, we find that individuals likely to suffer negative bias in the customary system are more inclined to exit to the formal system—consistent with rational forum shopping. We also show that plaintiffs bearing these (disadvantaged) characteristics receive greater differential utility from the formal versus the customary system. Furthermore, defendants with traits favored by the customary system do on occasion end up in the formal system and suffer utility losses when they do, all suggesting that judgments in the two systems have not converged and judgements 'stick', in the sense that infinite appeals are not feasible.

We first test two main predictions listed in Section 3 above: prediction 2, that disadvantaged plaintiffs—i.e., plaintiffs with lower u_0—are less likely to be able to bear the costs of reporting given their low utility endowments, and therefore on average more likely to choose the customary system (and conversely, privileged plaintiffs are less likely to do so) and prediction 3, that plaintiffs facing more customary bias—i.e. those facing high β—are less likely to report to the customary system.

We test these two hypotheses together using a multinomial logit specification, regressing an indicator for the choice of forum on empirical proxies for β.

$$I_i(j = N, C, F) \quad = \quad \gamma_0 + \Gamma_1 \beta_i + \Gamma_2 u_{0i} + \delta_d + \upsilon_i \tag{9}$$

where β_i is a vector of measures of pro-defendant bias, calculated as described earlier for each of four categories (sex, employment type, power and ethnicity); u_{0i} is a vector of measures of plaintiff utility endowment, as indicated by dummy variables for male, non-farm employment, powerful, and ethnic majority; and δ_d are dispute dummies for four categories of disputes ("Family", "Economic", "Violent", and "Other", which we use as crude controls for the level of harm inflicted.[10]

[10]These categories are constructed from 19 categories of disputes collected in the survey: "Accused of assault" "Accused of murder" "Accused of other crime" "Accused of property destruction" "Accused of rape" "Accused of rape/sexual abuse" "Accused of theft" "Accused of witchcraft" "Bribery/corruption" "Child custody dispute" "Child/wife neglect" "Debt dispute" "Divorce/separation" "Domestic violence" "Family or marital dispute" "Labor dispute" "Land dispute" "Property dispute (not land)" "Victim of assault" "Victim of murder" "Victim of other crime" "Victim of property destruction" "Victim of rape/sexual abuse" "Victim of theft" and "Victim

Our model predicts that when $j = C$, $\hat{\Gamma_1} < 0, \hat{\Gamma_2} < 0$—the customary system is the less preferred choice for (1) privileged plaintiffs with high u_0 (who can 'afford' the costs of accessing the formal system) and (2) plaintiffs who are disadvantaged relative to the defendant (high β). The results in Table 4 show that on the whole the model is quite successful in explaining the decision to choose the formal over the customary system. It is somewhat less successful in predicting the decision to report to the customary system, or not to report.

Column 1 shows that three of the four measures of β are significantly and negatively associated with taking a case to the customary system relative to the formal, as predicted. Similarly, in three of four cases, the four measures of u_0 also bear a significant, negative coefficient as predicted. In both cases, variables constructed from the ethnicity variable are insignificant. In short, the results are consistent with the notion that plaintiffs who face severe bias in the customary system and have the means to go elsewhere do so.

Column 2 shows the results for the decision to report to the customary system relative to not reporting at all. For both the β proxies and the proxies for u_0, coefficients display conflicting signs. Coefficients for non-farmers and powerful people are significant and negative, as predicted. However, the coefficient for sex is significant and positive, while the coefficient for ethnic majority is insignificant. The model's treatment of the decision to report was relatively sparse compared to the attention given to the distinction between the formal and customary systems, and further attention to the possibility of bargaining and reconciliation outside a third-party forum appears to be merited.

While the model has no strict prediction for the choice between the formal system and not reporting at all, Column 3 can perhaps provide some insight into the relative costs ϕ in accessing the formal system. For two of our four measures, plaintiffs with high u_0 or facing high β strongly prefer reporting to the formal system relative to not reporting at all, suggesting that all else considered, the relative cost of taking a case to the formal system is not prohibitive.

The key theoretical prediction regarding disputants' satisfaction (or perceptions of justice) from Section 3 is that defendants always prefer the customary system, except when bias is pro-plaintiff and the formal system is efficient at redistributing resources (Prediction 4). This suggests a series of interaction effects among the empirical determinants of utility, combining dummies for plaintiff/defendant status, relative bias, and a dummy for whether the case was taken to the formal system.

$$\begin{aligned} u_i &= \gamma_1 P + \gamma_2(P \times \beta) + \gamma_3(P \times F) + \gamma_4(P \times F \times \beta) + \\ &\quad \gamma_5 D + \gamma_6(D \times \beta) + \gamma_7(D \times F) + \gamma_8(D \times F \times \beta) + \epsilon_i \end{aligned} \tag{10}$$

where u_i is a proxy for utility, P is a dummy denoting that the respondent is the plaintiff, and

of witchcraft". We aggregate for the sake of space and simplification, but our results do not change if we include the 19 disaggregated categories.

F is a dummy for cases taken to the formal system. The empirical results in Table 5 are broadly consistent with the theoretical predictions. Column 1 shows that plaintiffs are generally indifferent between forums, but defendants are significantly less happy in the formal system. This is entirely in keeping with a customary system that produces greater aggregate welfare (prediction 4), as the sum satisfaction scores for the plaintiff and defendant is negative in the formal system ($0.43 + 0.51 - 1.13 = -0.19$) and positive in the customary system ($0.43 + 0.51 = 0.94$).

Columns 2–5 provide further interactions with each of our measures of bias and provide some tentative supporting evidence for our predictions regarding the interaction of forum choice and bias. For example, plaintiffs facing gender bias are significantly happier in the formal system (column 2, $\hat{\gamma}_4 > 0$, significant at 1%), while plaintiffs facing ethnic bias are significantly unhappier in the customary system (column 4, $\hat{\gamma}_2 < 0$, significant at 5%). Defendants in the formal system are consistently significantly unhappy $\hat{\gamma}_7 < 0$, significant at 1% across all specifications)—but significantly unhappier in the formal system when they would have had gender or ethnic bias going in their favor (columns 2 and 4). This pattern is consistent with punitive formal system remedies that harm defendants to a greater extent than they benefit plaintiffs, in a system where the custom is biased towards some types of people and not others.

4.3 Discussion

On the whole, this pattern of results suggests not only that forum choices are made rationally to benefit the interests of the plaintiff, but that the judgements received in the chosen forum have utility consequences which are not bargained away or overridden through appeal. It provides evidence of individual agency (and, in particular, forward-looking rational choice) in forum shopping, running counter to starker depictions of legal dualism such as in Mamdani (1996), and fits well with our anthropological understanding of justice choices in Liberia (Isser et al., 2009) and elsewhere.

Our claim that plaintiffs exercise strategic choice in forum shopping confronts a *prima facie* tension between (a) well-documented bias in Liberian customary law, depriving women and marginalized groups of basic rights, and (b) the the simple empirical fact documented here that even these disadvantaged plaintiffs take most (but not all) of their cases to customary forums. Why would marginalized groups choose to bring cases to customary courts that systematically repress them?

An obvious answer, in theory, is provided by high costs of entry to the formal system, i.e., barriers to "access to justice" in development speak. In rural Liberia, such barriers are undeniable. For plaintiffs in remote villages, travel costs alone to reach a police station or formal court are significant relative to the material stakes in many disputes. Court officials routinely solicit bribes and rural peasants may be ignorant of formal legal procedures.

Our results suggest that barriers to entry may not be the whole story, and that there are positive

features of customary justice that attract even disadvantaged plaintiffs. Notably, while plaintiffs who win favorable verdicts in the customary system exhibit higher satisfaction than those who do not, no such pattern exists in the formal system. Looking across systems, plaintiffs are generally less satisfied with the justice provided in formal than customary forums, questioning any notion of a clear hierarchy in the attractiveness of these systems. Finally, defendants are overwhelmingly less pleased with outcomes in the formal system – even after controlling for demographic characteristics and the nature of the dispute.

These patterns conform to the predictions of our simple model of strategic forum choice in which plaintiffs trade off the *rights* afforded them in the formal system in favor of the more efficient legal *remedies* delivered by customary courts. Our empirical evidence on the impotence of the formal system in generating utility for plaintiffs, combined with its success in creating disutility for defendants, corroborates one of our basic modeling assumptions: formal courts are relatively punitive, while customary law is more 'restorative', in the sense of efficiently redistributing utility from defendant to plaintiff with fewer Pareto losses.

To conclude this section, we briefly indulge ourselves by taking these empirical findings as given, and consider the normative policy implications of the underlying model.

As a thought experiment, consider a social planner with progressive preferences (very low β) and the power to influence both the customary and formal system, e.g., Liberia's central government under the leadership of the well-intending government of Ellen Johnson Sirleaf. The planner is always tempted to further reform the already-progressive norms of the formal system—by passing new laws—and to attempt to replace or reduce the dominance of custom, say by top-down decentralization programs. These approaches are popular with top-down reformers, especially in post-conflict settings where there is often (we would argue) a mistaken assumption that local institutions have somehow ceased to be relevant, or are weak enough to be written off. Yet these solutions lack clear coherence in our model, and seemingly fail to recognize the revealed preferences of the rural poor in seeking out customary justice focused on reconciliation and less punitive remedies. Aldashev et al. (2012) model this approach of top-down reform more explicitly, and highlight the potential for unintended negative consequences from increasing the distance between customary and formal laws.

Alternatively, the planner can attempt to reform customary norms (reduce β) or increase the appeal of formal justice by making the system less punitive and more focused on delivering tangible benefits for plaintiffs (reduce ϕ). Both will be effective in theory. In practice, these alternatives are manifested, respectively, in ongoing collaborations by domestic civil society organizations and international NGOs to train customary leaders in their judicial responsibilities, and to provide quasi-formal alternative dispute resolution mechanisms that are less costly and punitive than police and magistrates' courts. The rest of this paper examines one such intervention, which seeks to help the socially disadvantaged gain access to low-cost, remedial justice that incorporates the progressive features of the formal law, by means of *pro bono* mediation and advocacy services

provided by community paralegals.

5 Experimental evidence on *pro bono* legal aid

This section presents the results of a randomized controlled trial of a mobile paralegal intervention. The program's design, as noted earlier, is consistent with the broad implications of our analysis of forum shopping for the design of legal empowerment initiatives—paralegals reduce both the direct costs of accessing the formal law as well as the costs associated with punitive formal system remedies, thereby increasing competition between formal and customary law. However, we are unable to directly test the model's predictions in the experimental analysis, due to constraints imposed by the nature of program implementation. As such, the discussion in previous sections should be seen as a motivating framework for the intervention.

5.1 The community paralegal program

The Carter Center's access to justice initiative in Liberia, active since 2006, is a part of the larger push towards strengthening the rule of law. Implemented in partnership with local civil society organizations, the Ministry of Internal Affairs and the Ministry of Justice, the initiative aims to support formal legal reform, educate Liberians of their rights under the law, and provide them everyday access to justice and legal settlement of disputes.

The flagship component of the Carter Center initiative is a community paralegal program, meant to provide an immediate alternative to other local justice mechanisms, both formal and customary. Community-based paralegals are recruited from the counties in which they work, and typically possess secondary school or college education. They are trained periodically in mediation, advocacy, formal law, and the roles of the different legal agencies. They are mandated to provide free-of-cost legal advice and services to local residents. In particular, they assist individuals and communities with a wide range of disputes, by providing information about the law and their individual rights, advocating on their behalf to customary and formal authorities, and directly mediating disputes if so requested.

With some exceptions, paralegals generally wait for potential clients to approach them with live disputes or grievances. These may range from child and spousal support cases, to disputes over land, debt, labor, or property, to violent crimes such as assault and gender-based violence. Paralegals assess the client's initial story to see if it constitutes a 'case', i.e. where action of some sort is viable (as opposed to, for example, clients coming to talk about losses during the war, or crimes committed by unknown parties who cannot be identified or tracked down). If the dispute or grievance constitutes a 'case', paralegals decide whether or not to accept it depending on their existing caseload and the merits of the case. Paralegals encourage their clients to state what action they would like to take—for example, take the case to court, arrange a mediation to attempt to

reconcile, advocate on behalf of the client to relevant authorities, etc. (Notable exceptions are murder, rape, and other forms of violence where paralegals refuse to mediate and strongly advocate taking the case to a formal authority.) Paralegals lay out these and other options, and provide some information about the laws and processes involved and the likely outcomes.

If the client chooses to take the case to a formal authority, the paralegal becomes an informal legal advisor and advocate, guiding the client through the procedures and looking over the shoulder of authorities to ensure that due process is being followed. This is usually enough for the large majority of cases that do not formally make it into court. For cases that go to court, under some circumstances the paralegal could refer the client to in-house lawyers maintained by the Carter Center who may choose to represent the client. Lawyers also provide a limited range of other legal services that require formal legal assistance, such as filing bail applications in court, but even in those instances the paralegal is responsible for following up on the procedures.

Mediation, easily the most popularly selected recourse, is conducted along standard and familiar procedures of conflict resolution. Paralegals receive mediation training from the Carter Center's lawyers as well as other organizations such as the American Bar Association's local office. Mediation typically begins with the paralegal issuing an invitation (or multiple invitations, as required) to the other parties involved in a dispute. Invitations carry some weight, both because they are issued as written documents—a rarety in local dispute resolution—and because NGOs hold a great deal of sway in rural Liberia and are widely perceived as external, formal actors.

Mediation is conducted at a mutually agreed upon venue, and may be held over multiple sessions if so needed. If parties agree upon an outcome, the paralegal writes out a formal mediation agreement and leaves a copy with both parties. Though paralegals have no means of enforcing the agreement, they maintain a close watch on the case for at least three months after case resolution and check in periodically with their clients. If the agreement is reneged upon, paralegals may offer to reopen a case and pursue an alternative means of recourse if the client so desires.

Paralegals also play broader roles as individual and community advocates. They may approach traditional authorities, employers, government agents, or other informal actors as needed to advocate on behalf of their clients, which might be individuals or communities. They also play a broader social role that often goes beyond that of a legal representative—for instance, they may take on extreme cases of injustice *suo moto*, or in exceptional circumstances switch their allegiance to the other party—for example, if approached by a husband who has been beating his wife. To date, the paralegal program has taken up over 4,000 disputes, ranging from land and property to gender-based violence and child support.

Till 2009, paralegals were only based in the major towns in each districts. The high cost of travel, both in terms of time, transport, and the social cost incurred by individuals in taking a dispute to an outside third party, meant that new cases came predominantly from urban or peri-urban areas. From February 2009, the program added on a mobile outreach component as a means of extending its scope to more remote communities beyond the reach of the town-based

paralegals. 'Mobile paralegals' were deployed on motorbikes to 160 villages across five of Liberia's fifteen counties: Bong, Grand Gedeh, Lofa, Maryland, and Nimba. Program communities included mining towns and plantations, border towns with high refugee populations, and a large number of remote towns inaccessible by road. Each paralegal was assigned to ten communities in her or his county, and required to make two visits per month to each community, during which they would conduct information sessions, take new cases, follow up on ongoing cases, or check in on resolved cases.

Paralegals follow a strict protocol when arriving in the village. They begin by greeting the local leaders, who over time have become familiar with the aims and objectives of the program. They then follow up on ongoing cases, e.g. meeting with either party to a dispute, providing information to a client, etc. Depending on their workload, they also conduct information sessions that typically take the form of a community meeting. Each session covers a broad topic, typically related to women's rights (domestic violence, rape, spousal and child support, inheritance, etc.), or rights to land, labor rights, etc. On occasion they make forays into laws governing witchcraft and 'sassywood' (trial by ordeal), the structure of the legal system and local administration, political participation, etc.

This expansion means that, in principle, the program can bring (the content of) the formal law literally to the doorstep of those it serves. Furthermore, the 'mobile' model allows a relatively small number of paralegals to cover a large number of communities on a flexible schedule, making it a relatively cost-effective, labor-intensive approach to extending the reach of the formal legal system—and one with potential for scaling up.[11] Finally, this expansion provided a unique opportunity for a randomized evaluation of the program, expanding as it was into communities that had little prior experience of this sort of intervention and that were typically far from the reach of the formal legal system.

5.2 Evaluation design

The evaluation design follows a baseline and follow-up survey structure, combining difference-in-differences analysis with individual-level randomization of the Carter Center's mobile paralegal program. The evaluation was launched in July 2011 and extended through December 2011. The main objective was to explore effects on paralegal clients.[12] Client-level randomization was conducted in villages that were already part of the community paralegal program. As clients typically approach the paralegal with questions or requests for assistance, the sample was entirely self-identified from within the village population. Given the nature of the cases and intervention, the program NGO and the authors considered it unethical to deny paralegal services to any 'poten-

[11] Indeed, neighboring Sierra Leone is at the time in the midst of a government-sponsored national expansion of a very similar program

[12] A second tier of the evaluation, launched in February 2009, involved community-level randomization aiming to assess longer-term community-level impacts, but the broader study is beyond the scope of this paper.

tial' client beyond a three-month period. Thus clients were randomly assigned to treatment and control groups, with those assigned to the control group ("control group applicants") guaranteed the paralegal's attention three months after first contact.

In addition, to ensure that the sample of clients was large enough for analysis and selected transparently and representatively from the community program, a few innovations had to be made in the way the paralegals dealt with clients. First, each paralegal was asked to work in only six of their ten communities in order to deal with the anticipated increase in cases. In each community, paralegals conducted a civic education session on salient aspects of the laws dealing with land law, gender-based violence, female inheritance, spousal and child neglect, etc. After this session, the paralegal met with potential clients and verified that the client actually had a case that needed resolving. Each client was then interviewed by an enumerator using a handheld survey device programmed to randomly allocate clients to treatment and control groups based on a pre-determined algorithm. Once clients were allocated, the paralegal explained to the control group applicants that her/his time was limited, and encouraged them clients to take whatever actions they deemed necessary to resolve their cases—apart from being a basic ethical consideration, this was key to avoiding anticipation or 'queuing' effects, which could have artificially lowered outcomes in the control group and upwardly biased our impact estimates. The data collected at this stage provided a baseline measure for the individual randomization.

Due to privacy considerations—which in this case had direct implications for the relationship between paralegals and clients, as there was a fear that were clients to discover that enumerators were interviewing the other party, they would opt out of treatment—as well as logistical constraints, we were not able to collect dyadic data as in previous sections. This is one main limitation of the data presented in this section, as we cannot measure the sum impact of the paralegal treatment on both parties to a dispute, nor can we discuss in depth the composition of the plaintiff-defendant pairs that the paralegals treated.

For the subsequent three-month period, till November 2011, paralegals conducted follow-up activities on treatment cases only. For ethical reasons, paralegals were also provided a 'veto' option to be used in serious or urgent cases, for example cases that involved an immediate threat of violence or serious economic or social harm. Such cases were excluded immediately from the baseline sample. While this affects the representativeness of the client sample, it is not immediately clear whether paralegals would have had greater or less impact on the vetoed cases, so the direction of bias is hard to estimate. In November 2011, follow-up surveys were administered to the baseline sample of clients. Of 420 clients surveyed in the baseline, we were able to locate and re-interview 398 clients, for an attrition rate of just over five percent.

Based on this experimental design, the most general empirical strategy that we use to estimate the impact of the intervention on mean outcomes is

$$y_i = \alpha_0 + \alpha_1 Z_i + \varepsilon_i \tag{11}$$

where y_i is the outcome for individual i, Z_i is the treatment dummy, and ε_i is the random error term, clustered at the village level.

For indicators for which we have both baseline and endline data, we exploit the panel structure of the data by looking at the impact of the treatment on a change in outcomes, using three common specifications: ANCOVA (Equation 12), difference-in-differences (Equation 13) and fixed effects (Equation 14).

$$y_{i1} = \alpha_0 + \alpha_1 Z_i + \alpha_2 y_{i0} + \upsilon_{i1} \tag{12}$$

$$y_{it} = \alpha_0 + \alpha_1 Z_i + \alpha_2 P_t + \alpha_3 (Z_i \times P_t) + \varphi_{it} \tag{13}$$

$$y_{it} = \alpha_0 + \alpha_1 Z_i + \alpha_2 P_t + \zeta_i + \xi_{it} \tag{14}$$

where y_{it} is the outcome at time $t \in (0, 1)$, and P_t is the post-treatment dummy.

5.3 Data

Table 6 provides a snapshot of dispute incidence and forum choice—notably, choice between the formal system, the customary system, and paralegals. Columns 1 and 2 show the number of disputes experienced in the past three months by type of dispute (including disputes reported both in the baseline and the endline) as well as the percent of disputes of each type. Columns 3–6 display forum shopping decisions for the most recent dispute respondents experienced during the three-month intervention period (including the original dispute they brought to the paralegal), and whether they took the dispute to the customary system, the formal system, or the paralegal, respectively. Disputes taken to more than one forum were counted as having gone to both forums. As noted above, this is a snapshot of potential paralegal clients, and therefore not representative of Liberians as a whole – comparing to the numbers displayed in Table 1, for example, there are a disproportionate number of "support" cases, a large proportion of which went to a paralegal.

The theoretical framework in Section 3 predicted simply that legal aid would yield positive welfare benefits for clients, and that these effects would be particularly strong for plaintiffs facing pro-defendant bias in the customary system. The survey data contains a wide range of variables that capture diverse elements of these welfare improvements. We group them into five categories:

Category 1. *Case outcomes*

Because the data spans a wide range of cases and case types, from commercial debt disputes to marital infidelity, we must rely on fairly broad survey measures of a positive case outcome. We rely on clients' own subjective evaluations of the case outcome. Note, however, that we focus on their evaluation of the case outcome, rather than a more customer-service oriented assessment of the pro bono service they received from the paralegal.

We use five measures of case outcome, based on the following survey questions.

- "Did you think the outcome was fair or unfair?"
- "How satisfied are you with the outcome of this case?"
- "After this case, do you think you are... [better or worse off]?"
- "After this case, is your relationship with the other party... [better or worse]?"
- "After this case, is your relationship with other members of the community... [better or worse]?"

Responses for the first two questions are measured on a four-tiered Likert scale, and the last three questions on a five-tiered Likert scale. While we analyze these five outcomes separately, they reflect the same underlying hypothesis.

Category 2. *Legal knowledge and experience of the legal system*

Our second set of outcome measures stems from the hypothesis that informal legal aid—delivered by semi-skilled paralegals rather than lawyers, working primarily outside the formal court system—will increase confidence in Liberia's formal legal system, by (a) improving knowledge of formal law and (b) lowering the informal costs of seeking justice (e.g. by reducing bribery and harassment).

To measure impacts on legal knowledge, we ask eight questions about the formal law developed in consultation with the Carter Center. Questions cover a range of issues, including inheritance rights, spousal abandonment, *sassywood*, domestic violence, statutory rape and corruption, such as "According to the formal law: Do married women have the right to inherit part of the property from their late father?", "According to the formal law: It is illegal for an adult to have sex with someone below a certain age. What is that age?", etc. We code each question into an indicator variable for a correct response, and generate an index using the first principal component from a factor analysis.

Finally, we also look at other objective measures of clients' experiences with the legal system, including the proportion who reported being harassed or forced to pay a bribe during the past three months. We anticipate that the particular form of legal aid being evaluated here will reduce reliance on formal legal institutions, while simultaneously reducing the incidence of these abuses such as harassment and bribe payment.

Category 3. *Pro-social attitudes and subjective happiness*

We hypothesize that better, fairer resolution of legal disputes will improve clients' subjective wellbeing and attitudes towards others. We measure impacts on three, admittedly disparate, measures of attitudes: subjective happiness, trust, and attitudes towards gender-based violence. The first two are measured, respectively, by responses to the following survey questions:

"When you think about your whole life, do you think you are. . . ?"
Responses: "Very happy"; "Happy small"; "Not so happy"; "Not happy at all".

"All in all, you can say that most people can be trusted, or that you need to be very careful when we doing things with people?"
Responses: "Most people can be trusted"; "Need to be very careful."

The third is measured by the principal component from a factor analysis of the five standard questions from the Demographic Health Survey questionnaire:

"Sometimes a man is annoyed or angered by things that his woman does. Do you think it is alright for a man to beat his woman if. . . "

- ". . . she goes out without telling him?
- ". . . she neglects the children?
- ". . . she argues with him?
- ". . . she refuses to have sex with him?
- ". . . she burns the food?"

Category 4. *Economic behavior, specifically related to investment and credit*

Disputes over debt, land, property, labor are common in Liberia (Table 1). We hypothesize that informal legal aid will assist individuals in engaging in the credit market and taking steps to protect their property rights. Specifically, we investigate impacts on ownership of documents proving title, new demarcation of landholdings, and frequency of borrowing and lending, measured by affirmative responses to the following questions:

- "What kind of document do you have to assure that the land is for you?"
- "The land you making farm on, they na survey it for your to know your boundary?"
- "In the last three months, any of your family people in this house na credit money to someone outside?"
- "In the last three months, anyone outside this household na credit money to your?"

Category 5. *Household income and wellbeing*

The underlying hypothesis here supposes that cases are relatively large in monetary terms, i.e., that the legal disputes brought to paralegals have significant economic consequences, and that paralegals can significantly alter their outcome. Note that because our sample is drawn from would-be clients who brought a case to the paralegal, we cannot speak to the question of whether legal disputes are sufficiently frequent to meaningfully affect the wellbeing of the population at large. Our focus here is on a population with self-assessed grievances and an interest in legal aid.

"Loving problems", usually referring to marital infidelity and often a loss of financial support, and "child neglect" constitute a large share of the cases in our database. We also examine the impact of the program on (a) child support payments received, and (b) child-specific food security. Child support payments are measured conditional on whether the respondent's household has at least one single mother with an absentee partner, and the response to the question "Does the father living outside regularly send food or money to care for the children living in this household?"

We focus on two types of food security, one related to households on average and one focused on children. Paralegals receive a large number of cases that relate directly to child welfare (child support, child custody, and support for wives or girlfriends), and we hypothesize that such cases would likely directly impact child welfare. Our measures are based on the Household Food Insecurity Access Scale(HFIAS) developed by the United States Agency for International Development USAID (2007) and the child nutrition module in the ERS's US Household Food Security Survey Module USDA (2012). Together, these consist of a battery of eighteen questions that focus on uncertain or anxiety over food access, perceptions that food is of insufficient quantity or quality, reported reductions in food intake, and reported consequences of reduced food intake. We combine these into two separate indices using principal components: an aggregate household measure of food insecurity focused on adults, using the HFIAS questions, and a child-specific measure using the ERS questions.

We also explore impacts on any increase in farmland under the household's control, with the question "In the last three months, the land your can make farm on, your na add some to it or your take some from it?"

Finally, we measure the incidence of self-reported gender-based violence in the past three months, based on responses to standard questions from the Demographic and Household Survey. We ask respondents about any of nine different types of violence of varying intensity, and create an aggregate measure using the principal component from a factor analysis.

Table 7 reports summary statistics for the full vector of impact outcome measures tested in this section, as well as two additional key features of the endline data: the attrition rate, and overall status of cases brought to the paralegals in the baseline. The five measures related to "case results", as well as the case status measure, were collected in the endline round only and are thus subjected to a cross-sectional analysis along the lines of Equation 11. The remaining measures are subjected to a full difference-in-differences analysis along the lines of Equation 13. Attrition is low, at 5 percent of the baseline sample.

Table 8 reports balance statistics for baseline levels of the main outcomes and household characteristics used in all subsequent analysis. Columns 1 and 2 display the average levels for treatment and control groups, respectively. Columns 3 and 4 show coefficients from a regression of the baseline observations for each variable on the treatment dummy. Standard errors are shown in parentheses. No baseline variables differ significantly between treatment and control groups. The second-last row of Table 8 reports attrition rates in both groups—again, these do not differ

significantly, and attrition is quite low at 5.4% (22 respondents). Nonetheless, given the relatively small number of respondents in the study, we will conduct various robustness tests to account for attrition bias.

5.4 Results

We begin by analyze take-up of legal aid, before turning to impacts on clients. The model in Section 3 (Prediction 5) implies that there should be high demand for legal aid among cases that would otherwise go to the formal sector or, barring that, not be reported to any forum whatsoever.

The individual randomization in our experiment provides a way to test this prediction. The control group consists of individuals who endogenously self-selected into legal aid, but were turned away. These "control group applicants" then faced the same forum-shopping choices as our representative sample of disputes analyzed above (customary, formal, or none). This control group was interviewed again three months later, at which point the vast majority had pursued their case in other forums. Thus the randomized evaluation provides a counterfactual not only for the outcomes of treated individuals, but also for their forum-shopping behavior.

We exploit this set up in Table 9 by re-estimating the multinomial logit model presented in Table 4, but pooling our earlier (representative) sample of individuals with the self-selected control group applicants. Our hypothesis is that control group applicants should, *ceteris paribus*, be more likely to go to the formal sector or, otherwise, not to report their cases at all.[13]

Indeed this is what we find: conditional on individual and dispute characteristics, demand for legal aid is highest among cases that otherwise would choose formal over customary forums, as well as cases that would otherwise not be reported whatsoever. This can be seen by the large, positive, and significant coefficients on the dummy variable for "control group applicants" in columns (1) and (2). This implies that legal aid not only provides an attractive alternative to going directly to the formal sector, but also increases the number of reported disputes by providing an outlet for grievances that otherwise would have gone nowhere.

Turning now to the estimation of treatment effects, Tables 10 and 11 display our main results. In line with Prediction 6, we expect treated individuals to experience better outcomes as a result of the treatment. Outcomes are grouped by hypothesis, and each row reports a separate regression specified either along the lines of Equations 11, 12, 13 and 14. Given the large number of outcome variables we consider, a key risk is over-rejection of the null hypothesis due to the problem of multiple inference (Anderson, 2008). We thus include a mean effects index in each hypothesis, calculated along the lines of (Kling et al., 2007), that generates a composite measure of the impacts for each group of outcomes under the hypothesis that offers a crude measure of whether the null hypothesis as a whole stands rejected. Yet even with a mean effects approach, we are still testing

[13]Note that the earlier multinominal logit in Table 4 uses characteristics of both plaintiffs and defendants. As noted in the text, we lack data on defendant characteristics for the experimental sample, so Table 9 uses only plaintiff and dispute characteristics as independent variables.

multiple hypotheses and so we implement the family-wise error rate correction procedure proposed by Bonferonni and Sidak (Abdi, 2007), both within each hypothesis and across the mean effects indices. These are discussed further in Section 5.7.

One possible concern with the design of the experiment would have been a kind of anticipation effect, where applicants for legal aid that were assigned to the control group deliberately withheld making progress on their cases while waiting for the paralegal to intervene. Such "control group applicants" could then have looked worse off than otherwise, which would have biased treatment effects upwards. The first row of Table 10 provides some reassurance: there was no significant difference in case progression, in terms of average level of response to the question "What is the status of this case now?" with the options "Case pending, no agreement reached yet", "Unable to reach agreement or resolution", "Reached agreement, yet to be implemented" and "Reached agreement, successfully implemented". There is also no evidence of any difference in the average respondent's relationship with the community.

The remainder of Table 10 shows that paralegal clients were overall much happier with the outcome of the case: relative to the control group, our measure of fairness went up by 34.8 percent, satisfaction by 37 percent, whether the client considered themselves better off by 26.7 percent, and whether the relationship with the other party was now better by 23.3 percent. The mean effects index is strongly significant at the 5% level.

Turning to those outcomes for which we have panel data, we provide the results all four specifications listed above in Table 11. We find, firstly, convincing evidence that the quality of interaction with the overall justice system has changed—treated respondents reported a 10 percent decrease in having to pay a bribe to a police officer or public official, suggesting that paralegal involvement lowers the corruption costs of accessing justice. There was however no accompanying impact on harassment by public officials. Treatment also strongly impacted legal knowledge, which is measured as the first principal component of eight questions regarding respondents' knowledge of the formal law. The PCA of treated respondents' knowledge of the law improved significantly by 0.31 standard deviations over the course of the three months of interaction with the paralegal.

Moving further down the table, we find no impacts on any of our three measures of attitudes—generalized trust, subjective happiness, and attitudes towards gender-based violence. Neither do we find any impacts on behavior related to actions taken to protect property rights (land titling and demarcation) or engage in credit market activity (lending and borrowing). It is somewhat striking that there is absolutely no hint of an impact on any of these measures, suggesting by implication that any downstream impacts on household wellbeing do not come from changes in attitudes, credit market behavior, or greater security of property.

The intervention does, however, show significant downstream impacts on household wellbeing—in particular, on three measures: household food security, child food security, and proportion of households with single mothers receiving child support payments from absentee fathers. Clients were 22.8 percent more likely to receive child support payments, and reported large increases

household and child food security of 0.24 and 0.38 standard deviations, respectively, as measured by our aggregate indices. The intervention did not appear to have any impact on the remaining two measures of household wellbeing: the amount of land respondents farmed on, and the incidence of gender-based violence.

Figure 6 summarizes the key outcomes. The vertical axis displays all the main outcome indicators, and the horizontal axis measures the size of the impact and the precision of the estimate. All outcomes are normalized by subtracting the mean and dividing by the standard deviation, to provide comparable impact measures—thus the horizontal axis simply measures the standard effect size, i.e. the number of standard deviations of impact. Circles provide the point estimate, while the length of the line displays a 90% confidence interval around the point estimate. Thus if a line crosses the vertical origin, the estimate is not statistically different from zero. Statistically significant outcomes are presented in red and insignificant outcomes in blue.

5.5 Case interactions

To further investigate the mechanism underlying the effects we observe, we interact our vector of outcome variables with the disputes brought to the paralegal by each respondent during the three months of the intervention period. This provides some indication of whether a given downstream impact was associated with taking a relevant case to the paralegal. Thus for each outcome and case type, we run the following specification:

$$y_{it} = \alpha_0 + \alpha_1 Z_i + \alpha_2 P_t + \alpha_3 \delta_i + \alpha_4 (Z_i \times P_t) + \alpha_5 (Z_i \times \delta_i) + \alpha_6 (P_t \times \delta_i) + \alpha_7 (Z_i \times P_i \times \delta_i) + \varsigma_{it} \quad (15)$$

where y_{it} is the outcome for individual i in period t, $Z_i \in [0, 1]$ is a dummy indicating treatment for individual i, $P_t \in [0, 1]$ is the post-treatment dummy, and δ_i is a measure of whether individual i experienced at least one dispute of a given category during the intervention period.

For the sake of parsimony, we examine three categories of cases: "family case" (wife or child neglect, 'loving problems', and child custody disputes), "violence case" (rape, domestic violence, and generalized assault), and "economic case" (disputes related to land, labor, property, and debt). Table 12 presents the results. The leftmost column lists dependent variables grouped by hypothesis. Each element in columns 1-4 is the coefficient on $\delta_i \times Z_i \times P_t$ in a separate regression on each given case category, following the specification in Equation 15 above.

We find that case outcomes are generally linked to whether the individual took a relevant type of case to the paralegal. For example, although the coefficient on acquiring land title is insignificant on average, it is positive and strongly significant for individuals who brought an economic case to the paralegal—and this effect is driven entirely by those with land cases. Similarly, household and child food security increase differentially for individuals who brought either an economic case or a family case (involving spousal or child support or custody) to the paralegal. Gender-based

violence goes down differentially for individuals who brought a violence case (involving assault, rape, or domestic violence), but also for those who brought neglect or support cases. Child support payments are strongly associated both with taking a family case or an economic case to the paralegal. These patterns are consistent with the mechanism that taking the paralegal's direct assistance in resolving the dispute improved the client's outcomes relevant to that case type.

5.6 Heterogeneous effects

The first half of this paper made a case for the paralegal intervention as a solution to the constrained forum choices of the socially disadvantaged—unable to afford the formal system but discriminated against under the custom, they are the paralegals' natural clients. We investigate heterogeneous treatment effects suggested by the theoretical model (Prediction 7) for six characteristics: women, subsistence farmers, ethnic minorities, individuals with no primary education, migrants, and refugees. Given the large number of categories and the much larger number of dependent variables, we choose to restrict ourselves to the mean effects index for each hypothesis so as to try and restrict the risk of Type 1 errors. Thus for the first hypothesis, we run the cross-sectional specification:

$$y_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 \beta_i + \alpha_3 (Z_i \times \beta_i) + \nu_i \tag{16}$$

and for the remaining four hypotheses we run the difference-in-differences specification:

$$y_{it} = \alpha_0 + \alpha_1 Z_i + \alpha_2 P_t + \alpha_3 \beta_i + \alpha_4 (Z_i \times P_t) + \alpha_5 (Z_i \times \beta_i) + \alpha_6 (P_t \times \delta_i) + \alpha_7 (Z_i \times P_i \times \beta_i) + \sigma_{it} \tag{17}$$

Table 13 reports the results, which provide mixed but nonetheless suggestive evidence that the treatment effect is driven by differential impacts on women, subsistence farmers, and ethnic minorities. First, the impacts on case results, experiences with the justice system, and household wellbeing is more precisely estimated for each of these subgroups (columns 2, 5 and 8), and in general the coefficients on the interaction terms are large and positive, and in some cases significant. So for example, female clients experience significantly positive treatment effects on case results and in their interaction with the justice system, while male clients do not (the second and third column). Furthermore, the effect on female clients is significantly greater than males (column 3). While these results are in all likelihood held back by the relatively small sample size, they suggest that even within the population that has selected into paralegal treatment, the impacts are distributed in favor of those whose rights are most limited under customary law.

5.7 Robustness

Given our large vector of outcome variables, we need to address the risk of over-rejecting the null hypothesis due to the problem of multiple inference, which arises when testing multiple hypotheses simultaneously (Anderson, 2008). We therefore implement a multiple comparisons correction using the procedure proposed by Bonferonni and Sidak to Abdi (2007) to control the family-wise error rate (FWER), defined as the probability of making any false discovery. These tests are generally considered overly conservative.

We conduct the multiple comparisons correction across the mean effects indices for those hypothesis that were significant, as well as on the variables within each hypothesis. Figure 7 presents the results for each correction. The horizontal axis in each panel plots the parameter estimate, while the vertical axis plots the p-value. The horizontal red lines indicate the cut-off for 90% significance ($p = 0.10$) as well as the 'critical' or corrected p-value that reflects the Bonferonni-Sidak corrections. Outcome variables above both the red lines indicate p-values that survive the correction procedure. Figure 7 shows, somewhat remarkably, that all our significant mean effects indices and the majority of our individual outcome measures survive the corrections tests.

Finally, we address the issue of attrition. In principle this type of intervention could easily generate differential attrition, if for example individuals with poor justice experiences become less inclined to speak to outsiders. On the face of it, however, we have no reason to be concerned—Table 7 showed that the attrition rate was only around five percent, and Table 8 showed that it was balanced across treatment and control groups. However, given our relatively small experimental sample, even a five percent rate could be significant, and it is conceivable that different types of people attrited out from treatment and control groups.

We therefore conduct a difference-in-differences analysis between attritors and non-attritors, across treatment and control groups. Table 14 presents the difference in outcomes and household characteristics between those who attrited and those who remained. Column 1 displays the baseline difference in means between attritors and non-attritors in the treatment group, for each outcome variable collected in the baseline. Column 2 displays the corresponding values for the control group while Column 3 reports the $Z \times \Lambda$ interaction coefficient from the following regression:

$$y_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 \Lambda_i + \alpha_3 (Z_i \times \Lambda) + \theta_i \tag{18}$$

Column 4 shows robust standard errors clustered at the village level. The results show that individuals with demarcated land and those more likely to borrow were more likely to exit the sample. Both results are significant at the 10 percent level. None of the other variables differ significantly between treatment and control, and in all the results appear to be reasonably resilient to attrition effects.

6 Conclusions

We began with the question of whether progressive, statutory legal reform could be made to meaningfully affect the lives of the poor and socially disadvantaged, given that they tend to remain outside the ambit of formal law and rely on customary institutions for their justice needs. In the first part of this paper, we attempted to explicate the underlying reasons by developing a simple model of forum choice that asks the poor to choose between a repressive custom and a punitive, costly formal system. We built the model relying on extensive contemporary anthropological data, and tested it using new household survey data on over 4,500 legal disputes in rural Liberia. Consistent with the model, we found empirical evidence that plaintiffs facing a disadvantageous pairing under the custom were more likely to opt for the formal system, but that customary remedies were more efficient in redistributing resources between plaintiffs and defendants. Thus our analytical and empirical results suggested a clear avenue for legal empowerment—to provide the poor access to low-cost, remedial justice that incorporates the progressive features of the formal law.

We then presented the results of a randomized controlled trial of a legal empowerment intervention in Liberia designed around these same basic principles, providing *pro bono* mediation and advocacy services through community paralegals trained in the formal law. We found strong and robust impacts on justice outcomes, as well as significant downstream welfare benefits—including increases in household and child food security of 0.24 and 0.38 standard deviations, respectively. We find no impacts on attitudes or behavior, but strong impacts associated with taking a case to the paralegals, suggesting that at least in the short term paralegals provide a directly redistributive role. Our experimental results are highly robust to different specifications, mean effects analyses, multiple comparisons corrections, and attrition.

Overall, we interpret these results as preliminary evidence that legal empowerment interventions aimed at improving access to justice and reviving dead letter laws can produce large socioeconomic benefits. Moreover, our results suggest that these gains can be achieved not by bringing the rural poor into the formal domain of magistrates' courts, government offices, and police stations, but by bringing the formal law into the organizational forms of the custom, through low-cost third-party mediation and advocacy.

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Table 1: Where do disputes go?

			% of all cases taken to:		
	Cases	%	None	Customary	Formal
Family dispute	728	15.9	61.1	37.5	1.4
Economic dispute	2676	58.4	60.1	36.3	3.7
Land	339	7.4	37.8	56	6.2
Debt	1374	30	69.9	28.6	1.5
Labor	125	2.7	61.6	38.4	0.0
Property (incl. theft)	838	18.3	52.9	40.5	6.7
Violent dispute	712	15.5	52.1	40.3	7.6
Assault	561	12.2	53.8	42.8	3.4
Rape/GBV	85	1.9	47.1	31.7	21.2
Murder	66	1.4	43.9	30.3	25.8
Other disputes	470	10.2	52.1	43.8	4.0
Total	4,586		58.2	37.9	3.9

Note: Columns 1 and 2 display the number and relative proportion of disputes of different types faced by the 2,081 households in our household survey sample. Columns 3–5 show the percentage of disputes of each type that went to "No forum", "Customary", and "Formal", respectively.

Table 2: Who uses the customary system?

			% of all cases taken to		
Plaintiff		#	No forum	Customary	Formal
Gender	Female	939	55.4	41.5	3.1
	Male	3,647	59.0	36.9	4.1
Occupation	Farmer	4,128	58.2	38.2	3.6
	Non-farmer	458	58.3	35.2	6.6
Ethnicity	Minority	501	55.7	37.9	6.4
	Majority	4,085	58.5	37.8	3.6
Kinship	No	3,721	56.1	40.0	3.9
	Yes	865	67.4	28.7	3.9
Total		4,586	58.2	37.9	3.9

Note: Column 1 displays the total number of disputes faced by plaintiffs with particular characteristics across the 2,081 households in our household survey sample. Columns 2–4 show the percentage of disputes that plaintiffs of each type took to "No forum", "Customary", and "Formal", respectively.

Table 3: Subjective satisfaction measures

	Customary	Formal
Outcome was fair	92.3	85.0
Outcome was in respondent's favor	70.3	59.0
Satisfied with outcome	89.3	78.2
Satisfied with respect shown	89.2	75.7
Would return to this forum	90.5	76.4
First principal component	0.315	-0.243

Note: Columns 1 and 2 present respondents' average levels of subjective satisfaction for disputes taken to "Customary", and "Formal", respectively, across the 2,081 households in our household survey sample

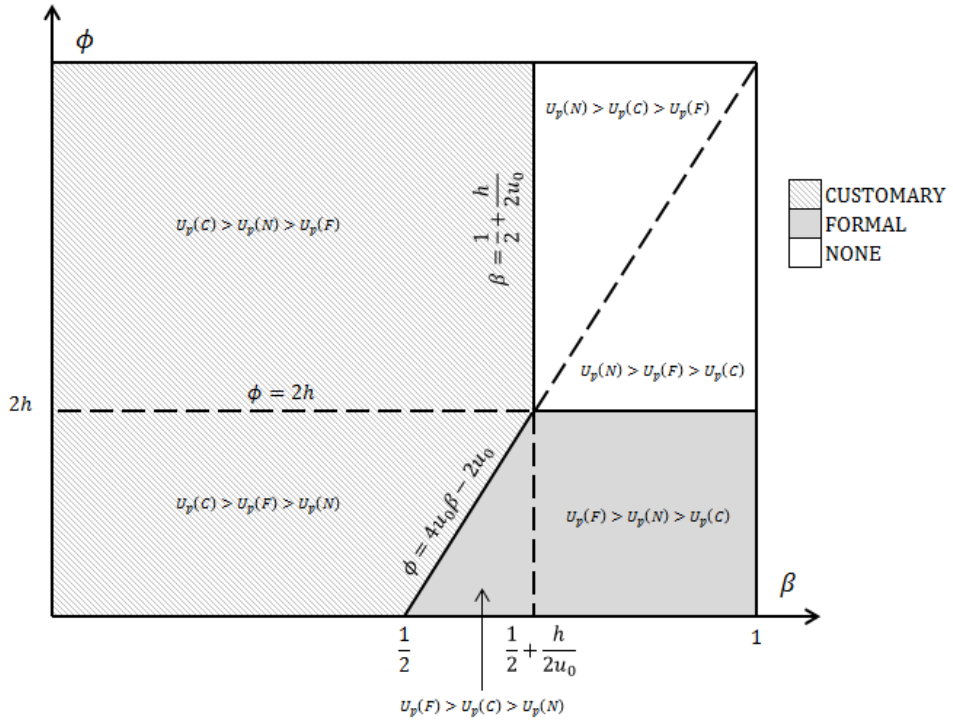


Figure 3: Plaintiff chooses forum (stage 2)

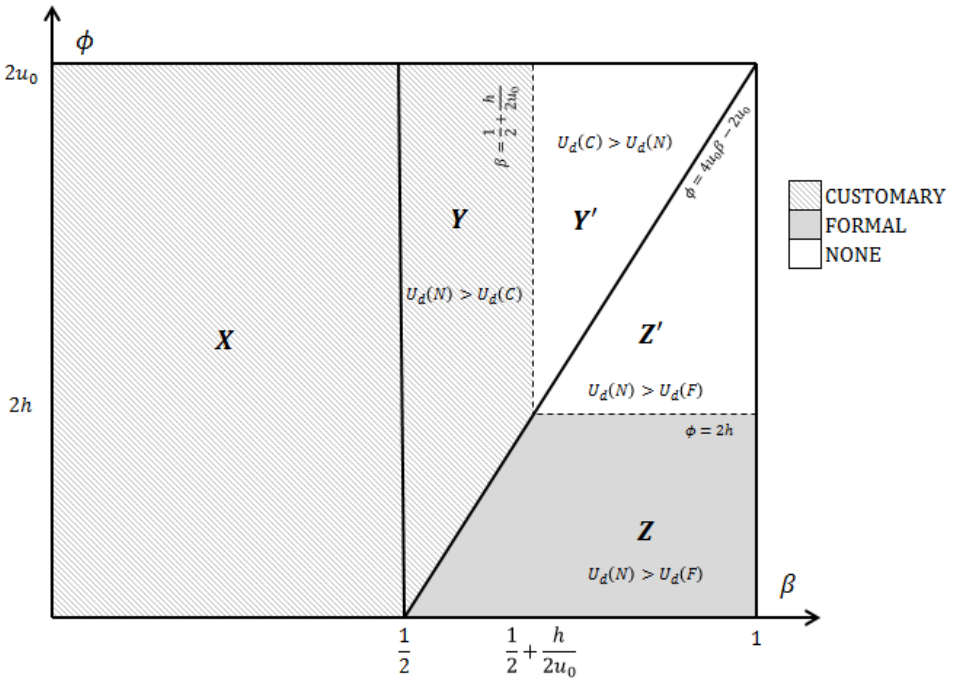


Figure 4: Defendant chooses harm (stage 1)

Figure 5: Map of surveyed counties

Table 4: Plaintiffs facing high β (Prediction 2) or with high u_0 (Prediction 3) exit the customary system

	Formal over Customary	None over Customary	Formal over None
Defendant - Plaintiff (β)			
	(+)	(+)	(?)
Gender bias	1.111***	-0.276***	1.387***
	(0.262)	(0.074)	(0.261)
Income bias	0.550**	0.268**	0.282**
	(0.201)	(0.103)	(0.199)
Elite bias	0.400	0.040	0.360
	(0.197)	(0.119)	(0.203)
Ethnic bias	0.092	-0.015	0.107
	(0.205)	(0.085)	(0.204)
Plaintiff ($u0$)			
	(+)	(+)	(?)
Male	1.446***	-0.195***	1.641***
	(0.318)	(0.107)	(0.318)
Non-farm employment	1.050	0.343	0.708
	(0.271)	(0.142)	(0.269)
Related to chief	0.732**	0.365**	0.367**
	(0.249)	(0.135)	(0.252)
Ethnic majority	-0.191	0.009	-0.201
	(0.239)	(0.114)	(0.240)
Dispute type			
Economic dispute	1.703	-0.012	1.715
	(0.339)	(0.111)	(0.340)
Violent dispute	1.106	0.066	1.040
	(0.323)	(0.087)	(0.323)
Other dispute	0.856*	-0.288*	1.144*
	(0.486)	(0.176)	(0.493)

Note: Coefficients displayed for each pair of choices from a single multinomial logit regression on the categorical variable of forum choice ("None", "Customary", "Formal"). β represents the gap between the defendant and plaintiff values ($x_D - x_P$) for the characteristic listed in the column heading. The omitted dispute category is "Family dispute". Specification includes a dummy for whether the respondent was the plaintiff or defendant. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 5: The customary system provides greater aggregate welfare (Prediction 4)

	Benchmark	Male	Non-farm employment	Related to chief	Ethnic majority
Plaintiff	.43	.45	.43	.44	.40
	(.12)***	(.12)***	(.12)***	(.12)***	(.12)***
Plaintiff $\times\beta$		.11	.08	.10	-.25
		(.09)	(.17)	(.13)	(.12)**
Plaintiff × Formal	-.16	-.25	-.15	-.28	-.12
	(.21)	(.21)	(.21)	(.25)	(.21)
Plaintiff × Formal $\times\beta$		1.68	-.29	-.40	.54
		(.56)***	(.51)	(.41)	(.65)
Defendant	.51	.53	.51	.60	.52
	(.16)***	(.16)***	(.16)***	(.17)***	(.16)***
Defendant $\times\beta$		-.03	-.11	-.34	-.10
		(.18)	(.28)	(.23)	(.29)
Defendant × Formal	-1.13	-1.17	-1.14	-1.20	-.91
	(.28)***	(.28)***	(.28)***	(.32)***	(.30)***
Defendant × Formal $\times\beta$		-1.15	-.05	.22	-1.36
		(.57)**	(.52)	(.54)	(.75)*
Observations	940	940	940	940	940
Adj. R^2	0.070	0.082	0.067	0.075	0.070

Note: Coefficients correspond to an OLS regression where the dependent variable is 'utility', i.e., the first principal component of the subjective justice metrics, as described in the text. β represents the gap between the defendant and plaintiff values ($x_D - x_P$) for the characteristic listed in the column heading. Specification includes dispute-type dummies. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 6: Forum shopping in experimental sample

	All disputes		Most recent dispute			
	Any (%)	Paralegal (%)	Unreported (%)	Formal (%)	Customary (%)	Paralegal (%)
Family case	42.8	32.2	12.3	2.0	23.2	13.9
Child neglect	29.4	23.7	6.7	1.4	12.6	12.1
Child custody	10.8	8.2	2.3	0.5	4.8	3.4
'Loving problem'	20.5	11.9	5.4	0.3	12.3	3.5
Violence case	23.6	6.0	11.8	0.9	9.0	0.9
Assault	6.3	1.8	2.3	0.4	3.1	0.5
Rape	1.8	0.5	1.0	0.5	0.3	0.0
Domestic violence	19.8	5.0	9.4	0.1	7.3	0.4
Land	14.3	8.9	2.9	0.6	6.4	5.5
Economic case	41.2	22.6	17.0	2.6	20.1	9.3
Labor	7.0	3.4	3.5	0.3	1.4	2.1
Property	10.9	6.4	4.1	0.1	6.5	1.1
Debt	19.8	6.9	8.2	1.6	9.7	1.6

Note: Columns 1 and 2 display the number and relative proportion of disputes of different types faced by the 420 households in our experimental sample. Columns 4–6 show the percentage of the most recent disputes of each type that went unreported, or were reported to the customary system, the formal system, and the paralegal, respectively. Disputes taken to more than one forum are counted as having gone to both forums.

Table 7: Summary statistics for experimental sample

	Observations	Mean	Std. Dev.	Min.	Max.
Case results					
Fair judgment	348	1.85	1.20	0.00	3.00
Satisfied	357	1.79	1.19	0.00	3.00
Better off	356	2.99	1.12	0.00	4.00
Other party relations	355	2.94	1.14	0.00	4.00
Community relations	357	3.16	1.04	0.00	4.00
Justice system					
Legal knowledge	768	0.00	1.00	-3.20	1.04
Not harassed	795	0.98	0.14	0.00	1.00
Did not bribe	789	0.93	0.26	0.00	1.00
Attitudes					
Oppose GBV	778	0.00	1.00	-5.35	0.44
Happiness	794	1.55	0.98	0.00	3.00
Trust	794	0.10	0.31	0.00	1.00
Behavior					
Land papers	645	0.26	0.44	0.00	1.00
Land demarcated	616	0.26	0.44	0.00	1.00
Lending	796	0.25	0.43	0.00	1.00
Borrowing	796	0.35	0.48	0.00	1.00
Household wellbeing					
HH food security	731	0.00	1.00	-1.35	2.36
Child food security	672	0.00	1.00	-1.37	2.16
Land gained	644	0.04	0.48	-1.00	1.00
Child support	233	0.23	0.42	0.00	1.00
Less GBV	753	0.00	1.00	-7.54	0.30
Attrition rate					
Respondent attrited	420	0.05	0.22	0.00	1.00

Note: Columns 1–5 present summary statistics for the core outcome measures and attrition for the 420 households in our individual survey sample.

Table 8: Balance in experimental sample

	Treatment	Control	Difference	Std. Err.
Justice system				
Legal knowledge	0.08	-0.04	0.115	(0.104)
Not harassed	0.99	0.98	0.012	(0.013)
Did not bribe	0.93	0.92	0.006	(0.027)
Attitudes				
Oppose GBV	-0.09	-0.05	-0.039	(0.110)
Happiness	1.23	1.32	-0.093	(0.096)
Trust	0.11	0.13	-0.024	(0.033)
Behavior				
Land papers	0.21	0.27	-0.062	(0.049)
Land demarcated	0.32	0.33	-0.006	(0.056)
Lending	0.31	0.29	0.029	(0.046)
Borrowing	0.39	0.37	0.018	(0.049)
Household wellbeing				
HH food security	-0.14	-0.13	-0.010	(0.101)
Child food security	0.01	-0.09	0.097	(0.103)
Land gained	0.01	0.06	-0.056	(0.060)
Child support	0.17	0.13	0.050	(0.064)
Less GBV	-0.17	-0.12	-0.050	(0.132)
Attrition rate				
Respondent attrited	0.05	0.05	-0.001	(0.022)

Note: Columns 1 and 2 present the treatment and control means for all outcome variables with baseline data, as well as mean attrition rates for each group. Columns 3 and 4 report the coefficient and standard error, respectively, from a t-test of each variable across the paralegal treatment group.

Table 9: Latent demand for formal law (Prediction 5)

	Formal over Customary	None over Customary	Formal over None
Plaintiff ($u0$)			
Male	0.004	0.040	-0.037
	(0.142)	(0.069)	(0.139)
Non-farm employment	0.483	0.059	0.424
	(0.171)	(0.095)	(0.165)
Ethnic majority	-0.160	-0.029	-0.131
	(0.179)	(0.089)	(0.176)
Take-up	(+)	(+)	(?)
Control group applicants	3.573***	0.599***	2.974***
	(0.226)	(0.180)	(0.183)
Dispute type			
Economic dispute	0.795	0.115	0.680
	(0.219)	(0.094)	(0.215)
Violent dispute	0.745***	0.513***	0.232***
	(0.203)	(0.083)	(0.199)
Other dispute	0.449	0.165	0.284
	(0.256)	(0.151)	(0.240)

Note: Coefficients displayed for each pair of choices from a multinomial logit regression on the categorical variable of forum choice ("None", "Customary", "Formal"). "Control group applicants" indicates that the respondent opted into the paralegal intervention, but was assigned to the control group. The omitted dispute category is "Family dispute". Specification includes a dummy for whether the respondent was the plaintiff or defendant. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Figure 6: Treatment effect estimates (Prediction 6)

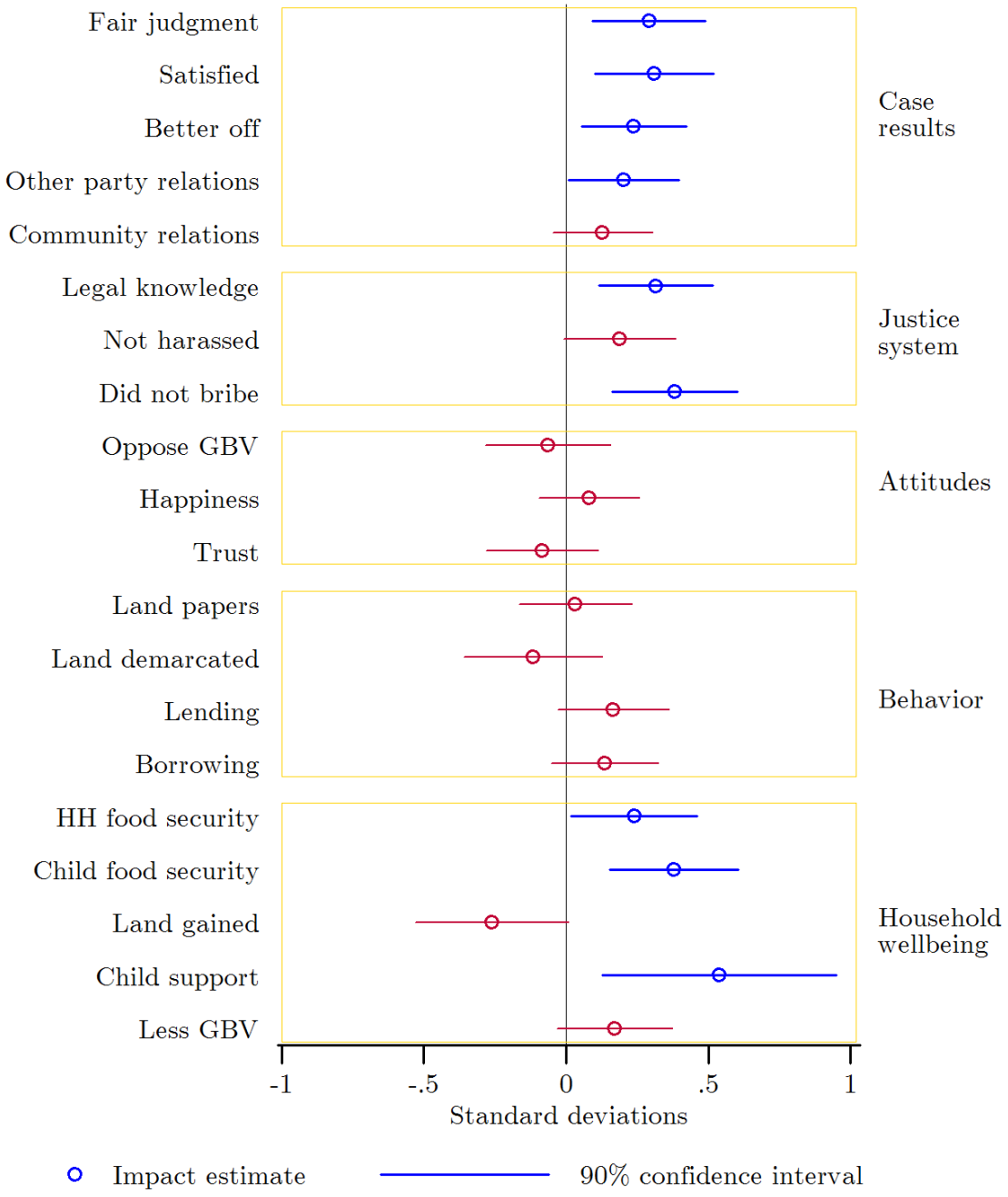


Table 10: Case results (Prediction 6)

	Coeff.	Std. Err.
Case results		
Fair judgment	0.348**	(0.142)
Satisfied	0.370**	(0.149)
Better off	0.267**	(0.123)
Other party relations	0.233*	(0.133)
Community relations	0.134	(0.110)
Mean effect index	0.219**	(0.092)

Note: Each row reports the coeff. and std. error for Z in a separate regression based on Equation 11, where $Z \in [0, 1]$ indicates treatment. Std. errors are clustered at the village level. ***, ** and * denote significance at 1%, 5% level and 10%, respectively.

Table 11: Treatment effect estimates (Prediction 6)

	Cross-section		ANCOVA		Diff-in-diff.		Fixed effects	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Justice system								
Legal knowledge	0.190**	(0.090)	0.231**	(0.094)	0.316***	(0.119)	0.323***	(0.117)
Not harassed	0.015	(0.013)	0.017	(0.013)	0.027	(0.017)	0.028	(0.018)
Did not bribe	0.094***	(0.028)	0.089***	(0.028)	0.100***	(0.035)	0.095***	(0.035)
Mean effect index	0.184***	(0.046)	0.208***	(0.046)	0.275***	(0.073)	0.281***	(0.073)
Attitudes								
Oppose GBV	−0.058	(0.094)	−0.069	(0.099)	−0.068	(0.133)	−0.081	(0.136)
Happiness	0.163**	(0.074)	0.147**	(0.072)	0.085	(0.104)	0.095	(0.104)
Trust	−0.004	(0.025)	−0.011	(0.025)	−0.026	(0.036)	−0.026	(0.036)
Mean effect index	0.028	(0.059)	0.011	(0.057)	−0.026	(0.068)	−0.027	(0.068)
Behavior								
Land papers	0.079	(0.052)	0.034	(0.051)	0.002	(0.053)	−0.028	(0.058)
Land demarcated	−0.056	(0.052)	−0.073	(0.054)	−0.058	(0.063)	−0.077	(0.063)
Lending	0.046	(0.036)	0.060*	(0.036)	0.074	(0.051)	0.081	(0.052)
Borrowing	0.041	(0.047)	0.059	(0.047)	0.068	(0.054)	0.075	(0.055)
Mean effect index	0.066	(0.057)	0.067	(0.056)	0.056	(0.063)	0.060	(0.064)
Household wellbeing								
HH food security	0.260**	(0.119)	0.292**	(0.114)	0.260**	(0.130)	0.299**	(0.127)
Child food security	0.296**	(0.127)	0.273**	(0.127)	0.365***	(0.133)	0.337**	(0.141)
Land gained	−0.072	(0.050)	−0.051	(0.063)	−0.123	(0.078)	−0.102	(0.086)
Child support	0.179*	(0.094)	0.220*	(0.109)	0.243**	(0.103)	0.352**	(0.141)
Less GBV	0.226***	(0.072)	0.211***	(0.061)	0.171	(0.125)	0.157	(0.146)
Mean effect index	0.218***	(0.047)	0.218***	(0.046)	0.229***	(0.052)	0.226***	(0.053)

Note: Each row reports four regression specifications on the listed outcome variable. The leftmost column lists dependent variables grouped by hypothesis. The remaining columns display the coefficient and standard error on Z and $Z \times P$ as appropriate, where $Z \in [0, 1]$ indicates treatment and $P \in [0, 1]$ indicates the post-intervention period. The second and third columns present the cross-sectional specification (Equation 11), the fourth and fifth present the ANCOVA specification (Equation (12), the sixth and seventh report difference-in-differences (Equation 13), and the last two report the fixed effects specification (Equation 14). Standard errors are clustered at the village level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 12: Case interactions

	Family case		Economic case		Violence case	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Justice system						
Legal knowledge	-0.011	(0.282)	-0.026	(0.254)	-0.200	(0.295)
Not harassed	0.043	(0.034)	-0.036	(0.036)	0.051	(0.057)
Did not bribe	0.256***	(0.058)	-0.098	(0.064)	0.030	(0.092)
Mean effect index	0.373**	(0.148)	-0.185	(0.152)	0.004	(0.179)
Attitudes						
Oppose GBV	-0.081	(0.178)	-0.228	(0.183)	0.422	(0.335)
Happiness	0.382*	(0.213)	-0.012	(0.194)	0.206	(0.261)
Trust	0.028	(0.072)	0.056	(0.061)	0.056	(0.130)
Mean effect index	0.122	(0.131)	-0.016	(0.126)	0.268	(0.210)
Behavior						
Land papers	0.195	(0.117)	0.074	(0.132)	0.136	(0.181)
Land demarcated	0.154	(0.125)	-0.338***	(0.104)	0.191	(0.180)
Lending	0.008	(0.099)	0.005	(0.076)	0.308**	(0.152)
Borrowing	0.236**	(0.103)	-0.059	(0.117)	0.331*	(0.174)
Mean effect index	0.275**	(0.113)	-0.151	(0.122)	0.508***	(0.161)
Household wellbeing						
HH food security	0.484*	(0.276)	0.041	(0.268)	0.231	(0.378)
Child food security	0.706**	(0.270)	-0.158	(0.270)	-0.054	(0.402)
Land gained	-0.097	(0.134)	-0.152	(0.123)	-0.198	(0.240)
Child support	0.578**	(0.228)	0.093	(0.186)	0.518**	(0.250)
Less GBV	0.525**	(0.228)	-0.487***	(0.175)	2.861***	(0.919)
Mean effect index	0.333***	(0.116)	-0.151	(0.106)	0.470**	(0.210)

Note: The leftmost column lists dependent variables grouped by hypothesis. Rows report interactions between the dependent variable and three categories of case type: "Family case" (wife or child neglect, 'loving problems', and child custody), "Economic case" (land, labor, property, and debt), and "Violence case" (rape, domestic violence, and assault). Each element in columns 1-4 is the coefficient on $\delta_i \times Z_i \times P_t$ in a separate regression on each case category δ, following the specification in Equation 15. Standard errors are clustered at the village level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 13: Heterogeneous treatment effects (Prediction 7)

	Sex			Occupation			Ethnicity		
	Male	Female	Diff.	Non-farmer	Farmer	Diff.	Majority	Minority	Diff.
Case results	-0.025	0.298***	0.324*	0.080	0.234**	0.155	-0.056	0.394***	0.450***
	(0.154)	(0.109)	(0.184)	(0.286)	(0.094)	(0.295)	(0.123)	(0.120)	(0.164)
Justice system	0.059	0.344***	0.285**	0.406***	0.272***	-0.134	0.079	0.415***	0.336**
	(0.068)	(0.097)	(0.132)	(0.148)	(0.081)	(0.173)	(0.063)	(0.120)	(0.144)
Attitudes	0.016	-0.047	-0.063	-0.191	-0.005	0.186	-0.044	0.009	0.053
	(0.112)	(0.087)	(0.151)	(0.164)	(0.078)	(0.195)	(0.082)	(0.105)	(0.134)
Behavior	-0.059	0.097	0.157	0.212	0.044	-0.169	-0.030	0.101	0.131
	(0.110)	(0.075)	(0.129)	(0.174)	(0.070)	(0.194)	(0.085)	(0.094)	(0.127)
Household wellbeing	0.257**	0.221***	-0.036	-0.121	0.259***	0.380**	0.302***	0.192***	-0.110
	(0.108)	(0.057)	(0.115)	(0.170)	(0.057)	(0.183)	(0.086)	(0.071)	(0.113)

Note: The leftmost column lists the dependent variable, which is the mean effects index for each hypothesis. Rows report interactions between the dependent variable and the plaintiff's sex, occupation, and ethnicity. Columns 2 and 3 present the coefficients for males and females, respectively, while column 4 presents the coefficient on $\beta_i \times Z_i \times P_t$, where β is sex, following the specifications in Equations 16 and 17. Columns 5-10 repeat this exercise for occupation and whether or not the respondent is a member of the dominant ethnic group. Standard errors are clustered at the village level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

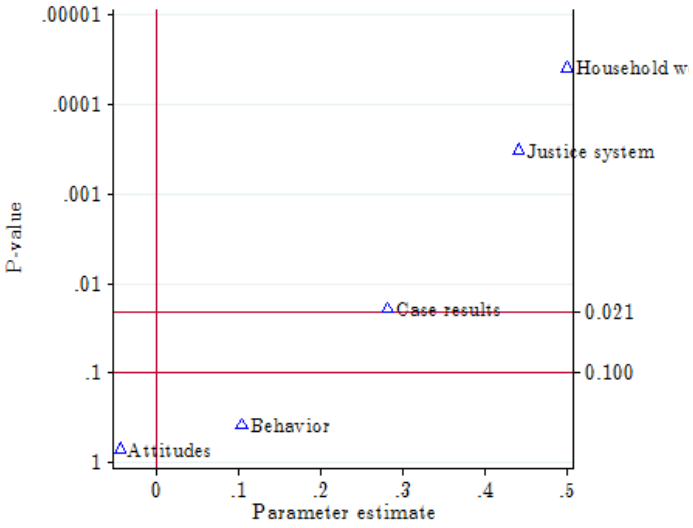
Table 14: Attrition

	Attritors minus non-attritors			
	Treatment	Control	Difference	Std. Err.
Justice system				
Legal knowledge	0.140	-0.103	0.243	(0.439)
Not harassed	0.024	0.011	0.013	(0.014)
Did not bribe	0.083	0.076	0.007	(0.027)
Attitudes				
Oppose GBV	0.510	-0.165	0.675	(0.434)
Happiness	0.142	-0.087	0.229	(0.391)
Trust	-0.046	-0.115	0.069	(0.095)
Behavior				
Land papers	-0.052	-0.011	-0.042	(0.238)
Land demarcated	-0.045	-0.336	0.291*	(0.167)
Lending	-0.205	-0.179	-0.026	(0.181)
Borrowing	-0.197	-0.402	0.205*	(0.105)
Household wellbeing				
HH food security	-0.149	0.045	-0.194	(0.357)
Child food security	-0.281	-0.058	-0.223	(0.728)
Land gained	0.051	-0.008	0.058	(0.324)
Child support	-0.131	-0.180	0.049	(0.078)
Less GBV	0.329	0.495	-0.166	(0.145)

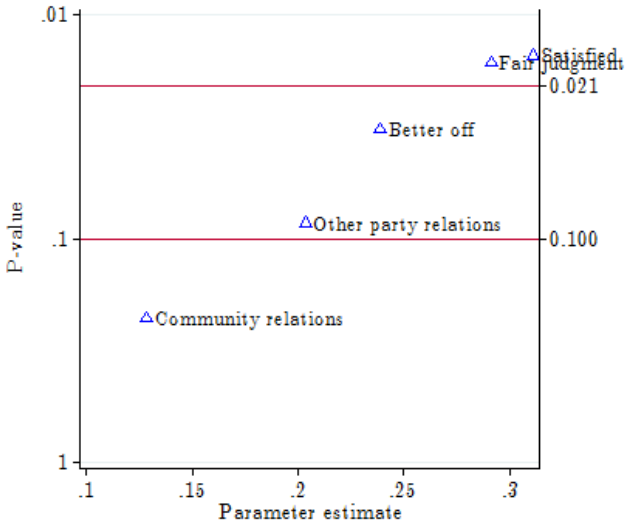
Note: Columns 1 and 2 report differences between the mean of each variable for attritors and non-attritors in the treatment and control groups, respectively. Column 3 reports the coefficient on the $Z \times A$ interaction, where $Z \in [0, 1]$ is the treatment dummy and $A \in [0, 1]$ is a dummy for whether the respondent attrited. Column 4 displays robust standard errors clustered at the village level.

Figure 7: Bonferroni-Sidak Family-Wise Error Rate (FWER) Correction

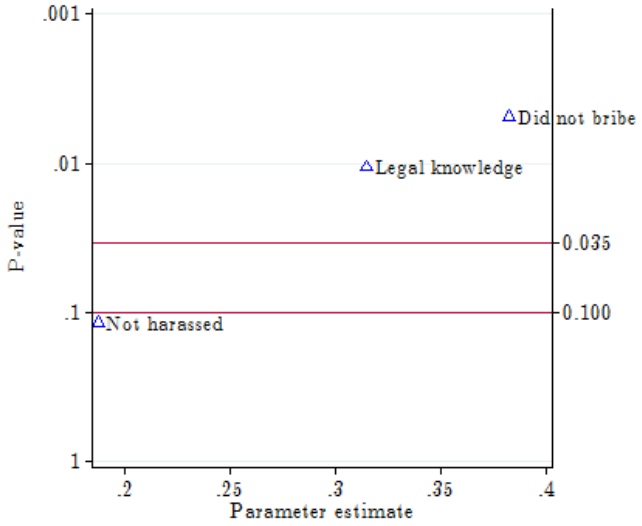
(a) Mean effects index



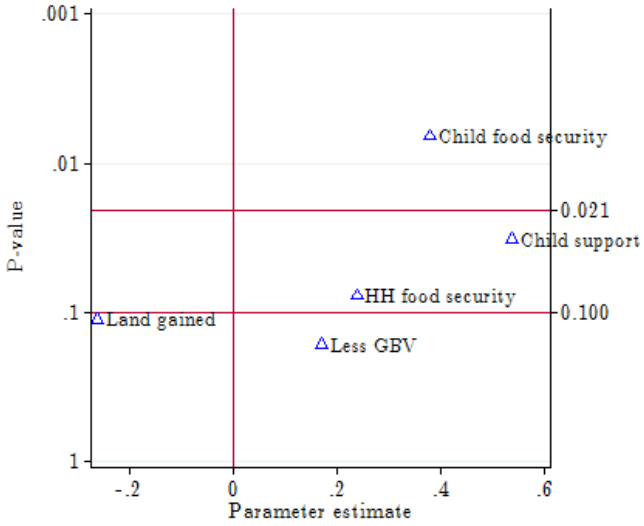
(b) Case results



(c) Justice system



(d) Household welfare



THE POLITICAL LEGACIES OF COMBAT: ATTITUDES TOWARDS WAR AND PEACE AMONG ISRAELI EX-COMBATANTS*

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Abstract

Does combat experience foster hardliner approaches to conflict, diminishing the likelihood of reconciliation? We exploit the assignment of health rankings determining combat eligibility in the Israel Defense Forces (IDF) to examine the effect of combat exposure on support for peaceful resolution of conflict. Given the centrality of the Israeli-Palestinian conflict to global affairs, and with no resolution to the conflict currently in sight, the question of the political consequences of combat becomes all the more pressing. We find that exposure to combat hardens attitudes towards the rival and reduces support for negotiation and compromise. Importantly, these attitudes translate directly into voting behavior, such that combatants are more likely to vote for hardliner parties. These findings cast doubt on research highlighting the benign effects of combat and underscore the importance of combatant reintegration for the transition from conflict to peace.

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1 Introduction

Are former combatants likely to promote or impede the resolution of ongoing conflicts? Policy makers have long sided with the latter approach, viewing ex-combatants as a threat to the successful transition from war to peace. This perception has led to the rise of combatant reintegration programs in societies recovering from violent conflict (Humphreys and Weinstein, 2007). Recent research, however, has challenged this perception, suggesting that the political effects of combat are surprisingly benign. Combat experience has been linked to increased political mobilization and participation (Blattman, 2009), greater volunteerism (Nesbit and Reingold, 2011) and higher turnout rates (Teigen, 2006). These studies join an emerging literature on the political effects of violence exposure on civilian victims, which finds that exposure to violence increases political and civic engagement (Bellows and Miguel, 2009; Voors et al., 2012; Gilligan, Pasquale and Samii, forthcoming). Violence exposure, it is argued, increases personal resiliency and collective coping, resulting in increased political participation and social cohesion.

The recent wave of research highlighting the benign effects of violence exposure has undoubtedly enhanced our understanding of the consequences of political violence. However, such studies are limited in their ability to address the relationship between violence exposure and conflict resolution. First, studies conducted in the aftermath of war and in the midst of reconstruction efforts may be biased in favor of more prosocial effects (Jha and Wilkenson, 2012). Second, increased engagement and mobilization do not in themselves guarantee support for reconciliation or post-conflict stability. Political activism can be harnessed in support of exclusionary policies, promoting further conflict and violence (Chambers and Kopstein, 2001). Third, studies emphasizing violence exposure's benign effects have primarily examined attitudes and behavior towards *ingroup* members.[1] Conflict resolution efforts, however, depend first and foremost on attitudes and behavior towards the *adversary* group, which likely follows a different logic. Finally, the majority of recent studies address the impact of political violence on civilians rather than the political legacies of violence for combatants. In sum, how combat experience affects support for war and peace in the midst of an ongoing conflict remains an open question.

The effects of combat exposure on attitudes towards reconciliation are important since ex-combatants exercise considerable influence on the possibility and viability of conflict resolution. Depending on the context, this may be because their skills and training make them more likely to pose a credible violent threat (Jha and Wilkenson, 2012), because they enjoy moral credibility for having participated in battle (Horowitz and Stam, 2012), or simply because their sheer numbers are large enough to sway public opinion, as in countries with mandatory conscription. Yet research on this important question is hindered by the difficulty of isolating the causal effects of combat experience. Selection into combat is typically not random, making it difficult to disentangle its

[1]For example, Voors et al. (2012) focus on altruistic behavior towards neighbors, and Bellows and Miguel (2009) examine attendance in community meetings and membership in community groups.

effects from pre-existing differences between combatants and non-combatants that independently affect attitudes towards war and peace.

We employ data from an original survey of former Israeli Defense Forces (IDF) soldiers to investigate the consequences of combat for political behavior and for attitudes towards negotiated resolution of the Israeli-Palestinian conflict. Israel's mandatory conscription laws reduce the selection bias associated with voluntary recruitment methods. Nevertheless, assignment into combat, though not completely voluntary, is subject to some self-selection. We overcome the problem of unobserved heterogeneity in combat recruitment by exploiting the health rankings assigned to Israeli youth as part of the IDF recruitment process. We show that health rankings, the primary factor determining combat eligibility, can be used as an instrument for combat exposure.

We find that combat exposure during high-intensity conflict substantially hardens attitudes towards the rival and heightens preferences for military solutions over negotiated ones. Importantly, the hardening effect we identify is not merely attitudinal but finds stark expression in voting behavior, with ex-combatants voting for more hawkish parties. This difference is highly consequential in the context of the Israeli parliamentary system, in which the majority of citizens vote for parties at the center of the political spectrum and in which the split between the Right and Left blocs is typically close. The relative rightward shift is significant and robust for ex-combatants who served under the high-intensity conditions of the Second Intifada. We do not, however, find evidence for a similar shift among combatants who did the bulk of their military service in the relatively low intensity conditions that have generally characterized the Israeli-Palestinian conflict in the years since the IDF's withdrawal from the Gaza Strip in 2005.

Our analysis also reveals that the hardening effects of combat exposure depend on the political backgrounds of combatants prior to enlistment. We find that the lion's share of combat's observed causal effect is driven by a significant shift to the right on the part of ex-combatants who grew up in left, center, and moderate right-leaning households. In contrast, ex-combatants from households on the far right report attitudes to the left of non-combatants from similar backgrounds. The implications of this pattern are that while combat service reduces political polarization among recruits, it also hardens the attitudes of precisely those who might otherwise have supported negotiations and compromise, eroding the support base of political parties advocating reconciliation.

Finally, we find some evidence that combat socialization processes as well as prejudice are mediators of the relationship between combat exposure and attitudes towards war and peace. Combat socialization during high-intensity conflict contributes to political hardening by framing the relationship with the adversary through military lenses, resulting in greater belief in the use of force as an appropriate solution for conflict (Snyder, 1984). As for prejudice, while under some circumstances intergroup contact can reduce prejudice (Allport, 1954), our data lends support to the idea that "negative contact" exacerbates intergroup hostility by making group identity more salient and increasing feelings of anxiety and threat (Stephan et al., 2002).

This article contributes to the emerging literature on the political effects of violence exposure by making an important analytic distinction between political attitudes and behavior towards in-group members and towards the rival out-group. While exposure to war related violence may have positive *intra*-group effects (though we do not find evidence for such effects in our sample), its negative impact on *inter*-group attitudes and behavior provides cause for concern. As such, this paper offers important qualifications to this emerging literature and cautions against the interpretation of violence exposure as benign based on measures of political engagement alone. From a policy perspective, our findings thus suggest a renewed focus on combatant reintegration programs to reduce prejudice and to build faith in the viability and desirability of peaceful conflict resolution.

This article also contributes to a longstanding debate on whether military experience is likely to lead to more conservative or enthusiastic attitudes regarding the use of military force (Huntington, 1957; Feaver and Gelpi, 2004). Evidence for either of these claims remains elusive due to the difficulty in isolating combat's causal effects. As the first well-identified study examining attitudes towards war and peace among ex-combatants, this paper challenges the dominant wisdom that soldiers, having personally experienced the costs of war, are less likely to support military action. Instead, our findings demonstrate that social-psychological processes associated with combat socialization and participation in armed conflict can overcome calculations of self-interest, leading to greater support for military solutions.

Finally, the paper makes a methodological contribution by identifying health rankings as a valid instrument for combat, which may be useful for other outcomes as well as in other contexts beyond Israel. The study's identification strategy can allow for improved causal inference regarding combat exposure's effects, improving our understanding of the long-term legacies of conflict.

2 Combat Exposure and Attitudes towards War and Peace

A considerable body of research has sought to examine how military experience in general, and combat exposure specifically, affects political attitudes and behavior. Contrary to some popular portrayals of ex-combatants as disaffected and violence-prone, the predominant approach argues that combat experience has positive political effects, increasing civic engagement and reducing support for the use of force. The primary mechanism through which combat exposure was postulated to affect attitudes towards war and peace was self interest: Dating back to Huntington (1957) and Janowitz (1960), the conventional wisdom has held that since veterans have experienced the costs of war first hand and might expect to bear those costs again should conflict resume, they are likely to be more conservative about the decision to go to war and to perceive it as a last resort.

These initial theories were based primarily on historical case studies and focused on the influence of senior military officials on the formation of foreign policy. Later studies confirmed that at the elite level, those with military experience tend to be more conservative than civilians regarding the

use of military force (Feaver and Gelpi, 2004). Such research is consistent with studies of *civilian* attitudes towards war, which largely find that as the costs of conflict become more concrete and war casualties more proximate, support for military solutions declines (Gartner, 2008). Relatedly, Horowitz and Levendusky (2011) find that conscription reduces support for war, especially among citizens most likely to be affected by a draft.

Yet while military elites are likely more conservative regarding the use of force than their civilian counterparts, the attitudes of ordinary soldiers who served for limited periods may be different than career officers. Investigating this proposition based on cross-sectional public opinion data, Feaver and Gelpi (2004) find that at the mass level, veterans are more supportive of military interventions than non-veterans. As they note, however, it is difficult to ascertain whether these differences are due to military exposure or to pre-existing differences in political attitudes that shape military recruitment patterns. This limitation is partially addressed in a study by Erikson and Stoker (2011), who examine the effect of lottery-based draft vulnerability on attitudes towards the Vietnam war. The authors find that those more vulnerable to the draft adopt more antiwar attitudes. Consistent with the self-interest approach, Erikson and Stoker also hypothesize that actual military service alienates individuals from the war effort, leading to more dovish attitudes. Their findings, however, indicate that military service is weakly associated with greater support for war.[2]

A second approach linking combat with positive political outcomes emerges from the recent micro-level literature on the consequences of civil wars (Bellows and Miguel, 2009; Blattman, 2009; Voors et al., 2012). Several of these studies have focused on the psychological processes associated with violence exposure, invoking Tedeschi and Calhoun (2004)'s concept of post-traumatic growth to explain findings that violence exposure results in increased political and civic engagement. According to this approach, traumatic experiences can induce positive change, leading to an increased sense of personal strength and more meaningful relationships. While most of this research concerns civilian victims of violence, Blattman (2009) exploits rebel abduction in Uganda to examine the effects of combat exposure, finding that former rebels are more likely to vote and become community leaders. Based on this logic it is plausible that processes of personal growth and resilience may extend to other prosocial outcomes, including support for peaceful solutions to conflict.

As noted, the majority of recent research on the consequences of violence exposure has been conducted in the aftermath of conflict and has focused on political outcomes for civilians at the intra-group, or communal level. In the context of ongoing conflict, however, there are several reasons to expect that war experiences have far more negative effects on inter-group attitudes and behavior, reducing support for conflict resolution.

Socialization: Combat soldiers undergo intense socialization processes that are intended to increase aggression towards rival out-groups and inculcate a sense of identification with the military's

[2]Erikson and Stoker (2011, 227-230) readily acknowledge, however, that their instrument is too weak to allow identification of actual service in Vietnam and is inappropriate for overcoming the problem of self-selection into combat.

objectives and tactics (Grossman, 1996). Such powerful processes, which take place both through formal institutions like basic training and informal initiation and hazing rituals (Wood, 2008), go far beyond ordinary military socialization and are designed to make combatants more comfortable with the use of force. As a result, they may lead combatants to frame relations with the adversary through military lenses, resulting in greater acceptance of military solutions to conflicts (Posen, 1984; Snyder, 1984).

Prejudice: Combat exposure may also lead to the rejection of reconciliation by fostering prejudice. Research in social psychology suggests that when groups are engaged in disagreement over resources and when their interests are perceived as incompatible, intergroup relations will be characterized by prejudice (Brown, 2010). While such processes are applicable to societies in conflict in general, combatants are likely to be more susceptible, as they are typically engaged in sustained negative, threatening contact with the outgroup (Stephan et al., 2002). The circumstances of irregular warfare, in which fighting takes place among ordinary civilians rather than on conventional battlefields, can further heighten prejudice, as civilians are low-status opponents, which tend to elicit contempt and even disgust (Fiske et al., 2002).

Threat: Several studies demonstrate that real and perceived threats raise support for aggressive action against those groups perceived as threatening (Huddy et al., 2005). The perception of threat leads to increased hostility and prejudice (Giles and Hertz, 1994), and ethnocentrism (Grant, 1993). Combatants typically experience such threats first hand, and as such may be more likely to harbor hostility towards the outgroup and support aggressive rather than conciliatory measures. Threat may be compounded by the experience of irregular conflict, where boundaries between combatants and civilians are blurred and violent situations sometimes erupt from what initially seem like innocuous conditions.

Trauma: Exposure to violence can be traumatizing, leading to psychological strain, distress, and PTSD (Levy and Sidel, 2009). Research investigating the role of trauma on attitudes towards war and peace, rather than on political participation generally, finds that violence exposure reduces support for reconciliation, among conflict victims (Pham, Weinstein and Longman, 2004) and former child soldiers (Bayer, Klasen and Adam, 2007). While to our knowledge research has not yet investigated the relationship between trauma and political behavior among adult combatants, it is plausible that similar patterns would hold, reducing support for reconciliation.[3]

All of these mechanisms suggest that combat exposure may actually *reduce* support for peaceful conflict resolution among former combatants, decreasing the likelihood of reconciliation. The handful of studies that have examined the effects of violence exposure on inter-group attitudes and behavior provide some evidence for these grim predictions. For example, Jha and Wilkenson (2012) find that during the partition of South Asia, districts with larger numbers of ex-combatants

[3]There exists a large literature on the relationship between combat trauma and anti-social behaviors such as crime, aggression, and domestic violence, but this literature focuses on the relatively small subset of combatants with PTSD rather than combat's impact generally.

were more likely to undergo minority ethnic cleansing.[4] Beber, Roessler and Scacco (2012) find that Northern Sudanese who personally experienced rioting by Southerners are less likely to favor allowing Southerners to retain citizenship in the North, and Rohner, Thoenig and Zilibotti (forthcoming) find that conflict in Uganda reduces inter-ethnic trust. In the Israeli-Palestinian context, Canetti-Nisim et al. (2009) find that exposure to terrorist violence is associated with hostile and exclusionist attitudes towards Israel's Palestinian minority, and Berrebi and Klor (2008) find that terror attacks within three months of the elections increased support for right wing parties. Finally, using longitudinal public opinion polls, Jaeger et al. (2012) find that exposure to Israeli violence radicalizes Palestinian public opinion, though this effect is relatively short-lived.

These studies all suggest that violence exposure may have dire consequences for inter-group relations and for the likelihood that ex-combatants support compromise and peaceful conflict resolution. However, with the exception of Jha and Wilkenson (2012), past research examines the inter-group consequences of violence for civilian victims, not combatants. And though insightful, the study by Jha and Wilkenson uses aggregated district-level data and hence is unable to test the effect of combat exposure at the micro-level. Our article employs a novel identification strategy and unique survey data that allow us to examine the causal effect of combat exposure at the individual level on three political outcomes: attitudes towards war and peace, vote choice, and political participation.

3 Military Service in the Palestinian-Israeli Conflict

The ongoing Israeli-Palestinian conflict has been a defining feature of Israeli politics since Israel's occupation of the West Bank, Gaza Strip, and East Jerusalem in 1967.[5] After two decades of military occupation, Palestinians first rose against Israeli rule in December 1987 in what became known as the First Intifada (literally "shaking off"). Since then relations between Israelis and Palestinians have been characterized by episodes of intense violence followed by periods of relative calm. Repression of the First Intifada was followed by formal negotiations under the Madrid process in 1991 and secret negotiations culminating in the Declaration of Principles (DOP, also known as the Oslo I Accord), signed by Israel and the Palestinian Liberation Organization (PLO) in September 1993. The DOP and subsequent Cairo (1994) and Taba (1995) agreements called for gradual withdrawal of Israel from the Occupied Palestinian Territories (OPT) and the establishment of a Palestinian National Authority, and provided a framework for final status negotiations. The Oslo process was met with violence from opposition groups among Palestinians and Israelis and its implementation stalled, until ultimately collapsing in 2000 following the Camp David summit.

[4]However, they also find that such districts had fewer deaths, attributing these findings to the organizational skills acquired by former combatants. This mechanism is therefore consistent with both negative and positive intergroup outcomes, depending on the ways in which combatants' skills are put to use.

[5]The roots of the Israeli-Palestinian conflict are, of course, deeper, extending to the struggle between Jewish and Arab nationalist movements beginning around the turn of the 20th century.

On September 29, 2000, the conflict erupted into its bloodiest phase to date, an armed insurgency that came to be known as the Second Intifada. In the course of repression of the insurgency the IDF re-entered areas from which it had withdrawn under the DOP framework, conducting thousands of offensive operations and raids as well as dramatically intensifying its population control measures restricting and regulating Palestinian movement. Over a period of five years levels of insurgent violence gradually declined, and in 2005 Israel unilaterally withdrew its forces and civilian presence from the Gaza Strip. Since 2006 a period of relative calm has ensued, punctuated by occasional bouts of violence. Israeli troops retain freedom of operation in the West Bank, remaining for the most part outside of Palestinian population centers though continuing to conduct limited operations. In Hamas-controlled Gaza, Israeli ground troops are stationed alongside the border while the IDF maintains indirect control through air and naval power, drones, and occasional large scale operations.

The bulk of Israel's military effort in the OPT is born by young men carrying out their compulsory term in the IDF. Since its establishment, Israel has recruited soldiers through mandatory conscription. All citizens are required by law to enlist in the military at the age of 18 and serve a period of three years for males and two years for women. Though the law applies in principle to all citizens, in practice Israel has exempted or created special arrangements for several groups, chief among them Israeli Arabs, Ultra-Orthodox Jews and religious women. As a result, approximately 50 percent of the Israeli population serves in the military, with the figure rising to 75 percent among Jewish males. Upon completing their compulsory service a minority chooses to remain as career soldiers while the rest are released and enter the IDF reserves.

Israeli combatants begin their service with a training period ranging from six to eighteen months and are subsequently deployed to Israel's various theaters of operation, along its borders and in the OPT. Traditionally, soldiers have alternated throughout their service between periods of deployment and periods of training. However, the nature of combat service has varied widely by period, depending on the intensity of conflict in which Israel was engaged at the particular time. During the Second Intifada Israeli troops spent nearly all of their deployment in the OPT, as ongoing training of soldiers was all but suspended to allow for continuous participation in counterinsurgency operations. 3,557 Palestinians and 996 Israelis were killed between 2000-2005, of which 310 were members of the IDF.[6] Combatants who served in this period were therefore exposed to high levels of violence, as perpetrators, victims, and witnesses.

In contrast, combatants who served in the years following the IDF's withdrawal from Gaza experienced much less violence on average. Israeli casualties dropped by 90 percent from the previous period, and though numbers of Palestinian fatalities were often high, most were killed from air and artillery strikes without direct engagement from ground troops. The nature of deployment

[6] In terms of civilian deaths, the Second Intifada was Israel's most violent episode of conflict since its establishment; see data by the Israeli human rights organization B'Tselem, at `http://www.btselem.org/statistics`.

changed as well, as ongoing training resumed, tours of duty became more varied, and soldiers had far less direct military engagement with Palestinians.

Though levels of violence have fluctuated considerably, the OPT remain a central theater of operations for the IDF, and nearly all IDF combatants have taken part in the military occupation. Mandatory conscription renders combat exposure a formative socialization experience for a substantial segment of the population. Ex-combatants attitudes are thus instrumental in forming public opinion on matters of security, war, and peace, and an important political force in encouraging or impeding reconciliation.

4 Research Design, Data, and Measurement

To study the causal effects of combat exposure on voting behavior and attitudes towards reconciliation, we designed and implemented an original survey of former IDF soldiers. Our target population is Jewish, male citizens born between 1980 and 1991 and released from mandatory military service between 2001 and 2012. We exclude Arab Israeli and ultra-Orthodox Jews that are exempt from military service, as well as women who serve in the IDF but for the most part do not serve in combat roles.

The survey was implemented in two waves in March and April 2013, generating two samples: former soldiers who enlisted in the IDF between 2004 and 2009 (the "Post-Gaza withdrawal sample"), and former soldiers who enlisted between 1998 and 2003 (the "Second Intifada sample"). Respondents were recruited by iPanel, Israel's largest "opt-in" internet survey firm. As is common in online survey panels, members collect points for responding to surveys, which they can then redeem for gift certificates. Our power calculation indicated a sample size of about $1,100$ was necessary for identifying a treatment effect of combat exposure in each of the two conflict periods: the Second Intifada and the post-Gaza withdrawal years. Given the expected participation rate of online surveys (between 15 and 20%) and the size of iPane's pool of respondents, we invited every member that matched our inclusion criteria to participate in the survey.

Our mode of respondent recruitment raises natural questions about the representativeness of our samples. While our sample is representative of the Israeli Jewish male population in the relevant cohort in terms of geographical background, immigration status, and share of combatants, it is somewhat skewed towards more educated and less religious respondents.[7] However, as our aim in this study is not to estimate precise population values but to investigate the causal relationship between combat service and political attitudes and behavior, concerns of internal validity make the use of a volunteer panel appropriate (Malhotra and Krosnick, 2007). A more detailed discussion of

[7]Socioeconomic bias is typical in studies that are based on opt-in Internet users. In the U.S., for example, studies find that opt-in panels have higher shares of whites and higher educated individuals than the general population (Malhotra and Krosnick, 2007).

sample representativeness can be found in the Supplementary Information (SI), section 3.3.

Of the 15,216 invitations issued to participate in the study, 2,936 individuals responded, constituting a 19 percent participation rate. Of these, 328 respondents were screened for not serving in the military. We further screened out 146 respondents who served in the air force or navy, since they do not engage in direct contact with Palestinians, volunteers, individuals with large amounts of missing data, and 'satisficers,'[8] leaving a final dataset of 2,334 respondents. The online survey consisted of three parts: We first asked our respondents to provide socio-demographic information in order to test the plausibility of our identification strategy and analyze heterogeneous treatment effects. To avoid priming, we then ask a battery of questions on political attitudes and behavior before turning to a set of questions about military experience and violence exposure. Tables A.1 and A.2 provide summary statistics of the two samples.

4.1 Identification Strategy: Combat Eligibility Health Rankings

Measurement of the causal impact of combat exposure is complicated by selection problems. Even under Israel's mandatory conscription laws, assignment into *combat* is commonly subjected to at least some self-selection. Combatants may differ from non-combat soldiers in important ways, which is especially problematic when pre-service differences contribute to political attitudes later in life. To account for potential unobserved differences between combat and non-combat soldiers we use individual IDF health rankings determining combat eligibility as an instrument for combat exposure. This section describes and provides evidence in support of the use of health-based combat eligibility as a valid instrument, at least in the context of this study.

In Israel, all prospective recruits are summoned to their local recruitment center for several mandatory visits in the year prior to enlistment, to evaluate their suitability for the various possible military assignments. On the first visit, prospective recruits are required to produce detailed information from their family physician and other specialists who have treated them in the past, as well as undergo comprehensive medical examinations including orthopedic and vision evaluations, height, weight, blood pressure, pulse, and urinalysis tests. In some cases, they are ordered to provide additional medical information and attend a follow-up evaluation by medical specialists. The results of the medical examinations are evaluated by a medical committee that assigns each recruit a health score on an A-F scale, known as a "profile score" (Chaiter et al., 2010). These scores are the single most important criterion in determining the likelihood of serving as combatants. Recruits that score A-D are eligible to serve as combat soldiers; those assigned a score of E or F are ineligible for combat and are assigned to non-combat roles; and those assigned a G or H are released from service, temporarily or permanently (though some nonetheless decide to volunteer).

[8] "Satisficing" refers to haphazard responses by respondents employing mental shortcuts to proceed quickly through the survey, compromising data quality (Malhotra and Krosnick, 2007). Note, however, that our findings are robust to the inclusion of airforce personnel and satisficers (see SI).

Members of the latter two categories are excluded from the analysis, since they either did not serve or self-selected into military service, a factor which may well be associated with political attitudes.

Notably, lower health rankings reflect the presence of medical conditions such as chronic illness, allergies, or physical impairments, rather than lifestyle choices such as physical fitness. While low physical fitness may make individuals ineligible for some of the more elite IDF combat units, it does not render them ineligible for combat generally (see SI, section 1, for more information on health rankings). Finally, since combat eligibility may change after initial assignment due to changing health conditions, we use initial eligibility rankings as an instrument.

This study's key independent variable is combat exposure. In order to ensure that only respondents with combat experience are coded as combatants, we define combat exposure as over twelve months of service in a combat role. This means that IDF recruits who dropped out of their combat units during or shortly after basic training are coded as non-combatants. According to this definition approximately 42% of our sample of former IDF soldiers served as combatants, which is almost identical to the share of combatants in the relevant cohorts in the larger population.[9] Table 1 provides information on the bivariate relationship between health-based combat eligibility and combatant status. Only six percent of those assigned a health score *below* the health eligibility cutoff point served as combatants, compared to about half of individuals assigned a health score above the cutoff point.[10] This suggests that a recruit's health score is a strong but imperfect predictor of combat status.

Health-based combat eligibility cutoff	Combatant Status No	Yes	Total
Below (E-F)	385	24	409
	(94)	(6)	(100)
Above (A-D)	973	952	1,925
	(51)	(49)	(100)
Total	1,358	976	2,334
	(58)	(42)	(100)

Table 1: Bivariate Relationship Between Health Score and Combat status.

To be a valid instrument, health eligibility must not only strongly predict combat status but must also be exogenous to the study's outcomes. Key to our identification strategy is the implicit assumption that the presence of chronic health conditions that affect health scores—such as hearing or visual impairments—are *randomly assigned by nature*. If this is in fact the case, pre-treatment covariates between former soldiers above and below the combat eligibility cutoff should be balanced.

[9] Calculations are based on statistics released by the IDF in 2011 and 2013 regarding rates of combat service per district for recruits born in 1990 and 1991. See SI, section 3.3 for details.

[10] Individuals with health scores below the cutoff can become combatants if they successfully appeal their ineligibility based on changes in medical condition. As noted, we only use initial scores as an instrument.

Our survey includes a battery of questions regarding the respondent's background prior to enlistment that may affect motivation to serve as a combatant. These include household income at the age of 18, household ideology at 18 on a 1-7 Right-Left scale, immigrant or Israeli-born, whether the respondent's father served as a combatant, level of religiosity at age 18, ethnic background, and residence during high school. Table A.3 provides a balance check for the Second Intifada and post-Gaza withdrawal samples. A rule of thumb for assessing covariate balance is if the means of both groups are less than one fourth of a standard deviation apart (Ho et al., 2007). Table A.3 indicates that all covariates are relatively well-balanced.

There are two potential ways in which our identification assumption could be undermined. First, health impairments may in principle be correlated with household income, which in turn may be correlated with political attitudes. We do not, however, find any evidence for such a correlation in our sample, likely due to Israel's comprehensive universal healthcare system that ensures that all citizens have access to healthcare regardless of socioeconomic status. Second, it is possible that some individuals intentionally sort themselves into low or high health categories and that such sorting patterns are correlated with political attitudes. We do not believe that sorting constitutes a major concern in this study. First, the ability to sort is rather limited given the comprehensiveness of the medical examination at the recruitment centers and the quality control mechanisms in place to assess the profile assignment process (Chaiter et al., 2010). Second, since only about 50% of those who are eligible ultimately serve as combatants, there are ample ways for new recruits with low motivation to serve to avoid combat assignment without manipulating health information.

We nonetheless take several steps to eliminate any remaining concerns about sorting. First, we conducted a pilot study of approximately 400 Israeli youth who had already been assigned combat eligibility rankings but had not yet enlisted. Had a significant share of recruits intentionally manipulated their medical condition, we would have witnessed that combat eligibility is correlated with political attitudes or behavior *before* military service. We find no significant differences in political attitudes or participation between youth above and below the combat eligibility threshold.[11] Second, we conduct a sensitivity analysis that estimates how large an unobserved covariate u must be to impact combat eligibility, thereby violating the exogeneity assumption. Assuringly, we find that our findings are robust to the presence of relatively large potential unobserved confounders affecting treatment status (SI, section 2.2).

In addition, to better account for the fact that selection into combat is not random for those *above* the eligibility threshold we conduct the following analysis. First, we subset our data to only include those who are assigned a health score above the combat eligibility threshold. Second, we fit a logistic model in which a combat status indicator is regressed on all available covariates.[12] Using

[11] Further details regarding sampling, data, measures and analysis of the youth survey are presented in the SI, section 2.

[12] Since the IDF releases no demographic indicators on its recruits, there is no systematic evidence available regarding the characteristics of people who select into combat.

this model, we then predict for *all* survey respondents the propensity to serve as a combatant, whether their health scores are above or below the combat eligibility cutoff. Strikingly, the mean combat propensity is almost identical for both of our samples. In the Second Intifada sample, the mean combat propensity for those above and below the eligibility cutoff is 0.47. Similarly, in the post-Gaza withdrawal sample, the mean combat propensity is almost identical for those above (0.48) and below (0.49) the cutoff . Had there been systematic intentional sorting into health categories above and below the eligibility cutoff we would expect the mean propensity to serve as combatants among those below to be significantly lower than the mean propensity of those above. In sum, we believe the evidence at hand supports the use of health-based combat eligibility as an instrument for combat exposure, at least in the Israeli context. We now turn to describe measurement of the study's main outcomes of interest.

4.2 Measurement of Key Variables

Our study examines the causal effect of combat exposure on three broad outcomes: *attitudes towards reconciliation*, *political participation* and *vote choice*. We operationalize the first two outcomes by grouping a number of related measures into summary indices. Following Anderson (2008), a summary index is a weighted mean of several standardized outcomes, where the weights—the inverse of the covariance matrix—are used to maximize the amount of information captured by the index. This approach improves statistical power while being robust to over-testing because each index represents one test. Moreover, summary indices ensure that the probability of a false rejection does not increase as additional items are added to the index, and minimize the risk that researchers over-interpret individual proxy measures that may be statistically significant due simply to random chance. We report the effect of combat exposure on both the summary index and the constituent items, but conclude that combat exposure has a causal effect if and only if the coefficient on the summary index is statistically significant by conventional standards. Each of the outcomes is described in turn.

Attitudes Towards Reconciliation

Attitudes towards reconciliation are operationalized using an index of ten items that are considered central matters of dispute between Israelis and Palestinians and constitute obstacles to resolving the conflict: Degree of support for territorial compromise in the West Bank ("*negotiated territorial withdrawal*"); degree of support for territorial compromise in Jerusalem ("*negotiated division of Jerusalem*"); degree of support for compromise on a right of return for Palestinian refugees ("*negotiated refugee right of return*"); degree of belief that there is a Palestinian partner for peace ("*Palestinians are partners for peace*"), degree of support for conciliatory policies for ending the conflict such as a two-state solution ("*support conciliatory policies*"); degree of agreement with

the notion that the land of Israel belongs to the Jewish people and is therefore indivisible ("*Israel indivisible*"); degree of support for Israeli settlements in the West Bank ("*settlements endanger Israeli democracy*"); degree of principled opposition to the occupation of Palestinians ("*occupation is illegal/immoral*"); degree of support for freedom of operation for human rights monitoring NGOs ("*oppose limitations on NGOs*"); and self-identification on a seven-point Right-Left scale ("*dovish self-identification*"). All items were recoded such that higher rankings represent views that are more conciliatory, and were standardized to allow for easy comparison of effect magnitude. The ten items are positively correlated with a Cronbach's alpha of 0.90.

Vote Choice

A finding that combat exposure impacts the willingness to support political compromise, by itself, does not reveal the extent to which such attitudes translate into meaningful political action. We therefore examine the effect of combat exposure on vote choice. We use two variables to measure voting behavior. First, we asked respondents to indicate which party they voted for in the most recent parliamentary elections, held in January 2013. We then coded all responses on a nine-point Right-Left scale, constructed using the 2013 Israeli Election Compass published by the Israel Democracy Institute (IDI) (See SI, section 4.6, for coding details). Second, we measure vote choice in the first elections that took place after release from the IDF. A comparison of these two measures of vote choice provides an indication of the degree to which effects of combat exposure may change over time.

Political Participation

Political participation is operationalized using a summary index of nine proxy measures. These include a five-point scale measure of interest in politics, a three-point scale measure of degree of party activism, and a set of seven indicator variables measuring whether the respondent participated in various political activities in the past twelve months, such as taking part in demonstrations, contacting a member of Parliament, writing a newspaper or calling in radio shows, writing on political affairs on social media sites, etc. All nine variables are positively correlated though the Cronbach's alpha of 0.66 is somewhat low.

4.3 Estimation Strategy

Estimation strategies were developed in advance in a detailed pre-analysis plan posted on the Experiments in Governance and Politics (EGAP's) Design Registration webpage prior to analysis. The plan specified the measures of variables and econometric specifications that we would use in our analysis. In the online appendix we describe deviations from the pre-analysis plan and the rationales for these.

To estimate the impact of combat exposure we use an encouragement research design whereby an individual's health-based combat eligibility Z instruments for combat experience d. In an encouragement design, randomization is not feasible for the treatment itself (in this case, combat exposure), but the encouragement to receive the treatment is 'as good as random'. While the encouragement substantially increases the probability of treatment, it does not guarantee it, as compliance behavior varies within the encouraged group. Put differently, health scores represent the intention to treat (ITT) rather than the treatment itself.

Our main estimation strategy for the effect of combat exposure on the compliers is, therefore, a simple 2SLS instrumental variable regression with no controls. The IV model considers the effect of combat exposure — endogenously chosen binary treatment d_i, $E[\varepsilon|d \neq 0]$ — on outcome variable Y_i, conditional on the instrument Z_i, where Z_i is an indicator variable that takes the value of 1 for combat eligible soldiers, and zero for soldiers ineligible for combat. The primary interest is in the regression function:

$$Y_i = \alpha_y + \delta d_i + \varepsilon_i \tag{1}$$

where δ is the Local Average Treatment Effect estimate, and ε_i is the error term. The binary decision to serve or not as a combat soldier d_i is modeled as the outcome of a linear function of the instrument Z_i and a random component υ_i. Specifically,

$$d_i = \alpha_d + \Pi Z_i + \upsilon_i \tag{2}$$

5 Main Results

This section presents our findings regarding the impact of combat exposure on our three outcome variables: attitudes towards reconciliation, vote choice, and political participation. To account for differences in combat environments, we distinguish between our Second Intifada sample (n=1,189) and post-Gaza withdrawal sample (n= 1,145) in all results.[13] Results are reported in figures for ease of presentation, with dots representing the point estimates and surrounding lines representing 90% confidence intervals (see SI, section 4, for tabular results.)

Attitudes towards Reconciliation

Does combat exposure affect attitudes towards war and peace? Figure 1 indicates that for ex-combatants from the Second Intifada combat exposure is associated with a substantial and significant decline in the reconciliation summary index (0.3 standard deviation; pvalue=0.081). This

[13]Core findings for the pooled survey are substantially unchanged in terms of coefficient direction and significance levels. See Table 5 in SI for full pooled results.

pattern holds for most of the constituent variables that make up the index: Ex-combatants are significantly less likely to support territorial withdrawal as part of a possible peace agreement, to believe that the current Palestinian leadership is a genuine partner for peace, and to support a conciliatory solutions to the conflict. They are also significantly more likely to support Jewish settlements in the West Bank and rank themselves as hawkish on a Right-Left scale. In contrast, ex-combatants who served in the post-Gaza withdrawal years do not for the most part differ from noncombatants in their attitudes towards reconciliation. These findings, as well as our other main results, are discussed in the next section.

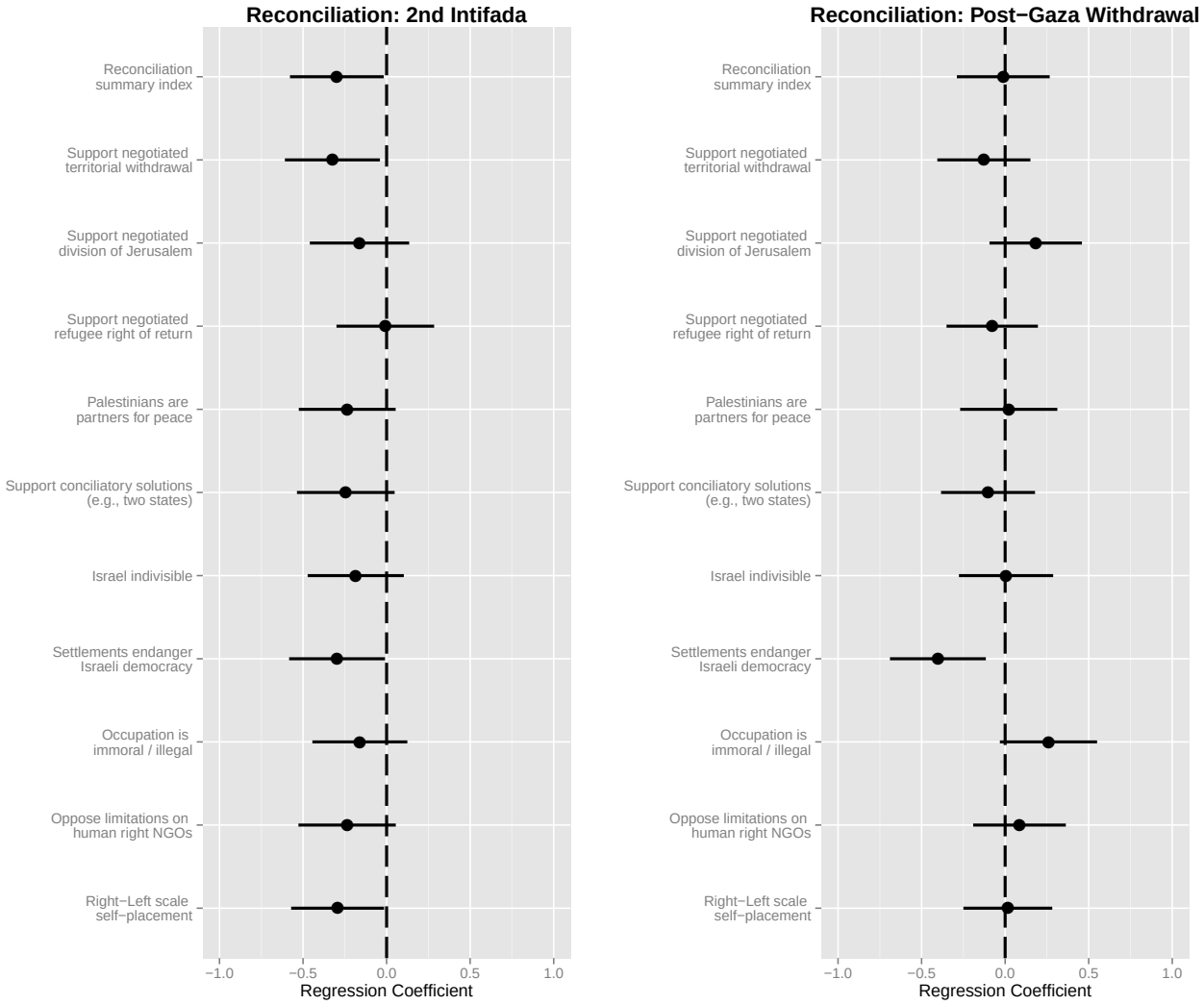

Figure 1: **Impact of combat exposure on attitudes towards reconciliation.**

Vote Choice

As shown in Figure 2, ex-combatants who served during the Second Intifada are substantially and significantly more likely to vote for hardliner parties. The magnitude of the effect of combat exposure on vote choice is remarkable: Ex-combatants from the Second Intifada voted in Israel's recent elections for parties located 0.7 to the right on the right-left scale of parties (pvalue=0.031). We find no evidence for an attenuation of the hardening effect. Quite the contrary, the magnitude of combat's effect on vote choice rises from 0.4 to 0.7 between the first elections following military release and the elections of 2013. A similar hardening pattern is evident among combatants who served in the post-Gaza withdrawal period, but the effect is not statistically significant.

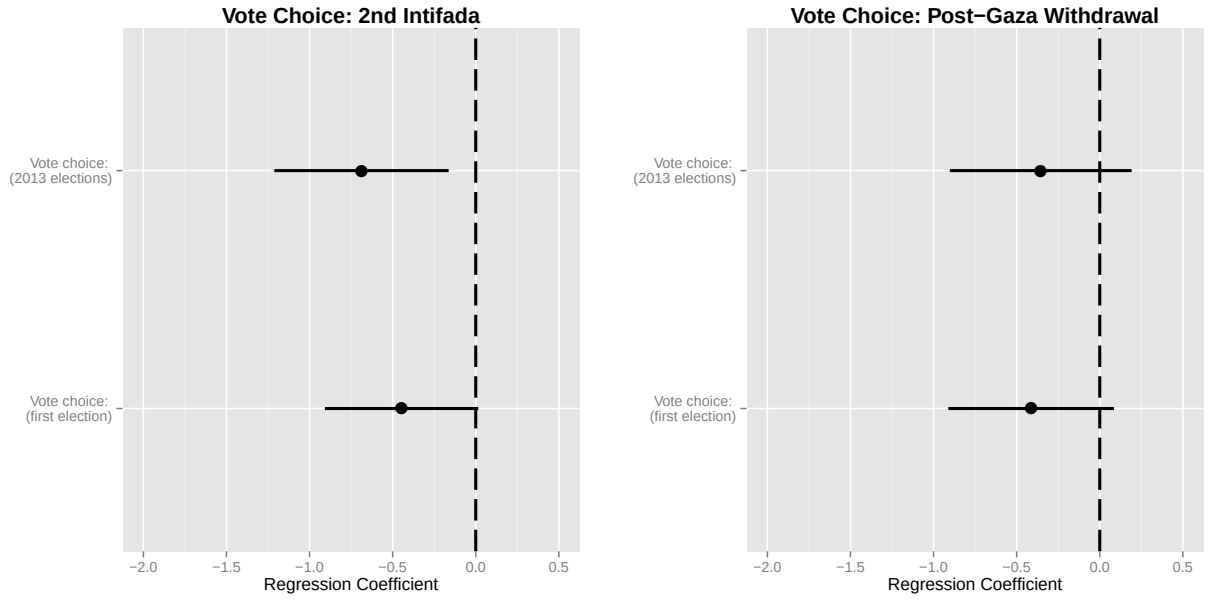

Figure 2: **Impact of combat exposure on vote choice.**

Political Participation

We find that combat exposure results in lower levels of political participation (Figure 3). The effect is substantial and significant among ex-combatants from the Second Intifada (0.31 standard deviation, p=0.062), and approaches statistical significance in the post-Gaza withdrawal sample as well (p=0.155). These results run contrary to findings by Blattman (2009) that combat exposure among rebels leads to greater political engagement. The divergence in findings may be due to differences in the political environment between Israel and Uganda. For example, it might be that in Israel combat service itself, including reserve duty, is viewed as a form of political participation.

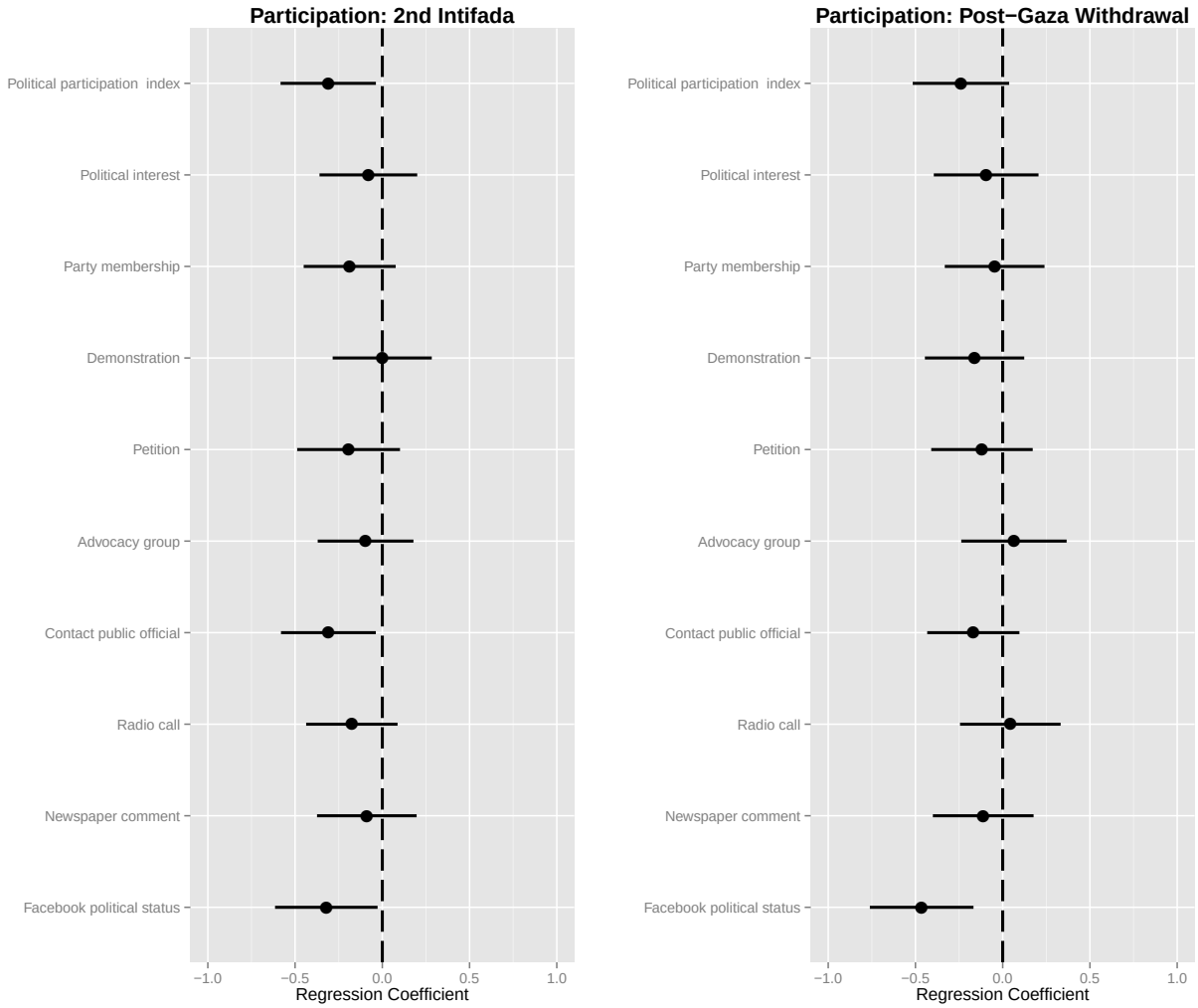

Figure 3: Impact of combat exposure on political participation.

Whose Attitudes Harden? Combat-effect Heterogeneity

For a more nuanced understanding of how combat exposure affects the political attitudes of combatants from the Second Intifada, we turn to examine heterogeneous treatment effects as a function of combatants (pre-treatment) ideological backgrounds. We measure background by asking respondents to rank the political ideology of the household in which they were raised on a 1-7 Right-Left scale. We first dichotomize the household ideology scale at the median, coding household ideologies below the median as right-leaning (1-3), and those at the median or above as center and left-leaning (4-7). The left panel of Figure 4 depicts the effects of combat on individuals from right-leaning households, individuals from center and left-leaning households, and the average effects on the entire sample. The results indicate that the effects of combat vary by household ideology: while combatants from center and left-leaning households exhibit a substantial and significant hardening in attitudes and vote choice and a significant reduction in political participation, combat has no

significant effect on soldiers from right-leaning households.

Next we split the data between the far-right (household ideology 1-2) and the left, center, and moderate right (3-7). The results, shown in the right panel of Figure 4, reveal that the hardening effects of combat extend to soldiers from moderate-right leaning backgrounds. In contrast, combatants from backgrounds in the far-right actually exhibit a substantial, significant shift to the *left* compared to individuals from similar backgrounds who served in non-combat roles. Combat exposure, then, hardens the political attitudes of all but the most right-leaning combatants, while moderating somewhat the attitudes of combatants from the far right. The overall effect is a significant reduction in polarization, compared to the distribution of political attitudes of non-combatants (see SI, section 4.1 for a formal test using randomization inference).

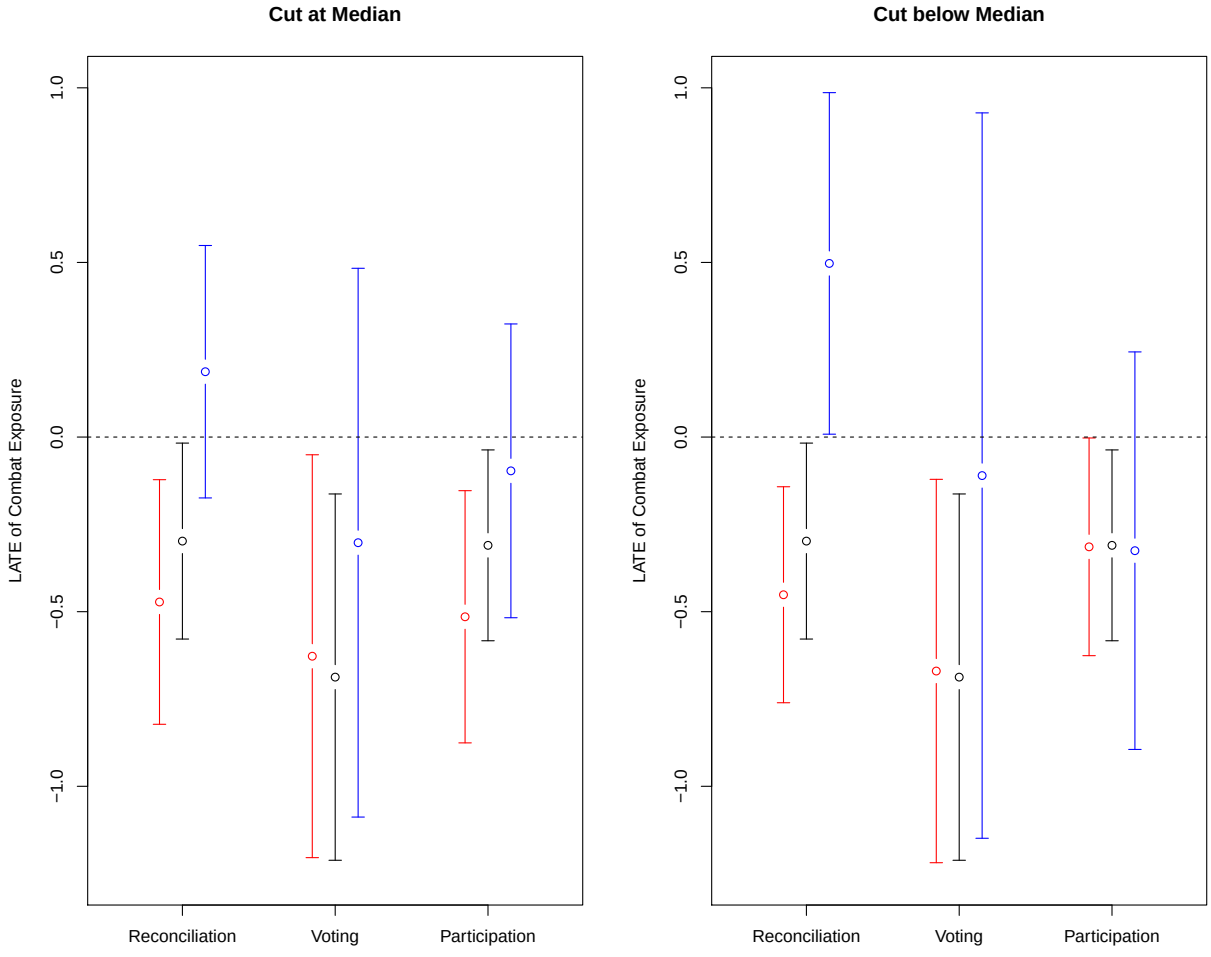

Figure 4: Heterogeneous Effects of Combat Exposure as a Function of Household Ideology. In the left panel, the red line indicates center and left-leaning backgrounds and the blue indicates right-leaning backgrounds. In the right panel, the red line represents the center, left, and moderate right, and the blue represents far-right backgrounds. The black line is the overall local average treatment effect.

6 Robustness

We conduct several tests to assess the robustness of our findings, employing an alternative indicator for the independent variable, estimating the effects of combat on the subset of observations close to the health-based eligibility threshold, and using a variety of matching methods that improve covariate balance between the treated and untreated groups. This section summarizes these tests in brief. Details and full results can be found in section 4 of the SI.

First, to ensure that our measure of combat exposure captures actual combat experience, we test an alternative measure of combat: a binary variable indicating whether respondents reported participating in at least one of the major combat operations that took place during the Second Intifada. Results are even stronger than those reported here: combat exposure has a substantial and significant negative effect on support for reconciliation, vote choice, and political participation. Analysis of heterogeneous effects by household ideology produces the expected pattern, with the hardening political effects of combat evident among combatants from center and left-leaning backgrounds but not among those from right-leaning homes.

Next we estimate effects on the subset of the data closest to the eligibility threshold, excluding from the analysis former soldiers who were assigned a health score of A or B. Narrowing the window in this way results in a loss of approximately 70% of the observations, such that even large point estimates fall below conventional significance levels. Still, the direction of coefficients is generally similar to our findings, increasing our confidence in the results.[14]

Finally we test the robustness of our results to various matching estimations. We first use our estimate of combat propensity (see section 4.1 above) to match between combat eligible and combat ineligible former soldiers with similar propensities to become combatants. We then run the instrumental variable regression on the matched sample using a variety of matching algorithms. The core pattern identified here remains in evidence: combatants from center and left-leaning backgrounds are substantially and significantly less supportive of reconciliation, negotiations, and compromise, while combatants from right-leaning backgrounds show opposite effects, though these do not quite reach statistical significance. We then perform an additional matching analysis, this time matching eligible and ineligible soldiers on the *covariates* themselves. Once again we find that combat exposure has substantial and significant negative effects on support for reconciliation among soldiers from center and left-leaning backgrounds, and positive (but non-significant) effects on combatants from right-leaning backgrounds.

[14] In order to identify a significant effect for a coefficient of the magnitude reported here the sample would need to be more than double in size. See SI, section 4.3, for simulation-based power analysis.

7 Discussion

The findings in this study present an important challenge to the growing literature stressing the benign effects of violence exposure and to past work on the conservative attitudes of former soldiers towards deployment of military force. Our data suggest that high intensity combat hardens attitudes and significantly reduces support for reconciliation. Ex-combatants who served during the Second Intifada self-identify as more hawkish on a Right-Left scale than non-combatants and are also significantly less likely to support negotiations and compromise. These effects are remarkably durable, remaining evident nearly a decade after release from service.

Importantly, this hardening effect is not limited to attitudinal change but affects vote choice, with former IDF combatants substantially and significantly more likely to vote for hardliner parties. This finding reflects the fact that the Israeli-Palestinian conflict is the most salient issue in the Israeli political arena since 1967, and attitudes towards the conflict are the key determinant of Israeli voting behavior (Arian and Shamir, 2008). The size of the effect of high-intensity combat exposure on vote choice is considerable, indicating a relative shift of close to a party to the right on a nine point left-right scale.

As a rough illustration of the magnitude of this effect, consider that in 2013, the Israeli parliament split nearly equally between the Right block (61 seats) and Center-Left blocs (59 seats). One parliamentary seat is equal to approximately 1/120 of the popular vote, which translated into $29,364$ votes in the 2013 elections. The number of IDF troops is undisclosed, but according to estimates by the International Institute for Strategic Studies, in 2004 IDF ground troops included $85,000$ conscripts, which is equivalent to about one parliamentary seat a year. Eight cohorts of combatants serving during all or part of the Second Intifada are thus equivalent to 7-8 seats, a number that can exercise considerable political influence in Israel's polarized political arena. A comparison of the predicted values of the dichotomous "*support conciliatory policies*" variable reveals that among non-combatants, support for a conciliatory solution to the conflict is 59%, compared to 46% for ex-combatants. The hardening effect of combat thus reduces *de facto* the likelihood that political parties supporting a negotiated peace agreement could form a winning coalition.

Our analysis of heterogeneous effects reveals that combat nonetheless affects different combatants in different ways. While combat experience reduces support for peaceful conflict resolution among individuals from backgrounds in the political left, center, and even moderate right, ex-combatants from hardliner political backgrounds become somewhat less radicalized, though the significance of this moderation effect is not as robust to changes in model specifications. The political significance of this pattern is twofold. First, it demonstrates that service in combat units reduces political polarization among recruits. Totalizing socialization processes, brotherhood-in-arms, and the intense experience of participation in armed conflict decrease political differences among combat soldiers, reflected in a large and significant decline in political attitudes variance

among ex-combatants when compared to former non-combatants (SI, section 4.1). At the same time, the fact that all but the most hardliner combatants become more hawkish in their opinions has substantial implications for the likelihood of reaching a peace agreement, as combat hardens precisely those individuals who would otherwise have advocated more conciliatory views. Combat exposure thus erodes the support base of leaders and parties advocating for peace negotiations, a factor all the more acute given mandatory conscription.

More generally, these heterogeneous effects suggest that the political effects of combat exposure are moderated by additional factors. This is also reflected in the divergence of our results in the two periods examined. While we find substantial political hardening among combatants who served during the Second Intifada we do not find similar effects in subsequent years. What accounts for this difference? A likely explanation is the very different nature of combat service in the two periods. In the Second Intifada, combatants confronted an armed Palestinian insurgency, engaging in continuous operations in the OPT under risky and intense conditions. The high levels of violence and particularly the suicide terror attacks in Israeli cities raised the legitimacy of the IDF's counterinsurgency campaign, which enjoyed very high popular support among the Israeli public generally and among combatants in particular. The subsequent years saw a sharp reduction in Palestinian violence and consequently a change in the nature of combat operations, which were typically more limited and far less intense.

The difference in conflict intensity across the two periods is evident in our sample: Table A.4 compares the mean levels of violence exposure for individuals who served during and following the Second Intifada on three binary measures, indicating whether respondents reported witnessing first-hand the injury or death of an (a) Israeli soldier; (b) enemy combatant; and (c) Palestinian civilian. The differences in violence exposure are large and significant, corroborating official data on conflict casualties.

To assess whether the intensity of conflict can indeed account for the hardening effects of combat in the Second Intifada we examine the effects of combat-related violence exposure on our three political outcomes. Figure 5 shows that all three conflict intensity indicators are associated with a substantial and significant reduction in support for reconciliation, a shift to the right in vote choice, and decrease in political participation other than voting. Echoing our main results, this effect varies by household ideology, with combatants from center and left-leaning backgrounds, but not combatants from the political right, exhibiting a large and significant hardening effect.

The association between political hardening and conflict intensity suggests that serving in a combat unit leads to more exclusionary attitudes once a certain threshold of violence has been crossed. When combatants operate in relatively safe and nonviolent conditions, their military service is not very different from the type of service experienced by non-combatants, and as a result political effects are far less consequential. This proposition is consistent with studies of Israeli public opinion, which have found attitudes towards peace to be negatively affected by conflict intensity,

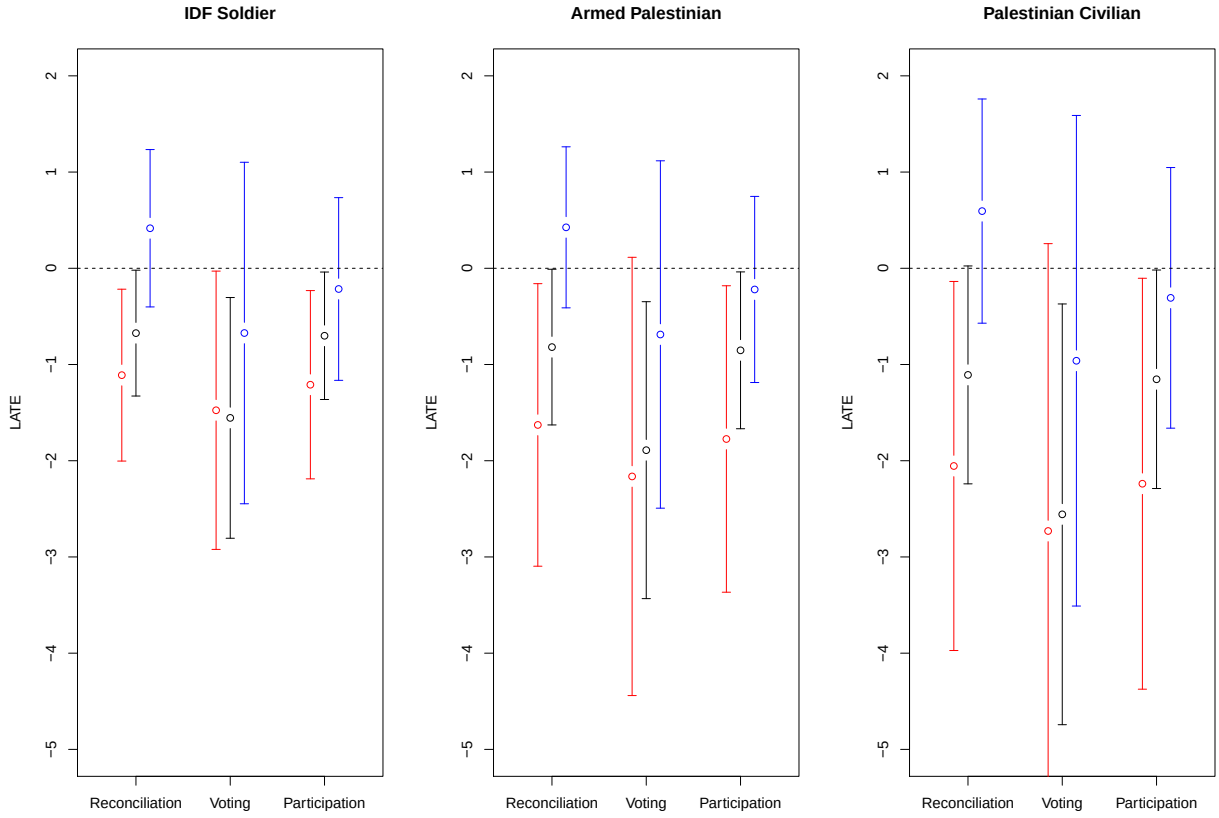

Figure 5: Political Effects of Conflict Intensity. The red line indicates center and left-leaning backgrounds and the blue indicates right-leaning backgrounds. The black is the average treatment effect.

and particularly by Israeli fatalities (Berrebi and Klor, 2008; Fielding and Penny, 2009). The next section explores the reasons for this effect in greater detail.

8 Causal Mechanisms

Why does exposure to high intensity combat lead to less conciliatory attitudes? Section 2 proposed several mechanisms that might explain such a relationship. In this section we explore these mechanisms empirically, with the exception of PTSD, which was not measured in our survey. For each, we examine heterogeneous effects when splitting data at the household ideology median and when splitting data at the moderate right. Full results are reported in SI, tables 7 and 8.

We note, however, that we do not conduct complete causal mediation analysis due to non-compliance in our data. The portion of LATE that goes through the mediator, let alone the average causal mediation effects (ACME) of the treatment itself, cannot be identified without making strong assumptions, even if the ignorability assumptions of mediator and outcome hold

(Imai et al., 2011). ACME may be identified if we are willing to assume that treatment uptake (combat exposure) is ignorable conditional on the encouragement and pre-treatment covariates. Recalling that only about half of those assigned a health score above the combat eligibility cutoff ultimately serve as combatants, we do not believe that this is a reasonable assumption. Our data suggests, instead, that unobserved heterogeneity significantly influences uptake *for those assigned a health score above the eligibility threshold*. In consequence, our causal mechanism analysis only allows testing for a causal relationship between the treatment (combat exposure) and the proposed mediator (which we treat as an outcome variable), but not for linking causally the mediator to the ultimate outcome. Findings should be interpreted with this caveat in mind.

We first assess two possible mechanisms that capture socialization processes: *framing* effects and *peer* effects. Combat socialization frames the adversary through military lenses as an enemy to be defeated by force. To test the presence of such framing effects in our sample, we ask our respondents to indicate their position on a seven-point scale between two opposite statements: 1) There is no military solution to the conflict with the Palestinians; or 2) Palestinians can only be overcome by military force. Here higher values indicate greater support for military solutions for conflict. Consistent with the general pattern of polarization reduction, we find large and significant framing effects (0.369 standard deviation, pvalue=0.065) among combatants from center, left, and moderate right backgrounds, while combatants from backgrounds in the far right show reduced support for military solutions (-0.560 SD, pvalue=0.136). This pattern is also evident when data is split at the median, though effects are no longer significant.

A second way in which socialization processes might have a mediating affect on political attitudes is through the influence of peers. Combat service is an intense small group experience in which camaraderie is cherished as a central value. The influence of peer attitudes is thus expected to be particularly high. We measure peer effects by asking about the dominant political position in the respondent's unit, where the variable receives a value of 1 if the unit is considered more right-leaning and zero otherwise. We generally find that combatants and non-combatants alike report that the dominant political opinion in their unit was right-wing. This likely reflects the fact that in the military context, militant opinions are more likely to be expressed and valued. We thus conclude that this cannot be a key channel through which combat experience hardens political attitudes.

Combat exposure can also lead to exclusionary views by fostering prejudice, as discussed above. We measure prejudice against Palestinians using a weighted summary index that includes both direct and indirect proxy measures. The direct measure is a survey question asking respondents to indicate their position on a seven-point scale between two opposite statements concerning the goals of the majority of Palestinians: 1) living peacefully alongside Israel's Jewish population or 2) taking over Israel and killing or expelling its Jewish population. We measured prejudice indirectly by assessing the extent to which attitudes towards Palestinians differ from attitudes towards Israelis: Respondents ranked Israelis and Palestinians on four dimensions on a nine-point

scale: intelligence, trustworthiness, altruism and non-violence, with question order randomized to minimize priming. The prejudice measure was constructed by subtracting ratings of Palestinians from ratings of Israelis. The five variables were positively correlated with Cronbach's alpha of 0.74. We find that combatants from center and left-leaning backgrounds are far more prejudiced against Palestinians than their non-combatant counterparts. While the size of this effect is quite large (0.312 standard deviations), it falls slightly short of statistical significance (pvalue=0.128).

It is also possible that combat exposure leads to a heightened sense of threat, reducing support for peaceful conflict resolution. We asked respondents to indicate, on a five-point scale, to what extent they feared (a) missile attacks; (b) terror attacks; and (c) that Palestinians would pose an existential threat to Israel should the IDF withdraw from the West Bank. We do not find evidence that combatants are more likely to fear terror or missile attacks than non-combatants, perhaps because combatants were no more likely to be targeted by such violence than non-combatants or civilians. However, ex-combatants from center and left-leaning backgrounds are far more likely to feel that ending the military occupation of the West Bank would pose an existential threat to Israel's security than soldiers from similar backgrounds who did not serve in combat units. Again, this effect is large (0.35 standard deviations) but falls slightly short of statistical significance (pvalue=0.171).

9 Conclusion

This study offers robust evidence that combat exposure has a significant and durable negative effect on attitudes towards conflict resolution, substantially reducing support for negotiated compromise. This effect is not limited to attitudes but extends to political behavior, producing a significant shift to the right in vote choice. These results are highly consequential in Israel, where the negotiated resolution of the Israeli-Palestinian conflict is a matter of intense contestation.

Our evidence suggests that it is not mere membership in combat units that matters, but rather that combat interacts with background characteristics of combatants as well as with features of the political and military environment. Combat reduces polarization among soldiers, hardening the attitudes of individuals from left-leaning, center, and moderate right-leaning backgrounds, and moderating somewhat the attitudes of combatants from the far-right. Furthermore, combat hardens the attitudes of those who served in the high-intensity environment of the Second Intifada, but has little effect on combatants who served in in the subsequent, low-intensity years. Put succinctly, high-intensity combat has the most negative political effect on the very individuals who might otherwise have supported and promoted peaceful conflict resolution.

These findings challenge the predominant approach regarding the effects of military service on attitudes towards war and peace. Invoking calculations of self-interest, this approach suggests that combatants should be more cautious regarding military action than their civilian counterparts since they have personally experienced the costs of war. While this approach may indeed hold

for senior military officials, our evidence suggests that at the mass level psychological processes brought about by combat socialization and participation in armed conflict trump self-interest and decrease support for compromise and reconciliation. Initial evidence provided by Feaver and Gelpi (2004) and Erikson and Stoker (2011) has offered preliminary support for this contention, but as the authors acknowledge, such evidence was based on cross-sectional survey data or on a weak instrument that could not well identify the causal effects of combat. The identification strategy employed here substantiates this claim with robust evidence for combat exposure's causal effects.

This study also provides a counterpoint to recent research on the benign effects of violence exposure. Such research has largely been conducted in post-conflict settings and has focused on attitudes and behavior towards ingroup members, such as social cohesion, trust and altruism towards neighbors. We demonstrate that in the context of ongoing conflict, the effects of violence exposure are far bleaker, reducing political participation, increasing prejudice, and fostering support for hardline solutions.

While our data preclude the possibility of conducting formal mediation analysis, we find suggestive evidence that the hardening effect of combat is due both to combat socialization processes and the experience of combat under conditions of asymmetric warfare, heightened violence, and risk. These experiences inculcate a sense of threat and increase prejudice towards the outgroup, which in turn result in more exclusionary attitudes that linger long after combat service has ended.

Though mechanisms of prejudice and threat are supported in the data, there may be other explanations underlying the observed relationship between combat exposure and reduced support for compromise and reconciliation. For example, it could be that combatants and non-combatants follow different life trajectories upon release from service. By this logic combat still has an identified causal effect, but it does so by determining different post-military paths for combatants and non-combatants, which in turn contribute to variation in political attitudes. As this study is a first effort to investigate the causal effects of combat exposure on attitudes towards war and peace in the midst of ongoing conflict, a fruitful avenue for future research would be to further examine the causal mechanisms that account for combat's hardening effects. The literature to date has invoked a multitude of mechanisms, each pointing towards distinct processes. Violence exposure is hypothesized to lead to exclusionary attitudes and behavior through psychological distress and PTSD (Canetti-Nisim et al., 2009; Pham, Weinstein and Longman, 2004; Bayer, Klasen and Adam, 2007), the desire to reduce threat (Beber, Roessler and Scacco, 2012), or organizational skills that facilitate collective action (Jha and Wilkenson, 2012). Our study provides initial evidence that combat exposure is associated with increased prejudice. Further studies may shed additional light on the relationship among these mechanisms as well as identify the potentially different processes at work for civilian victims and for combatants.

A second task for future research is to better specify conditions under which combat exposure leads to hardliner attitudes. We have seen that in our sample, findings diverge by period of service.

Our analysis suggests that this variation is best explained by differences in the intensity of combat and levels of violence exposure in the two periods. The implication of this finding is that the political effects of violence exposure are moderated by conflict intensity, a proposition that awaits further testing and analysis.

A natural question that arises is to what extent our findings apply beyond the Israeli case. Indeed, there are some unique features of the Israeli-Palestinian conflict that may contribute to our results, such as the relatively high share of former combatants in the Israeli population due to mandatory conscription or the salience of the conflict for Israeli voting behavior. In settings where combatants are a negligible share of the population and hence exercise little political influence, or where an ongoing violent conflict is less prominent in day-to-day politics, the impact of combat exposure on the likelihood of achieving peace may be considerably lower. In addition, as the effects of combat depend on the political backgrounds of soldiers, its impact is expected to vary depending on the types of individuals who enlist. In contexts where most combatants are radicalized before entering service, the experience of combat may actually moderate political attitudes.

The particular nature of the Israeli-Palestinian conflict may also limit the generalizability of our findings in other respects. As an irregular conflict taking place under conditions of military occupation, the conflict brings soldiers into sustained negative contact with civilians from another ethnic group, thereby accentuating differences and power asymmetries. Further research in other settings can shed more light on whether our findings apply to comparable contexts of irregular ethnic conflict, or whether the logic linking combat exposure to reduced support for reconciliation extends to additional types of conflicts, including conflicts between states or civil wars. Already, mounting evidence from settings as diverse as India (Jha and Wilkenson, 2012), Sudan (Beber, Roessler and Scacco, 2012), Uganda (Rohner, Thoenig and Zilibotti, forthcoming) and Georgia (Baur et al., forthcoming) has demonstrated the harmful legacies of political violence for intergroup relations. To the extent that those affected by violence are a large sector of society, this legacy may have considerable negative implications for the likelihood of reaching and upholding a peace agreement. Our study thus points to a potential micro-level mechanism for the oft-noted macro-level finding on the frequent recurrence of conflict (Doyle and Sambanis, 2000).

From a policy perspective, our study has implications for both the Israeli-Palestinian conflict and for conflict resolution more generally. In the Israeli-Palestinian context it suggests that mandatory conscription has far-reaching political effects that are not yet well-documented or understood. Individuals who are socialized into violent conflict at a formative period of their lives can be deeply affected by that experience in many ways, including in their political attitudes and behavior. Given the size and impact of the ex-combatant population, peace-building efforts should take its needs and experiences into account. More generally, our findings underscore the importance of combatant reintegration programs in reducing inter-group hostility and creating the foundation for a viable, durable peace.

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10 Appendix

Table A.1: **Descriptive Statistics: Second Intifada Sample**

Instrument and treatment variable	Mean	sd	Min	Median	Max	Obs
Combat eligibility	0.8	0.4	0.0	1.0	1.0	1189
Combat exposure	0.4	0.5	0.0	0.0	1.0	1189
Political attitudes outcomes						
Reconciliation summary index	0.1	1.0	-1.4	0.0	2.8	1189
Support negotiated territorial withdrawal	0.1	1.0	-1.1	-0.4	1.6	1189
Support negotiated division of Jerusalem	0.1	1.0	-0.8	-0.1	1.9	1189
Support negotiated refugee right of return	0.0	1.0	-0.8	-0.8	2.6	1189
Palestinians are partners for peace	0.0	1.0	-0.9	-0.1	2.4	1189
Support conciliatory solutions	0.1	1.0	-1.0	1.0	1.0	1189
Israel indivisible	0.1	1.0	-1.1	-0.2	1.7	1189
Settlements endanger Israeli democracy	0.0	1.0	-1.3	-0.2	1.8	1189
Occupation is immoral	0.1	1.0	-0.7	-0.7	3.1	1189
Oppose limitations on human right NGOs	0.1	1.0	-1.0	-0.3	1.9	1189
Right Left scale self-placement	0.1	1.0	-1.3	-0.1	2.4	1189
Vote choice outcomes						
Vote choice 2013 elections	4.7	1.8	1.0	5.0	9.0	1189
Vote choice first election	4.2	1.6	1.0	4.0	9.0	1189
Political participation outcomes						
Political participation index	-0.1	0.9	-1.1	-0.4	3.7	1189
Political interest	-0.1	1.0	-2.2	-0.3	1.5	1189
Party membership	-0.1	0.9	-0.4	-0.4	3.8	1189
Demonstration	0.0	1.0	-0.4	-0.4	2.3	1189
Petition	-0.1	1.0	-1.0	-1.0	1.0	1189
Advocacy group	0.0	0.9	-0.3	-0.3	3.0	1189
Contact public official	-0.1	0.9	-0.5	-0.5	2.2	1189
Radio call	0.0	0.9	-0.2	-0.2	4.5	1189
Newspaper comment	-0.1	1.0	-0.7	-0.7	1.4	1189
Facebook political status	-0.2	1.0	-1.1	-1.1	0.9	1189
Socialization processes						
Framing support military solutions	-0.1	1.0	-1.9	-0.2	1.4	1189
Peer effect right leaning unit	0.4	0.5	0.0	0.0	1.0	1189
Prejudice						
Prejudice summary index	-0.1	1.0	-2.5	-0.2	3.2	1189
Intelligence difference	0.0	1.1	-4.0	-0.3	3.0	1189
Trustworthiness difference	0.0	1.0	-4.5	-0.2	2.7	1189
Altruism difference	0.1	0.9	-3.3	0.1	2.4	1189
Non violence difference	-0.1	1.0	-4.0	-0.2	2.7	1189
Palestinians goal to expel Jews	-0.1	1.0	-1.8	-0.1	1.5	1189
Fear and Threat						
Terror attack	-0.1	1.0	-2.2	0.2	1.0	1189
Missiles attack	0.0	1.0	-1.8	-0.3	1.2	1189
Existential threat	-0.1	1.0	-2.4	0.0	0.8	1189

Table A.2: **Descriptive Statistics: Post-Gaza Withdrawal Sample**

Instrument and treatment variable	Mean	sd	Min	Median	Max	Obs
Combat eligibility	0.8	0.4	0.0	1.0	1.0	1145
Combat exposure	0.4	0.5	0.0	0.0	1.0	1145
Political attitudes outcomes						
Reconciliation summary index	-0.2	1.0	-1.4	-0.5	2.8	1145
Support negotiated territorial withdrawal	-0.3	1.0	-1.1	-0.4	1.6	1145
Support negotiated division of Jerusalem	-0.1	1.0	-0.8	-0.8	1.9	1145
Support negotiated refugee right of return	-0.1	0.9	-0.8	-0.8	2.6	1145
Palestinians are partners for peace	-0.1	1.0	-0.9	-0.9	2.4	1145
Support conciliatory solutions	-0.2	1.0	-1.0	-1.0	1.0	1145
Israel indivisible	-0.2	1.0	-1.1	-0.2	1.7	1145
Settlements endanger Israeli democracy	-0.3	1.0	-1.3	-0.2	1.8	1145
Occupation is immoral	0.0	1.0	-0.7	-0.7	3.1	1145
Oppose limitations on human right NGOs	-0.1	1.0	-1.0	-0.3	1.9	1145
Right Left scale self-placement	-0.2	0.9	-1.3	-0.1	2.4	1145
Vote choice outcomes						
Vote choice 2013 elections	4.1	1.9	1.0	4.0	9.0	1145
Vote choice first election	3.9	1.7	1.0	4.0	9.0	1145
Political participation outcomes						
Political participation_index	-0.1	0.9	-1.1	-0.2	3.7	1145
Political interest	0.0	1.0	-2.2	-0.3	1.5	1145
Party membership	0.0	1.0	-0.4	-0.4	3.8	1145
Demonstration	0.0	1.0	-0.4	-0.4	2.3	1145
Petition	0.0	1.0	-1.0	-1.0	1.0	1145
Advocacy group	0.0	1.0	-0.3	-0.3	3.0	1145
Contact public official	-0.1	0.9	-0.5	-0.5	2.2	1145
Radio call	0.0	1.0	-0.2	-0.2	4.5	1145
Newspaper comment	0.0	1.0	-0.7	-0.7	1.4	1145
Facebook political status	-0.1	1.0	-1.1	-1.1	0.9	1145
Socialization processes						
Framing support military solutions	0.2	1.0	-1.9	0.3	1.4	1145
Peer effect right leaning unit	0.5	0.5	0.0	0.0	1.0	1145
Prejudice						
Prejudice summary index	0.1	1.0	-2.3	0.1	3.2	1145
Intelligence difference	0.1	1.1	-5.6	0.3	3.0	1145
Trustworthiness difference	0.1	1.1	-3.5	0.3	2.7	1145
Altruism difference	0.2	1.0	-2.6	0.1	2.4	1145
Non-violence difference	0.0	1.1	-4.0	-0.2	2.7	1145
Palestinians goal to expel Jews	0.1	1.0	-1.8	0.4	1.5	1145
Fear and Threat						
Terror attack	-0.1	1.1	-2.2	0.2	1.0	1145
Missiles attack	-0.1	1.1	-1.8	-0.3	1.2	1145
Existentia threat	0.2	0.9	-2.4	0.8	0.8	1145

Table A.3: **Covariate Balance**

	Second Intifada Sample				**Post-Gaza Withdrawal**			
	Below	Above	Diff	std bias	Below	Above	Diff	std bias
HH income	2.946	2.895	-0.051	-0.061	3.048	3.003	-0.045	-0.049
HH ideology	3.563	3.352	-0.211	-0.142	3.021	2.920	-0.102	-0.071
Native	0.905	0.857	-0.048	-0.137	0.845	0.847	0.002	0.005
Father combatant	0.446	0.484	0.038	0.076	0.492	0.480	-0.012	-0.024
Secular	0.680	0.603	-0.077	-0.158	0.551	0.466	-0.085	-0.171
Traditional	0.149	0.207	0.058	0.144	0.209	0.209	0.000	0.001
Religious	0.162	0.184	0.022	0.057	0.219	0.285	0.066	0.146
Sephardic	0.288	0.364	0.076	0.157	0.326	0.381	0.055	0.113
Ashkenazi	0.491	0.388	-0.103	-0.212	0.374	0.364	-0.010	-0.021
Mixed race	0.144	0.133	-0.011	-0.032	0.150	0.116	-0.034	-0.106
USSR	0.068	0.107	0.039	0.126	0.134	0.118	-0.016	-0.049
Jerusalem	0.077	0.080	0.003	0.011	0.102	0.099	-0.002	-0.008
North	0.104	0.130	0.027	0.079	0.080	0.118	0.038	0.117
Haifa	0.108	0.161	0.053	0.145	0.086	0.130	0.045	0.133
Center	0.338	0.266	-0.072	-0.163	0.305	0.289	-0.016	-0.035
Tel Aviv	0.203	0.175	-0.028	-0.074	0.155	0.147	-0.008	-0.022
South	0.135	0.133	-0.002	-0.005	0.193	0.132	-0.061	-0.180
Territories	0.036	0.055	0.019	0.082	0.080	0.085	0.004	0.016

Table A.4: Violence Exposure by Period

Witnessed injury or death:	Second Intifada period	Post-Gaza withdrawal	Difference Δ	p-value (t-test)	Obs
IDF soldier	0.312	0.245	0.067	0	2,334
Armed Palestinian/ terrorist	0.246	0.181	0.065	0	2,334
Palestinian civilian	0.168	0.144	0.024	0.10	2,334