Conflict and the Persistence of Ethnic Bias*

Moses Shayo and Asaf Zussman

The Hebrew University of Jerusalem

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Abstract

How persistent are the effects of ethnic conflict on ethnic identification and in particular on bias towards co-ethnics? We employ a measure of ethnic bias derived from decisions made by Arab and Jewish judges in Israel to study and compare levels of bias during the 2000-04 Palestinian Intifada and its aftermath (2007-10). Despite the sharp drop in violence, we find no evidence of a general attenuation in ethnic bias. Furthermore, bias in the later period remains positively associated with past intensity of violence in different localities. This persistent effect does not appear to be due to judges' personal exposure to violence but rather to different dynamics in areas that suffered more violence. This is consistent with the view that conflict can produce stable social equilibria even if identities are malleable at the individual level.

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1. Introduction

This paper seeks to contribute to our understanding of the legacies of ethnic conflict. Our focus is on ingroup bias, one of the key behavioral manifestations of social identification. While the fluidity of social identities has been well documented both in constructivist writings and in lab experiments, when it comes to large scale ethnic violence a widespread view is that conflict tends to harden ethnic identities – perhaps irreversibly.[1] Previous work established a strong short-term effect of ethnic violence in a locality on the extent of ethnic bias in that locality (Shayo and Zussman 2011). This paper examines whether the effects of conflict persist even when the violence subsides or whether, on the contrary, ethnic bias in previously afflicted areas converges towards the lower levels of bias observed in non-violent localities. Furthermore, to the extent that the effects of conflict persist, it is important to know why. One possibility, consistent with the view that conflict irreversibly solidifies ethnic identities, is that the personal experience of ethnic violence has an enduring effect on individuals' social preferences. A second possibility is that individual identities are in fact quite fluid, and yet *in equilibrium* identities can be very stable. This may happen because individuals coordinate on patterns of identification and inter-group relations (see e.g. Laitin 2007) or because conflictual relationships and ethnic identification tend to reinforce each other (see e.g. Sambanis and Shayo 2013). Our analysis offers some clues regarding the mechanisms underlying conflict's persistent effects. Finally, our study is based on judicial decisions, thereby shedding light on the effect of conflict on the functioning of an important institution: the legal system.

We define ethnic bias as the preferential treatment awarded to members of one's ethnic group. A measure of ethnic bias should arguably have the following two properties. First, it would be based on behavior in a natural setting. Second, it would reliably capture bias rather than unobserved factors. Consider the first property. No doubt, survey data as well as carefully constructed lab and lab-in-the-field experiments have yielded crucial insights into the determinants of ethnic bias. Nonetheless, survey responses are sometimes hard to interpret and may have little predictive power with

[1] Chandra (2012, ch.1) provides an overview of constructivist accounts of ethnic identity and Shayo (2009) reviews the experimental literature on the endogeneity of social identities. The summer 2004 volume of *Security Studies* contains a collection of essays debating Kaufmann's (1996) thesis that "in ethnic wars both hypernationalist mobilization rhetoric and real atrocities harden ethnic identities to the point that cross-ethnic political appeals are unlikely to be made and even less likely to be heard" (p. 137) and his argument for partition as the solution to ethnic conflicts.

respect to behavior.[2] Experiments have the advantage of cleanly identifying biases in behavior and exploring their possible causes. Nonetheless, external validity may sometimes be a concern, e.g., due to the participants being nonprofessional decision makers, acting in an artificial context, possibly characterized by low stakes.

Turning to the second property, a large literature seeks to measure ethnic, racial, and gender biases based on behavior in natural settings (e.g. courts and the labor market). However, a serious challenge facing this literature is that outcomes that seem to reflect bias may actually be driven by other factors which are correlated with ethnicity, race or gender. For instance, wage disparities along ethnic lines could be due to unobserved variables (such as ability, motivation, and quality of education) rather than to ethnically-biased employers. A similar problem plagues empirical legal studies, where many case characteristics that might affect judicial decisions are unobserved by the researcher.

Here, we utilize the measure of ethnic bias developed by Shayo and Zussman (2011) which has these two properties. The measure is derived from rulings made by Arab and Jewish judges in Israeli small claims courts in cases involving either an Arab plaintiff suing a Jewish defendant or a Jewish plaintiff suing an Arab defendant. In these courts cases are effectively randomly assigned to judges. This allows us to estimate the extent of judicial ethnic bias. Data on fatalities from the Israeli-Palestinian conflict capture exogenous temporal and spatial variation in exposure to ethnic violence. This allows us to relate the extent of the bias to the court's and the judge's exposure to violence, while controlling for factors operating at the national level that may affect both judicial decisions and conflict intensity.

Figure 1 displays the number of Israeli civilian fatalities from the Israeli-Palestinian conflict during 1998–2010. We focus on fatalities within Israel proper (i.e. excluding fatalities in the Occupied Territories and Lebanon) since this is where the courts we study are located. Effects of violence in other areas cannot be identified and will be absorbed in our empirical analyses by time fixed effects and overall trends. Looking at the figure, one can distinguish between three periods. The period from 1998 to late 2000 was relatively peaceful. The period from September 2000 to the end of 2004 is known as the Second Palestinian Intifada (uprising). This period was characterized by intense conflict between Israel and the Palestinians in the West Bank and the Gaza Strip, with much of the violence taking place within Israel. Violence subsided in subsequent years, with 2007-2010 characterized again by relative calm.

[2] See Zussman (2013) and references therein on the relation between stated attitudes and discriminatory behavior.

Henceforth we refer to 2000-2004 as the "conflict period" and to 2007-2010 as the "post-conflict period" but it is worth remembering that the Israeli-Palestinian conflict, in the wider sense of the term, did not start in 2000 and has not reached a peaceful conclusion yet.

[Figure 1]

Shayo and Zussman (2011) analyzed the conflict period. During this period judges exhibited a significant degree of ethnic bias. A case was around nineteen percentage points more likely to be accepted when assigned to a judge of the same ethnicity as the plaintiff. Furthermore, the extent of the bias was strongly associated with the intensity of ethnic violence in the vicinity of the court in the period immediately preceding the trial. Courts that had seen limited violence exhibited little bias. Based on these findings, one might conjecture that a decline in violence should lead to a reduction in bias, especially in those courts where violence dropped most significantly.

This paper examines the actual legacy of the conflict. We are mainly interested in three questions. First, can a sharp decline in violence, such as that documented in Figure 1, indeed help improve the functioning of the legal system and in particular reduce ethnic bias? Second, how persistent are the local effects of exposure to violence on ethnic bias? Third, what can we learn about the channels of persistence?

The results are sobering. First, there is no evidence of an overall attenuation in bias as violence subsides. In the post-conflict period, claims were still around eighteen percentage points more likely to be accepted when assigned to a judge of the same ethnicity as the plaintiff. Furthermore, we observe no downward trend in ethnic bias within the post-conflict period.

Second, ethnic bias in the post-conflict period remains positively associated with local exposure to violence during the height of the conflict. This association is not due to any other observable differences across the localities where the courts are located – despite the fact that some such differences are associated with the level of bias (courts located in larger towns exhibit less bias and there is some indication that so are courts located in mixed Arab-Jewish towns). Indeed, while bias is positively associated with the intensity of political violence around the court in preceding years, the intensity of violence itself is not associated with bias in the court in preceding years. Nonetheless, the association between judicial bias and past violence declines compared to the conflict period. Taken together, this and the first set of results suggest some

convergence across courts toward a relatively high level of judicial bias in the post-conflict years.

Third, what is the source of persistence? Broadly, ethnic violence could have a persistent effect on the legal system via at least two distinct channels. The first operates through the personal experience and memories of individual judges. This channel is consistent with studies indicating that exposure to political violence produces psychological distress and increased threat perceptions, which may lead to increased militancy and to hostility towards minority groups (Canetti-Nisim et al. 2009, Hirsch-Hoefler et al. forthcoming). Such effects could conceivably persist even when the level of threat has subsided. Thus, judges who were exposed to violence are, in a sense, "scarred". But violence might produce enduring changes in exposed courts independently of the individual experiences of present-day judges. Violence during the time of the conflict can change the patterns of inter-ethnic relations in the region where the court is situated. Increased tensions between the groups raise the salience of ethnic cleavages in the region, thereby increasing the likelihood of ethnic identification which in turn reinforces inter-ethnic tensions (see Sambanis and Shayo 2013 for a theoretical analysis of this process). Thus, even individuals who were not personally exposed to much violence during the conflict may exhibit ethnic bias. Attempting to shed some light on the sources of persistence, we collect data on judges' personal history of exposure to violence. This allows us to try and disentangle the two potential mechanisms. The evidence seems more in line with the second: the persistent effect of exposure to violence that we identify operates primarily at the local (court) level rather than at the individual (judge) level.[3]

The paper contributes to a recent line of research that has begun to uncover the effects of conflict on political attitudes (Berrebi and Klor 2008; Gould and Klor 2010; Lyall et al 2013), social, risk, and time preferences (Voors et al. 2012; Cassar, Grosjean and Whitt 2013; Rohner, Thoenig and Zilibotti 2013; Callen et al. 2014; Gilligan, Pasquale and Samii 2014) and political participation (Bellows and Miguel 2009; Blattman 2009). Our contribution to this literature is in studying the impact of ethnic conflict on

[3] Whether the individual or the social mechanism is at work can be important to help guide post-conflict policies. Of particular relevance are Community Driven Reconstruction programs which seek to enhance local cooperation by encouraging individuals in a community to work together; and Truth and Reconciliation programs which emphasize psychological healing (see, respectively, Fearon, Humphreys and Weinstein (forthcoming) and Cilliers, Dube and Siddiqi (2015) for recent discussions and program evaluations).

intergroup relations and on the functioning of institutions. While some of the existing literature finds that exposure to violence can increase pro-social behavior or public spiritedness, it is unclear whether such favorable changes in behavior are biased towards one's own ethnic group. Ingroup bias plays a crucial role in intergroup relations and may contribute to the emergence of vicious cycles where violence breeds ethnic identification and ethnic identification intensifies conflict.

Concerning the relation between conflict and institutions, what we know is largely based on cross-country comparisons. Collier et al. (2003) report that indices of democratic institutions (from Polity IV) and of political freedom (from Freedom House) tend to deteriorate in the decade following civil war. Besley and Persson (2008) find that tax collection as a percentage of GDP (a proxy for state capacity) is lower in countries that experienced internal conflict. While instructive, such results are hard to interpret: countries with bad or deteriorating institutions may be more likely to experience war for many reasons. To our knowledge, this paper is the first to employ micro data to analyze the workings of legal institutions during and after a period of intense ethnic violence.

Several recent studies examine the persistence of biases, norms and preferences. The evidence suggests, for example, that trust, anti-Semitism, attitudes towards redistribution, and perceptions of gender roles can persist over very long horizons (see, respectively, Nunn and Wantchekon 2011; Voigtländer and Voth 2012; Alesina and Fuchs-Schündeln 2007; Alesina, Giuliano and Nunn 2013). Besley and Reynal-Querol (2014) find that the intensity of conflict in pre-colonial Africa is positively correlated with present-day ethnic (as opposed to national) identification and negatively correlated with inter-group trust. But while norms and identities can persist, it is well established that they can and do change, sometimes quite rapidly. Here, we closely examine the extent to which one crucial form of bias persists in the face of a rather dramatic change in the environment.

In terms of methodology, the paper builds on the (largely experimental) literature on social identity and ingroup bias (e.g. Tajfel et al. 1971; Bornstein 2003; Habyarimana et al. 2007; Chen and Li 2009; Fong and Luttmer 2009; Klor and Shayo 2010). One of the workhorse tools for studying ingroup bias has been the Minimal Group Paradigm introduced by Tajfel et al. (1971). In these lab experiments, subjects are assigned to groups (typically based on some trivial task) and are then asked to make allocation decisions between two other anonymous subjects, one belonging to their group (the ingroup) and the other belonging to the outgroup. Typically, subjects in these experiments exhibit ingroup bias and various studies examine the factors that enhance or reduce this bias. Our setting is unique in that it replicates some of the key properties

of these experiments, but with professional decision makers acting in their natural environment and with ethnic (rather than minimal) groups.

Finally, the study relates to the literature on ethnic and racial bias in the legal system (e.g. Glaeser and Sacerdote 2003; Abrams, Bertrand, and Mullainathan 2012; Anwar, Bayer, and Hjalmarsson 2012; Alesina and La Ferrara 2014, Grossman et al. 2015). Beyond documenting persistent ethnic bias, our contribution to this literature is in helping to understand the endogeneity of such biases to the history of intergroup relations.

2. Institutional and historical background

We analyze judicial decisions involving Arabs and Jews in Israeli small claims courts in 2000–2010. During this time, Arab citizens account for around 20% of Israel's population while Jewish citizens account for 76%.[4] Arab-Jewish relations within Israel are closely linked to developments in relations between Israel and the Palestinians in the Occupied Territories. Arab Israelis and the Palestinians of the Occupied Territories share a common heritage and many in the latter group have roots in towns and villages located in present-day Israel.

Small claims courts handle civil cases between private litigants, such as those involving minor traffic accidents. The courts have a cap on the amount of money they can award. This cap increased from NIS 17,800 in 2000 to NIS 31,200 in 2010 (roughly $US 4,000-8,000). The judge can also award legal expenses (this is entirely discretionary). The legal procedure is relatively simple. It starts when the plaintiff files a claim at the court, provides supporting documentation, and pays a small fee. The defendant is then notified and instructed to provide a defense statement within fifteen days. After a claim has been filed, the case is assigned a trial date and a judge. Due to a backlog in the system, trials are scheduled several months in advance. Importantly, each case is assigned to the first available slot; this means that the assignment of judges to cases within a given court is in principle orthogonal to characteristics of the case (we test this below).

[4] Israeli Central Bureau of Statistics, *Statistical Abstract of Israel 2013*. The remaining 4% are mostly immigrants from the former Soviet Union who are not officially classified as Jews.

The judge receives the case materials no earlier than a week before the trial. The trial itself typically lasts only a few minutes and the judge issues a ruling within seven days. Importantly, the litigants (rather than lawyers) present their case.

This setting has several attractive features for measuring ethnic bias. Most importantly, in these courts an effectively randomly assigned decision maker needs to make an allocation decision between two individuals, one of whom belongs to her ethnic group while the other does not. Compared to lab experiments designed to measure ingroup bias (e.g., Tajfel et al. 1971; Chen and Li 2009), decision makers are professional, the monetary stakes are high, and the groups to which the agents belong (Arabs and Jews) are not artificially constructed in the lab. Since decisions have to be made within a week of the trial, we also know their approximate timing. This allows us to relate the decisions to the context in which they are made and in particular to the intensity of the conflict.

We concentrate on two periods. The first is the conflict known as the Second Intifada, which started in late 2000 and lasted until the end of 2004.[5] It was characterized by intense violence between Israelis and Palestinians and claimed the lives of thousands. A major part of Palestinian violence took the form of suicide bombings carried out within Israel proper, where the courts we study are located. Importantly, this violence was not generated by the local population. In particular, participation of Arab Israelis in terrorist attacks was negligible in scale throughout the conflict period. Violence reached a peak in 2002 and then gradually declined (see Figure 1). The second period we study (2007–2010) was relatively tranquil and saw only 36 civilian fatalities within Israel compared to 514 in the first period.[6]

[5] The Intifada started in late September 2000. There is no agreed-upon date for the end of the Intifada. Two prominent candidate dates are November 11, 2004, when Yasser Arafat, President of the Palestinian National Authority, died, and February 8, 2005, when the Sharm el-Sheikh Summit was convened to end the Intifada. In this paper we use December 31, 2004, as the cutoff date for the end of the Intifada (none of the results depend on this decision).

[6] The post-Intifada period did see some outbreaks of violence, including the Second Lebanon War in the summer of 2006, which did not involve the Palestinians; and the winter of 2008–2009 Israeli military operation in the Gaza Strip. Both events involved a relatively small number of casualties within Israel. In the analysis that follows we control for these and other incidents of violence in the post-Intifada period.

3. Data

This section briefly outlines the data collection procedures. Details are provided in Appendix A. This appendix also reports comprehensive descriptive statistics.

Judicial decisions.

The main source of data is online transcripts of judicial decisions (rulings). We cover the universe of available documents (26,444 from 2000–2004 and 28,576 from 2007–2010). For each document we code the ethnicity of private litigants based on their names and the relative frequency of Arab and Jewish names in the Israel Population Registry. We keep only "mixed cases": those where at least one private plaintiff and one private defendant are of different ethnicities (N=4,038).[7] For these cases we conduct a comprehensive analysis of the documents, coding information on the court, judge, litigants, claim characteristics and judicial outcomes. We obtain additional socio-demographic information on judges from their biographies.

The main analysis in this paper excludes cases that were settled outside the court (325 cases) or withdrawn (303) as well as cases that have multiple plaintiffs (or defendants) such that one plaintiff (or defendant) is Jewish and another is Arab (305).[8] Finally, we exclude cases where the court is located in the Occupied Territories (1). This leaves us with 3,153 cases, 1,748 for 2000–2004 and 1,405 for 2007–2010.

Table 1 shows for each period the percentage of cases by the ethnicity of the judge, plaintiff, and defendant. Overall, 29% of the cases in our data were ruled by Arab judges. This share dropped from 31% to 25% across the two periods. There is also a small decline in the share of Arab plaintiffs, from 44% in the conflict period to 41% in the post-conflict period.

[Table 1]

Our main measure of trial outcome is an indicator for whether the claim was accepted. Out of the 3,153 cases in our main sample, 2,300 (73%) are coded as

[7] Focusing on mixed cases allows us to reliably measure ethnic bias, which is our primary interest. A comprehensive analysis of the universe of cases could perhaps allow us to answer additional questions but would be prohibitively costly.

[8] We find no evidence for differences in the likelihood that plaintiffs settle cases outside the court or withdraw them when assigned a judge of the same or opposite ethnicity. We similarly find no evidence that such differences are associated with court exposure to violence during the conflict. See Appendix B.

accepted. In the Online Appendix we also provide results using alternative outcome measures such as monetary compensation and legal expenses.

Exposure to violence.

We employ data on all Palestinian politically motivated fatal attacks inside Israel. For each attack we have information about date, location, and number of civilian fatalities.

Our first set of measures of exposure to violence is at the level of the court. These measures are based on the number of fatalities from attacks that occurred in the vicinity of the court during the conflict period. Vicinity is defined by three alternative geographical units: *natural area* (smallest geographical unit around the court) *sub-district* and *district*. Our data spans 24 natural areas, 15 sub-districts, and 6 districts.

The second set of measures is at the level of the judge. We focus on exposure at the judge's place of employment rather than at the judge's place of residence primarily because the latter is confidential and generally difficult to obtain. We use several sources to compile information on judges' employment history since 2000 (both as judges and in other jobs such as lawyers). Our procedure yields monthly location data for the entire Intifada period for 196 (82%) of the 240 judges in our sample. For an additional 37 judges we have partial information and for 7 judges we have no location information for the conflict period. Merging the location information with the fatalities data provides a measure of the number of fatalities each judge was exposed to in her place of employment in each month of the conflict.

4. Ethnic bias across periods

Are claims more likely to be accepted when assigned to a judge of the same ethnicity as the plaintiff? The random nature of the assignment of cases to judges within a court provides a straightforward method for credibly addressing this question. Our identification assumption is that given the court, the ethnicity of the plaintiff, and the ethnicity of the judge, cases assigned to a judge of the same ethnicity as the plaintiff are not systematically different from cases assigned to a judge of a different ethnicity. Notice in particular that we allow plaintiffs from different ethnicities to file cases with different characteristics. We also allow judges of different ethnicities to receive cases with different characteristics. Appendix C evaluates the validity of our identification assumption using the observed case characteristics (e.g. the number and characteristics of plaintiffs and defendants, the subject of the claim and other case characteristics). The evidence strongly supports the validity of the identification assumption.

4.1 Results

We first present the raw data and then proceed to an econometric analysis. Figure 2 displays the share of claims accepted for each period by judge and plaintiff ethnicity. The left box replicates Shayo and Zussman's (2011) results from the conflict period. The left pair of bars in this box pertains to cases where the plaintiff is Jewish and the defendant is Arab. Jewish judges accept 79.1% of these cases while Arab judges accept 71.7%. In itself, this difference of 7.4 percentage points is not necessarily evidence of ethnic bias: for example, it may be the case that compared to their Arab colleagues, Jewish judges are more inclined to accept claims. However, if this were the only reason for the difference, we would expect to observe a similar pattern regardless of plaintiff ethnicity. In fact, the right pair of bars in the same box shows that when the plaintiff is Arab, the pattern is reversed: Jewish judges are 10.6 percentage points *less* likely than Arab judges to accept such claims. The difference in these differences – 18 percentage points – provides an indication of the extent of ethnic bias (i.e., by how much Jewish judges are more likely than their Arab colleagues to accept a claim filed by a Jewish plaintiff rather than by an Arab one). From now on we will use this measure to track the evolution and identify sources of ethnic bias. It should be emphasized that this is a measure of *overall* bias: absent an ethnicity-free benchmark, it is impossible to say whether and to what extent Jewish judges favor Jewish litigants and Arab judges favor Arab litigants.

[Figure 2]

We can now turn to the post-conflict period (right box). Compared to the conflict period, we do observe some changes (e.g., Jewish judges accept a lower share of claims, while Arab judges accept a higher share). Remarkably, however, the overall level of bias is stable at 17.3 percentage points. Note, again, that it is impossible to say to what extent the bias is due to the behavior of Arab or Jewish judges. For example, looking at the right box, one might think that the bias is mainly driven by the behavior of Jewish judges (who accept a significantly lower proportion of cases filed by Arab plaintiffs). However, one cannot rule out the possibility that cases filed by Jewish plaintiffs are systematically stronger than those filed by Arab plaintiffs, in which case the decisions by Arab judges are biased.

A more detailed look at the evolution of bias over time is provided in Figure 3. The figure presents our difference-in-differences measure of ethnic bias, but this time it is calculated in two-year moving windows centered around a particular date. We start with the first observation in each period (conflict and post-conflict) and advance in 30-day intervals until the end of the period. To reduce noise, the analysis is restricted to

windows with at least 100 observations. The results show the bias fluctuating around 15–20 percentage points during both the conflict period and the post-conflict period, with no clear downward trend following the decline in violence.

[Figure 3]

We now turn to an econometric investigation. We start with a benchmark difference-in-differences specification:

$$(1) \quad y_{ijct} = \alpha_0 + \alpha_1 ArabPlaintiff_i + \alpha_2 ArabJudge_i + \alpha_3 ArabPlaintiff * ArabJudge_i + \delta_c + \varepsilon_{ijct}$$

where y_{ijct} is the outcome of case i assigned to judge j, in court c, at time t; $ArabPlaintiff$, $ArabJudge$ and the interaction term $ArabPlaintiff * ArabJudge$ are indicator variables; δ_c is a court fixed effect; and ε_{ijct} is an error term clustered within judge.

Consistent with our identification assumption, equation (1) allows for two possible differences across ethnic groups that do not necessarily indicate ethnic bias. First, it is possible that claims submitted by Arab plaintiffs have different unobserved characteristics than those submitted by Jewish plaintiffs. Thus, α_1 may be nonzero even in the absence of ethnic bias. Second, it is possible that Arab and Jewish judges are differently inclined to accept claims. In other words, α_2 may be nonzero even in the absence of ethnic bias. Judicial ethnic bias is captured by α_3. This coefficient is similar to the difference-in-differences measure used in Figures 2 and 3, except that we include court fixed effects to allow for differences in acceptance rates across courts. Columns 1–2 in Table 2 present these benchmark results for the conflict period and the post-conflict period, respectively. Consistent with Figure 2, the estimated ethnic bias is similar in magnitude across periods (and highly statistically significant in both).

[Table 2]

In columns 3–4 we augment equation (1) with a large set of controls. We now estimate:

$$(2) \quad y_{ijct} = \alpha_0 + \alpha_1 ArabPlaintiff_i + \alpha_3 ArabPlaintiff * ArabJudge_i + \delta_c$$
$$+\gamma_j + \lambda tenure_{jt} + X_i'\beta + \eta_t + \varepsilon_{ijct}$$

where γ_j is a judge fixed effect and $tenure_{jt}$ is the judge's tenure at the job.[9] The vector X_i is a list of case-specific controls (see notes to Table 2) and η_t is a vector of year, month, and day-of-week indicators. Compared to columns 1–2, the explanatory power of the regressions increases substantially. However, consistent with random assignment of cases to judges, the estimates of ethnic bias hardly change.[10]

In Appendix D we estimate equation (2) using four alternative outcome measures such as the monetary compensation awarded by the judge. The results are in line with those of Table 2: for the most part, the estimated bias is similar across periods. The main exception is that the bias measured in net monetary compensation decreases from NIS 926 (about $210) during the conflict period to 391 during the post-conflict period (panel A, columns 3–4). However, this decrease is driven by a small number of cases involving extraordinarily high monetary compensation. Excluding from the analysis the top and bottom one percent of cases in terms of the outcome variable, we find no evidence of a decline in bias across periods (panel B).

Before turning to an analysis of the relationship between judicial bias and violence, several issues are worth discussing. First, given that Arab and Jewish judges have different personal characteristics, it is possible that the estimated α_3 in equation (2) reflects these differences rather than ethnic bias per se. For example, Arab judges are more likely than Jewish judges to be male (66% vs. 49%). If for some reason male judges systematically treat Arab plaintiffs differently from Jewish plaintiffs, then α_3 would pick up this difference and hence would not be an accurate measure of ethnic bias. To address this possibility, in Appendix E we augment equation (2) with interactions between the *ArabPlaintiff* indicator and judge characteristics. None of

[9] Adding the judge fixed effect implies dropping the $ArabJudge$ indicator from the model since the fixed effect picks up any time-invariant judge characteristic. We keep the court fixed effect δ_c as some judges move between courts.

[10] The results in Table 2 are not driven by the decisions of a single judge. To check this, we repeat the analysis of columns 3 and 4, each time excluding decisions made by a single judge. Estimates of ethnic bias range between 0.158 and 0.207 in the conflict period and between 0.156 and 0.202 in the post-conflict period.

these added interactions meaningfully affect our estimate of ethnic bias (Appendix Table E1, second row).

Second, one could argue that the empirical pattern we observe is not due to preferential treatment of members of one's own group but rather to difficulties in communication between judge and plaintiff when they do not share the same mother tongue. Even if true, our results point to a severe malfunction in the legal system, as similar cases are not treated alike. However, it is hard to see why technical differences in the quality of communication between Arabs and Jews should respond (as we show below) to the intensity of violence in the vicinity of the court. Note also that cases are typically very simple (e.g., "fender-bender" accidents) and the decision is basically about whose version of the events to accept and as such should not rely on linguistic subtleties.

A third issue relates to the possibility that the strength of claims submitted by plaintiffs might depend on the ethnic makeup of the court. For example, Arab plaintiffs may file more "marginal" claims – and Jewish plaintiffs only "strong" claims – the higher is the share of Arab judges in the court. This can generate downward bias in our estimate of ethnic bias. To help assess this possibility we collect data on the ethnicity of all judges in each court and year, including judges that did not end up ruling in mixed-ethnicity cases. The data are derived from the official biographies of all current and retired judges and the computerized archive of judicial decisions described in Appendix A. We then augment equation (2) with two variables: the share of Arab judges in court c in year t and the interaction of this share with the $ArabPlaintiff$ indicator. If the above hypothesis is correct, we would expect the likelihood of a claim being accepted to be positively associated with the share variable and negatively associated with the interaction term. The two coefficients turn out to be statistically insignificant while the estimate of ethnic bias rises slightly (results not shown).[11]

A final issue worth addressing is whether the degree of ethnic bias varies with judge characteristics. For example, it is possible that more experienced or more educated judges exhibit less bias. Overall, we find little evidence for such heterogeneity in bias across judge characteristics (Appendix F), although in unreported results we find some indication that within the post-conflict period older and male judges are less biased.

[11] Recall also that we find no evidence of selective withdrawal of cases or their settlement outside of court depending on the ethnicity of the judge – which could similarly bias our estimate of ethnic bias (Appendix B).

5. Judicial bias and court exposure to violence

So far we have studied the evolution of judicial ethnic bias in the entire country. As we have seen, the dramatic drop in violence was not accompanied by a decline in bias. However, bias may well be affected by country-wide factors other than violence that vary over time. In this section we therefore explore whether judicial bias in the post-conflict period is associated with a court's *local* exposure to violence during the conflict. This allows us to better identify the causal effect of violence, as we can keep constant any country-wide determinants of bias. Note however that this approach probably yields an underestimate of the overall effect of violence, since Israel is a small country where national media outlets prominently cover any act of political violence.

Our identification assumption is that as local exposure to violence during the conflict period increases, cases assigned to a judge of the same ethnicity as the plaintiff do not become systematically different from cases assigned to a judge of a different ethnicity. Appendix G examines and finds support for the validity of this assumption.

5.1 Results

Figure 4 illustrates some broad patterns in the data. Using the same methodology as Figure 3, it shows simple (nonparametric) difference-in-differences estimates of judicial bias. We split the sample into two groups of courts based on the court's exposure to violence during the conflict. The first group ("low violence courts") consists of eight courts that saw no fatalities in their respective natural areas during the entire conflict period. The second group consists of all other courts. [12] During the conflict period, courts that saw no violence are consistently less biased than the other courts. In fact, the overall level of bias in this period is 0.05 in the former group and 0.23 in the latter. After the violence subsides, the difference between the two groups appears to narrow. The overall post-conflict bias is 0.15 in the low-violence courts and 0.23 in the high-

[12] We have 915 cases in the low-violence group of courts (454 of them in the conflict period) and 2,238 in the high-violence group (1,294 of them in the conflict period). The distinction between the two groups is for illustrative purposes only. Some of the point estimates for the high-violence courts are based on samples that include cases in courts that are yet to experience fatalities. Nonetheless, the figure is useful as it keeps the set of courts in each group fixed. In Appendix H (discussed below) we conduct placebo tests to check whether courts exposed to more violence were more biased to begin with. We find no evidence of this being the case.

violence courts. Contrary to what one might have expected, the two groups seem to converge to the level of bias previously associated with the *high-violence* courts.

[Figure 4]

We now examine more closely the relationship between exposure to violence and bias in the two periods. Consider the following equation:

$$(3)\quad y_{ijct} = \alpha_0 + \alpha_1 ArabPlaintiff_i + \alpha_3 ArabPlaintiff * ArabJudge_i$$
$$+\theta_0 Exposure_{ct} + \theta_1 Exposure_{ct} * ArabPlaintiff_i + \theta_2 Exposure_{ct} * ArabJudge_i$$
$$+\theta_3 Exposure_{ct} * ArabPlaintiff * ArabJudge_i$$
$$+\delta_c + \gamma_j + \lambda tenure_{jt} + X_i'\beta + \eta_t + \varepsilon_{ijct}$$

where $Exposure_{ct}$ is a measure of the number of fatalities in the vicinity of court c in a given time window. All other variables are defined as in equation (2). The association between exposure and bias is captured by θ_3, while α_3 now captures the "baseline bias," i.e., the level of bias when exposure to violence is zero.[13]

Column 1 of Table 3 shows the results from estimating this equation for the conflict period, using as our exposure variable the number of fatalities in the vicinity of the court in the year preceding the trial (divided by 100 for ease of exposition). The estimate in the bottom row suggests a strong and highly significant relationship. An additional fatality (in the natural area of the court) is associated with a 0.618 percentage-point increase in bias. Baseline bias is estimated at 0.135 (second row). This can be compared to the overall level of bias during this period, which is estimated at 0.192 (column 3 of Table 2). A qualitatively similar pattern emerges at the sub-district and district levels (columns 4 and 7 of Table 3).

[Table 3]

These results suggest a strong short-run effect of violence on bias. How persistent is this local effect of violence? To help address this question we need to compare (a) the association between exposure to violence during a given time interval and bias in a period immediately following it and (b) the association between exposure to violence during that same interval and bias in a later period. To that end, we use as

[13] We continue to cluster standard errors by judge. Clustering by geographical areas (a natural alternative given that exposure varies by region) yields significantly smaller standard errors than clustering by judge. We adopt the more conservative approach.

our measure of exposure the cumulative number of fatalities in the vicinity of the court during the first three years of the conflict (2000–2003). This measure can potentially affect judicial decisions both in the last year of the conflict (2004) and in the post-conflict period (2007–2010). In terms of equation (3), this implies that our exposure variable no longer varies over time within courts; that is, we replace $Exposure_{ct}$ with $Exposure_{c}$.[14]

The results for exposure at the natural-area level are in columns 2 and 3. In column 2, the sample includes cases from the last year of the conflict. In column 3 the sample covers the post-conflict period. The estimates indicate that 2000–2003 fatalities are positively associated with bias in decisions taken both in 2004 and in 2007–2010. However, the point estimate is smaller for cases decided in the post-conflict period. An additional fatality in the natural area of the court during the first three years of the conflict is associated with an increase in bias of 0.467 percentage points in the final year of conflict and of 0.280 percentage points in the post-conflict period.[15] A similar pattern is observed when examining exposure at the sub-district level (columns 5–6) and at the district level (columns 8–9). Note also that, regardless of the geographical level of aggregation, the estimated bias in courts that saw no fatalities in 2000–2003 (second row) appears to be slightly higher in the post-conflict period than in the last year of the conflict. In other words, the results seem consistent with an increase in baseline bias.

Table 4 examines the association between judicial decisions in the post-conflict period and court exposure during the *entire* conflict period (2000–2004).[16] As seen in the bottom row, the association between exposure and bias is always positive. For instance, the point estimate in the first column implies that an additional fatality (in the natural area of the court) during the entire conflict period is associated with a 0.256 percentage point higher bias post-conflict.

[Table 4]

[14] Since we have court fixed effects we cannot identify θ_0. We also cannot identify θ_2 for the 2004 sample since Arab judges in this sample did not move between natural areas.

[15] A formal test cannot reject the equality of these two coefficients. The same is true for other differences across periods highlighted in Table 3 (both in the baseline bias and in the association with exposure).

[16] Because there was some violence during the post-conflict period, the regressions in Table 4 also control for recent fatalities in the vicinity of the court (fully interacted with the ethnicity variables). This does not change the main results. We did not include these controls in Table 3 to preserve the same functional form for the 2004 and 2007-10 samples.

While our estimates of θ_3 seem to suggest a positive association between judicial bias and past exposure of the court to violence, one needs to be careful in interpreting the association as causal. Much of the analysis in Tables 3-4 relies on cross-sectional variation in exposure. This raises the concern that other factors that vary at the court level might be driving the results. For example, in our data more populous cities suffer from a larger number of fatalities. If, for some reason, larger cities are more biased, this would lead us to over-estimate θ_3. Table 5 examines this possibility for various traits of the towns in which the courts are located (e.g. population, education and income).[17] We augment the specification of Table 4 with interactions between town traits and the set of ethnicity indicators:

$$
\begin{aligned}
(4) \quad y_{ijct} = {} & \alpha_0 + \alpha_1 ArabPlaintiff_i + \alpha_3 ArabPlaintiff * ArabJudge_i \\
& + \theta_1 Exposure_c * ArabPlaintiff_i + \theta_2 Exposure_c * ArabJudge_i \\
& + \theta_3 Exposure_c * ArabPlaintiff * ArabJudge_i \\
& + \mu_1 TownTrait_c * ArabPlaintiff_i + \mu_2 TownTrait_c * ArabJudge_i \\
& + \mu_3 TownTrait_c * ArabPlaintiff * ArabJudge_i \\
& + \delta_c + \gamma_j + \lambda tenure_{jt} + X_i'\beta + \eta_t + \varepsilon_{ijct}
\end{aligned}
$$

For each of the town traits, Table 5 reports the estimated θ_3 and μ_3, where the exposure variable is the number of fatalities in the natural area around the court during the conflict. For ease of comparison, the first column replicates the baseline results of column 1 in Table 4.

[Table 5]

We begin (in column 2) by controlling for whether the town is ethnically mixed, i.e. has significant numbers of both Arab and Jewish residents.[18] Whether ethnic bias differs across ethnically mixed and ethnically homogeneous towns is an interesting question in its own right (beyond testing if the results in Table 4 might be driven by this town trait). In particular, the "contact hypothesis" (Allport 1954) argues that contact

[17] Data on these traits come from the Israeli Central Bureau of Statistics. We use data from 2006. The traits we examine vary little over time.

[18] More than two thirds of Arab Israelis live in strictly Arab localities. Most of the rest live in localities defined by the CBS as "mixed": localities in which the majority of the population is Jewish, but which have a significant minority of Arab residents (e.g. Jerusalem).

between groups should, under certain conditions, reduce tensions between them. It is well recognized, however, that simply bringing the groups into closer proximity with one another is no guarantee of social harmony and might even worsen the situation by making the groups psychologically more salient (Brewer and Brown 1998).[19] Similarly, Kaufmann (1996) argues that localities with mixed ethnic populations create security dilemmas that intensify violence. Of the twenty five courts we study, five are located in mixed towns. The results in column 2 indicate that these mixed towns are indeed associated with lower bias, but the association is not statistically different from zero. At the same time, the estimate of θ_3 remains positive and statistically significant.

For the remaining town traits, we construct indicators that split the towns at the median value of each trait in the sample. We look at population size; mean per capita income; education (measured by the high school completion rate); the proportion of families with four children or more (this might be viewed as a crude proxy for religiosity); and the median age. Results are in columns 3-7. In all cases, the association between exposure to violence and bias maintains its size and statistical significance (if anything, the estimated θ_3 tends to increase). Interestingly, except for population size, none of the traits appears to be systematically associated with ethnic bias. As for population size (column 3), we find that controlling for exposure to violence, larger towns exhibit less bias.

The results in Table 5 notwithstanding, one might still worry that fatal terrorist attacks occur more often in regions that are characterized by greater bias to begin with. This is unlikely since, as explained earlier, the violence is not initiated by the local population and is motivated by national rather than local considerations. Even so, this concern is worth addressing directly. One way to do this would be to examine the relationship between judicial decisions made in the *pre-conflict* period and our measure of court exposure to violence during the conflict. Unfortunately, the court documents required for the analysis became available online only in late 2000 (after the conflict started). Nevertheless, we can employ a similar approach using data on judicial decisions that were made before the conflict was over. These data allow us to examine whether judicial bias at the court in the early years of the conflict is associated with exposure of the court to violence in the last year of the conflict.

[19] Indeed, in his study of ethnic discrimination in the Israeli used cars market, Zussman (2013) finds that Jewish car sellers residing in mixed localities tend to discriminate more than others against Arab buyers.

In Appendix H (Table H1) we estimate the full specification from Table 4, using fatalities from the last year of the conflict as our measure of court exposure to violence. We do this separately for two samples of judicial decisions: one from the early years of the conflict (2000–2003) and the other from the post-conflict period (2007–2010, as in Table 4). This is done for all three geographical areas. Results are consistent with the causal interpretation of the association between violence and bias. Court exposure in the last year of the conflict is always positively associated with judicial bias in the post-conflict period (although the association is statistically insignificant in one case). In contrast, ethnic bias in 2000–2003 is not positively associated with the court's exposure to violence in 2004.

Another way to address the concern of spurious correlation uses variation in violence intensity throughout the conflict period. Specifically, we examine whether ethnic bias during the conflict period is associated with fatalities in the vicinity of the court in the year *following* the trial as well as in the year *preceding* the trial. The full results are reported in Appendix H (Table H2). For all geographical regions, bias is strongly and positively associated with past fatalities and not at all associated with future fatalities. Thus, there is no evidence to suggest that bias-prone courts were exposed to disproportionately more violence.

To sum up, during the conflict period ethnic bias in judicial decisions is strongly correlated with exposure to violence. This association weakens but persists into the post-conflict period. Taken together with the results of the previous section, which show that the overall level of bias does not decline, this pattern suggests a convergence across courts toward a relatively high level of bias in the post-conflict period.

6. Channels of persistence

Our analysis indicates a positive relationship between ethnic bias and past exposure to violence at the court level. What drives this persistent effect? One possibility is that ethnic violence produces enduring changes in exposed courts due to local dynamics, such as ethnic tensions in the locality where the court is situated or in the court itself. Alternatively, violence may have a persistent effect on the legal system via the personal experience and memories of individual judges, regardless of where they currently work. We attempt to disentangle these two mechanisms by exploiting the fact that judges do not stay in the same location throughout the period under investigation.

As detailed in Appendix A, we collect data on the number of fatalities each judge was exposed to in her place of employment in each month of the conflict. From these data we construct several measures of personal exposure. These measures are all based on the number of fatalities in the natural area surrounding the judge's place (or places) of employment during the conflict period.[20] Again, it is instructive to start by examining the raw data.

In Figure 5 we distinguish between cases handled by judges with "low personal exposure" and judges with "high personal exposure". The former category consists of judges who worked throughout the conflict period in natural areas that saw no fatalities, while the latter consists of judges who worked in areas with a positive number of fatalities. The first bar in each category shows the simple difference-in-differences estimate of ethnic bias during the conflict period. While ethnic bias is 8% in cases ruled by judges with low personal exposure, it is 21% for judges with high personal exposure. Note, however, that since the judge's place of employment is for the most part the court in which she rules, it is difficult to say whether this difference is due to personal exposure or to court exposure. The correlation between this measure of personal exposure and the analogous indicator of positive court exposure is 0.77. In the post-conflict period, however, cases are often handled by judges who were not working in the same region during the conflict period. As a result, the correlation between the personal and court exposure indicators drops to 0.33. This allows us to get a rough sense of the separate effects of court and personal exposure.

[Figure 5]

Consider post-conflict cases handled by judges with low personal exposure, and now distinguish between them according to court exposure during the conflict (no fatalities versus a positive number of fatalities). Ethnic bias is only 2% in the cases ruled in low violence courts while it is 20% in cases ruled in high violence courts (note however the very wide confidence intervals). Remarkably, the pattern is similar for cases ruled by judges with high personal exposure, with bias of 5% in low violence courts and 27% in high-violence courts. There seems to be more ethnic bias (20%) in decisions made in high-violence courts by judges with low personal exposure, than in decisions made in low-violence courts by judges with high personal exposure (5%). In

[20] In principle, it would be useful to also know the judge's exposure at her place of residence and whether relatives and friends of the judge were killed or injured by ethnic violence. Such information is difficult to collect since a judge's place of residence is confidential (surveying judges requires approval by the chief justice of the Supreme Court).

other words, court exposure seems to matter much more than personal exposure: where you stand depends on where you sit.

To examine this more carefully, we estimate equation (3) for the post-conflict period, replacing the court-level measures $Exposure_{ct}$ with personal-level measures. Results are reported in Table 6. Notice that since we do not have personal exposure data for all judges, the sample size falls from 1,405 to 1,322 cases.[21] To facilitate comparison to the court-level analysis, in column 1 we replicate the regression of column 1 in Table 4 for the restricted sample. The results are similar to the original ones.

[Table 6]

Our first measure of personal exposure is the mean monthly exposure to fatalities in the natural area of the judge's place of employment during the entire conflict period. As seen in column 2, and consistent with the patterns in Figure 5, the association between this measure of personal exposure and post-conflict judicial bias is weak and statistically insignificant.

One possible explanation for this surprisingly weak association is that we do not measure correctly the way judges recall past experiences. In other contexts it has been argued that evaluations of past aversive personal experiences are dominated by the affect associated with the *worst* and the *final* moments of the episodes – not with the average or cumulative experience (this is known as the peak-end rule, see e.g., Kahneman et al. 1993; Redelmeier and Kahneman 1996). It is conceivable that a similar mechanism is operating in the present context. We therefore define *peak exposure* as the exposure at the worst month of the conflict, i.e., the maximal monthly number of fatalities in the natural area of the judge's place of employment during the conflict period. We define *late exposure* as the mean monthly number of fatalities in the natural area of the judge's place of employment during the last year of the conflict (2004). As seen in column 3, peak exposure is not associated with bias in the post-conflict period. Late exposure does seem to be positively associated with bias, but the association is not statistically different from zero (column 4).

Finally, we attempt to disentangle the two mechanisms, one operating at the local (court) level and the other operating at the individual (judge) level. In columns 5–7 we include both the court exposure measure and each of the personal exposure

[21] Further excluding from the analysis judges with incomplete employment history does not affect the results.

measures.[22] Consistent with the patterns observed in Figure 5, we find that controlling for court exposure, none of the personal exposure measures is associated with judicial bias in the post-conflict period. At the same time, the court exposure measure itself remains positively associated with judicial bias.

A possible difficulty in interpreting the results in Table 6 is that judge movements may not be random. For example, it is conceivable that over time judges move to the vicinity of their home town or, more generally, closer to the place they care about. Thus, court exposure in fact captures violence at the place the judge cared about even during the conflict. To address this issue, in Appendix I we re-examine the association between judicial bias and violence during the conflict period. We construct a new measure of personal exposure to violence. For each judge we locate the natural area in which she spent the most time during the *post-conflict* period. We next compute for each decision the number of fatalities in the preceding year in that natural area (i.e. in the area where the judge will work after the conflict). This measure is available for 1,583 of the 1,748 cases in the conflict period. Finally, we augment the baseline specification for the conflict period (column 1 of Table 3) with this new measure and its interactions with the ethnicity indicators. In other words, we run a "horse race" between the original court exposure variable and the new personal exposure measure. If judges care about the place they work in during the post-conflict period, the new measure should be positively associated with ethnic bias, while court exposure should not. Instead, we find that the estimated effect of court exposure maintains its size and statistical significance, while the new personal exposure variable is not associated with ethnic bias. This seems to rule out the above alternative interpretation of Table 6.

In sum, the analysis in this section suggests that the persistent effect of exposure to violence on the judicial system that we uncover operates primarily at the local (court) level rather than at the personal (judge) level.

7. Conclusion

This paper sought to contribute to our understanding of the effects of ethnic conflict on intergroup relations and on the functioning of legal institutions. Using a decision-based methodology to measure the extent of ethnic bias, and exogenous variation in exposure to violence, we found that the effect of ethnic violence is not limited to the time of the

[22] The simple correlation coefficient between court exposure and the three measures of personal exposure ranges from 0.53 to 0.69.

fighting. Despite the dramatic drop in violence in the years that followed the Second Intifada, the level of ethnic bias remained roughly similar to that observed during the height of the conflict. In the post-Intifada years, courts that had experienced more violence still tended to exhibit greater ethnic bias than courts situated in quieter regions, but the difference was smaller than during the conflict period. Thus, courts seemed to be converging toward a relatively high level of bias. Evidently, the decline in violence was not sufficient to heal interethnic relations.

A possible silver lining is that a judge's personal exposure to violence does not seem to have a strong lasting effect on bias. Ethnic identities are not set in stone. Even individuals who were personally exposed to high levels of violence during the conflict, can show little systematic bias if located in a relatively peaceful environment. It is thus not unreasonable to hope that a movement from an equilibrium with both conflict and ethnic bias to a peaceful equilibrium with little bias may still be achievable.

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TABLE 1: CASES BY ETHNICITY OF JUDGE AND LITIGANTS

			Period		
Judge	Plaintiff	Defendant	Conflict	Post-conflict	Total
Arab	Arab	Jewish	280	175	455
			(16.02%)	(12.46%)	(14.43%)
	Jewish	Arab	265	180	445
			(15.16%)	(12.81%)	(14.11%)
Jewish	Arab	Jewish	485	396	881
			(27.75%)	(28.19%)	(27.94%)
	Jewish	Arab	718	654	1,372
			(41.08%)	(46.55%)	(43.51%)
Total			1,748	1,405	3,153
			(100%)	(100%)	(100%)

TABLE 2: ETHNIC BIAS DURING AND AFTER THE CONFLICT

Dependent variable: claim accepted

	Conflict	Post-conflict	Conflict	Post-conflict
	(1)	(2)	(3)	(4)
Arab plaintiff	-0.151***	-0.169***	-0.117***	-0.170***
	(0.026)	(0.033)	(0.031)	(0.044)
Arab judge	-0.077*	-0.005		
	(0.044)	(0.031)		
Arab judge*Arab plaintiff	0.170***	0.177***	0.192***	0.179***
	(0.054)	(0.046)	(0.049)	(0.057)
Court FEs	Yes	Yes	Yes	Yes
Judge FEs & tenure	No	No	Yes	Yes
Case characteristics	No	No	Yes	Yes
Time controls	No	No	Yes	Yes
Observations	1,748	1,405	1,748	1,405
R-squared	0.044	0.057	0.248	0.258

Notes: Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. Case characteristics include: number of plaintiffs; number of defendants; share of private plaintiffs; share of private defendants; share of male plaintiffs; share of male defendants; monetary compensation requested (and an indicator for missing values); indicators for claim subjects; an indicator for "defense present"; and an indicator for cases where the defendant filed a counterclaim. Time controls include indicators for year, month, and day of week.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE 3: EFFECTS OF VIOLENCE IN THE SHORT AND THE LONG RUN

Dependent variable: claim accepted

	Natural Area			Sub-district			District		
Cases from:	2000–2004	2004	2007–2010	2000–2004	2004	2007–2010	2000–2004	2004	2007–2010
Court exposure during:	Preceding year	2000–2003	2000–2003	Preceding year	2000–2003	2000–2003	Preceding year	2000–2003	2000–2003
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Arab plaintiff	-0.096***	-0.077	-0.162***	-0.095***	-0.059	-0.151**	-0.084*	-0.031	-0.141
	(0.032)	(0.080)	(0.056)	(0.034)	(0.084)	(0.062)	(0.045)	(0.135)	(0.092)
Arab judge*Arab plaintiff	0.135***	0.102	0.131*	0.121**	0.017	0.106	0.112*	-0.020	0.061
	(0.044)	(0.128)	(0.071)	(0.049)	(0.152)	(0.086)	(0.060)	(0.194)	(0.117)
Court exposure	0.044			0.041			0.067		
	(0.152)			(0.150)			(0.111)		
Arab plaintiff*Court exposure	-0.143	-0.219**	-0.021	-0.136	-0.238**	-0.041	-0.133	-0.176	-0.036
	(0.134)	(0.100)	(0.080)	(0.135)	(0.102)	(0.086)	(0.138)	(0.135)	(0.090)
Arab judge*Court exposure	-0.190		2.326	-0.249		2.088*	-0.300		
	(0.335)		(2.436)	(0.313)		(1.131)	(0.191)		
Arab plaintiff*Arab judge*Court exposure	0.618***	0.467**	0.280**	0.627***	0.601***	0.249	0.436**	0.410**	0.219
	(0.178)	(0.193)	(0.124)	(0.185)	(0.227)	(0.154)	(0.171)	(0.196)	(0.151)
Observations	1,748	589	1,405	1,748	589	1,405	1,748	589	1,405
R-squared	0.250	0.328	0.260	0.249	0.329	0.260	0.249	0.326	0.259

Notes: Analysis includes cases from the period indicated in the first row of the column title. Court exposure is the cumulative number of civilian fatalities in the vicinity (natural area/sub-district/district) of the court during the period indicated in the second row of the column title. Fatality figures are divided by 100 for clarity. Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include court fixed effects and the full set of judge, case and time controls from columns 3-4 of Table 2. *Arab judge*Court exposure* is omitted from column 9 because two districts had the same absolute number of fatalities during the conflict, which generates perfect multicollinearity.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE 4: LONG-RUN EFFECTS OF EXPOSURE TO VIOLENCE – COURT LEVEL ANALYSIS

Cases from the post-conflict period (2007–2010)

Dependent variable: claim accepted

	Natural Area	Sub-district	District
	(1)	(2)	(3)
Arab plaintiff	-0.157***	-0.150**	-0.148*
	(0.056)	(0.062)	(0.088)
Arab judge*Arab plaintiff	0.146**	0.102	0.034
	(0.066)	(0.085)	(0.108)
Arab plaintiff*Court exposure	-0.032	-0.040	-0.033
	(0.074)	(0.083)	(0.083)
Arab judge*Court exposure	2.322	2.127*	
	(2.412)	(1.167)	
Arab plaintiff*Arab judge*Court exposure	0.256**	0.254*	0.232*
	(0.108)	(0.150)	(0.139)
Observations	1,405	1,405	1,405
R-squared	0.267	0.264	0.265

Notes: Court exposure is the cumulative number of civilian fatalities in the vicinity (natural area/sub-district/district) of the court during the conflict period (28/9/2000–31/12/2004), divided by 100 for clarity. Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include court fixed effects and the full set of judge, case and time controls from columns 3-4 of Table 2. Regressions also control for the number of civilian fatalities (separately from the Israeli-Palestinian conflict and the 2006 Lebanon War) in the vicinity of the court in the year preceding the trial fully interacted with indicators for judge and plaintiff ethnicity. *Arab judge*Court exposure* is omitted from column 3 because two districts had the same absolute number of fatalities during the conflict, which generates perfect multicollinearity.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE 5: LONG-RUN EFFECTS OF EXPOSURE TO VIOLENCE, CONTROLLING FOR TOWN TRAITS

Cases from the post-conflict period (2007–2010)

Dependent variable: claim accepted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arab plaintiff	-0.157***	-0.158***	-0.172***	-0.187**	-0.193**	-0.155***	-0.159**
	(0.056)	(0.045)	(0.066)	(0.083)	(0.081)	(0.055)	(0.071)
Arab judge*Arab plaintiff	0.146**	0.147**	0.259***	0.156	0.164*	0.049	0.154**
	(0.066)	(0.061)	(0.066)	(0.095)	(0.093)	(0.151)	(0.077)
Arab plaintiff*Arab judge*Court exposure	0.256**	0.423***	0.365***	0.240***	0.245***	0.330**	0.318***
	(0.108)	(0.144)	(0.092)	(0.088)	(0.087)	(0.127)	(0.114)
Arab plaintiff*Arab judge*Mixed ethnicity town		-0.222					
		(0.165)					
Arab plaintiff*Arab judge*Large town			-0.295***				
			(0.103)				
Arab plaintiff*Arab judge*High-income town				0.007			
				(0.113)			
Arab plaintiff*Arab judge*High-education town					-0.006		
					(0.111)		
Arab plaintiff*Arab judge*Large-family town						0.103	
						(0.154)	
Arab plaintiff*Arab judge*High-median-age town							-0.089
							(0.144)
Observations	1,405	1,405	1,405	1,405	1,405	1,405	1,405
R-squared	0.267	0.268	0.272	0.268	0.268	0.268	0.267

Notes: Court exposure is the cumulative number of civilian fatalities in the natural area of the court during the conflict period divided by 100. Town traits are for 2006 and relate to the town where the court is located (source: Israeli Central Bureau of Statistics). Mixed ethnicity town is an indicator for towns with a significant numbers of both Arab and Jewish residents. Large town is an indicator for towns with a population above the median across cases (median=64,200). High-income town is an indicator for towns with an above median per-capita income (NIS 2,620). High-education town is an indicator for towns with an above median high-school matriculation rate (51.3%). Large-family town is an indicator for towns with an above median share of families with at least four children (9.35%). High-median-age town is an indicator for towns where the median age is above the median across cases (32). Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include the full set of controls from column 1 of Table 4 as well as interactions between the relevant town trait and the *Arab plaintiff* and *Arab judge* indicators.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE 6: CHANNELS OF PERSISTENCE – PERSONAL- AND COURT-LEVEL EFFECTS OF VIOLENCE

Cases from the post-conflict period (2007–2010)

Dependent variable: claim accepted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arab plaintiff	-0.158***	-0.160***	-0.169***	-0.174***	-0.156***	-0.161***	-0.153**
	(0.057)	(0.051)	(0.059)	(0.046)	(0.055)	(0.061)	(0.065)
Arab judge*Arab plaintiff	0.105	0.160**	0.174**	0.169**	0.106	0.116	0.103
	(0.078)	(0.070)	(0.073)	(0.067)	(0.077)	(0.078)	(0.083)
Arab plaintiff*Arab judge*Court exposure	0.288**				0.294*	0.323**	0.307*
	(0.116)				(0.160)	(0.137)	(0.183)
Arab plaintiff*Arab judge*Mean personal exposure		0.054			-0.042		
		(0.115)			(0.108)		
Arab plaintiff*Arab judge*Peak personal exposure			0.000			-0.006	
			(0.007)			(0.007)	
Arab plaintiff*Arab judge*Late personal exposure				0.188			-0.043
				(0.118)			(0.175)
Observations	1,322	1,322	1,322	1,293	1,322	1,322	1,293
R-squared	0.268	0.266	0.266	0.266	0.268	0.268	0.268

Notes: Court exposure is the cumulative number of civilian fatalities in the natural area of the court during the conflict period (28/9/2000–31/12/2004). Fatality figures are divided by 100 for clarity. Mean personal exposure is the mean monthly number of civilian fatalities in the natural area of the judge's place of employment during the conflict period. Peak exposure is the maximal monthly number of civilian fatalities in the natural area of the judge's place of employment during the conflict period. Late exposure is the mean monthly number of civilian fatalities in the natural area of the judge's place of employment during the last year of the conflict (2004). Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include the full set of controls from column 1 of Table 4.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Figure 1: Civilian Fatalities by Year and District

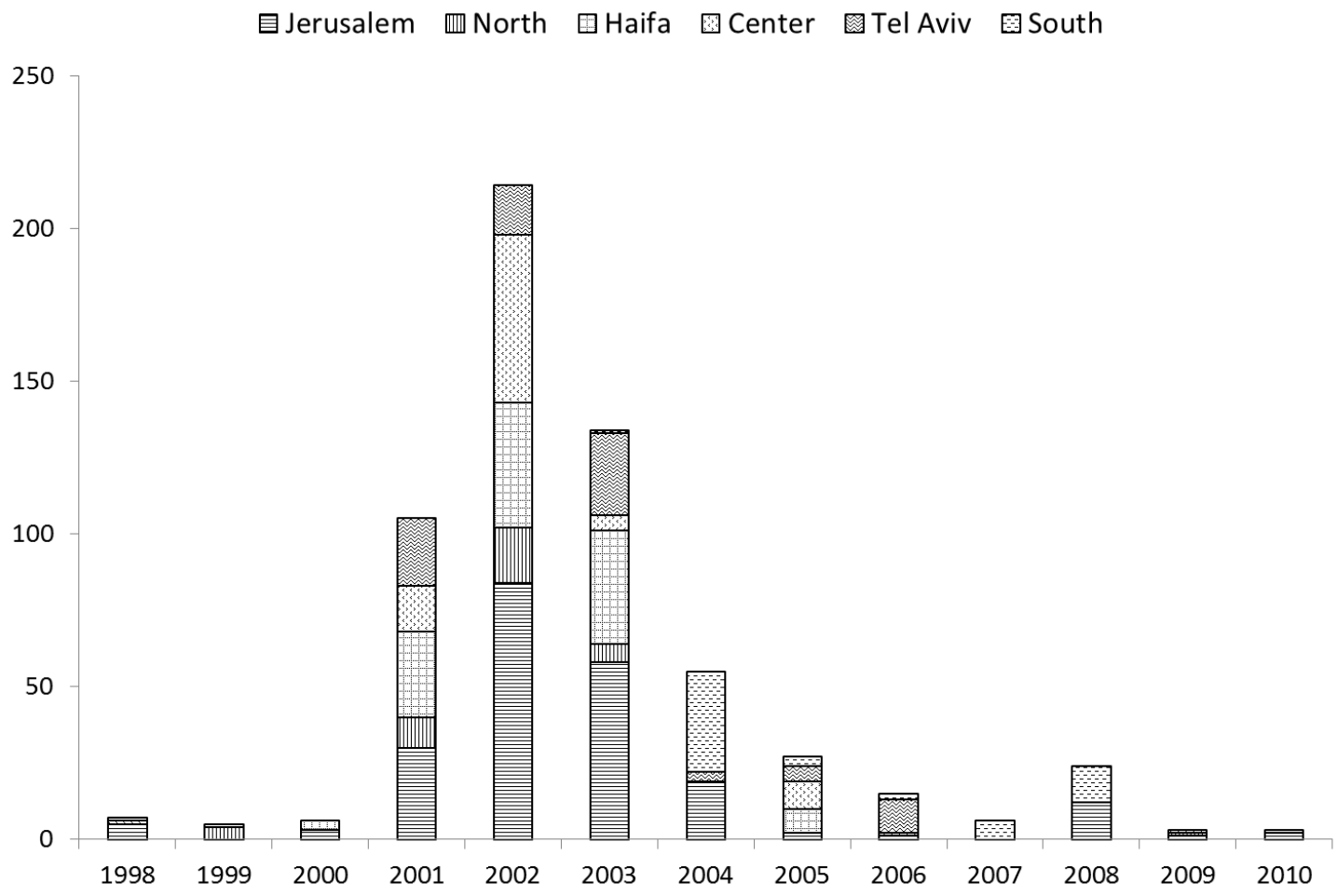
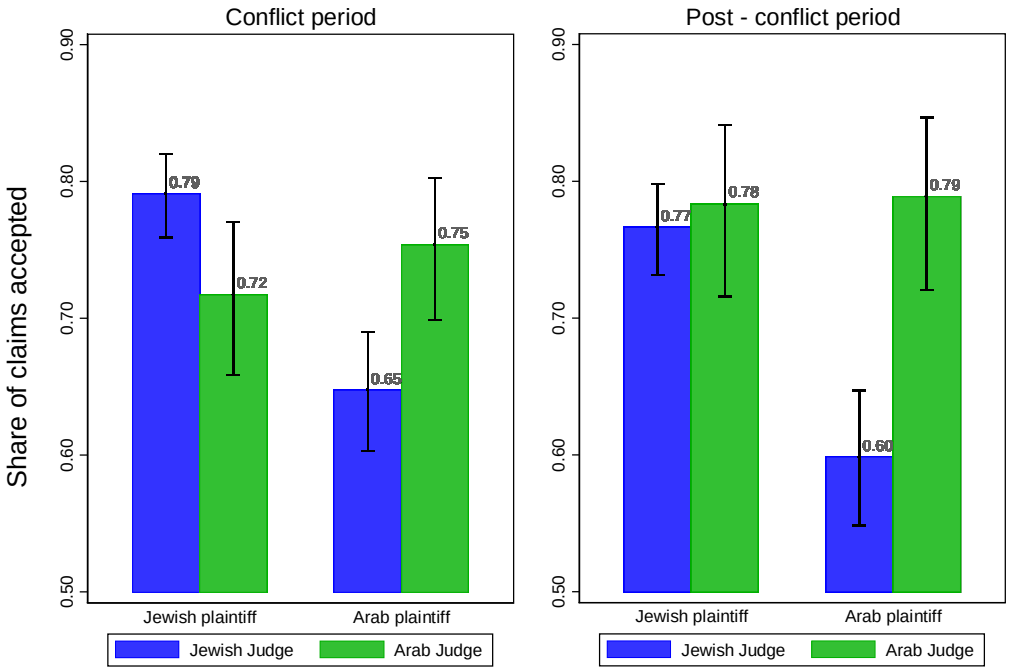
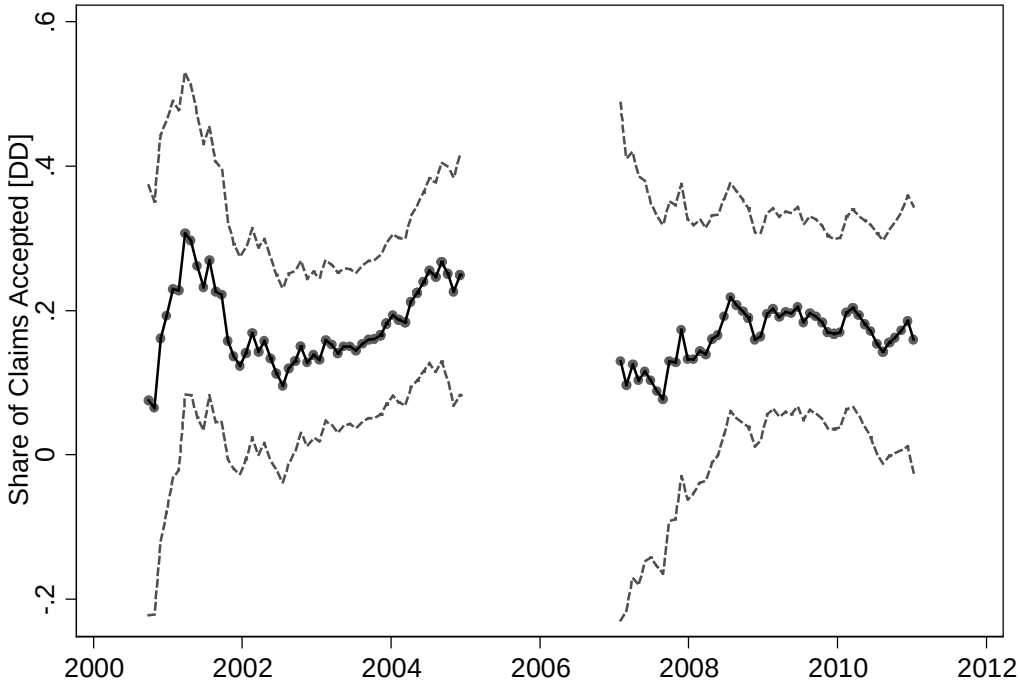


Figure 2: Ethnic Bias by Period



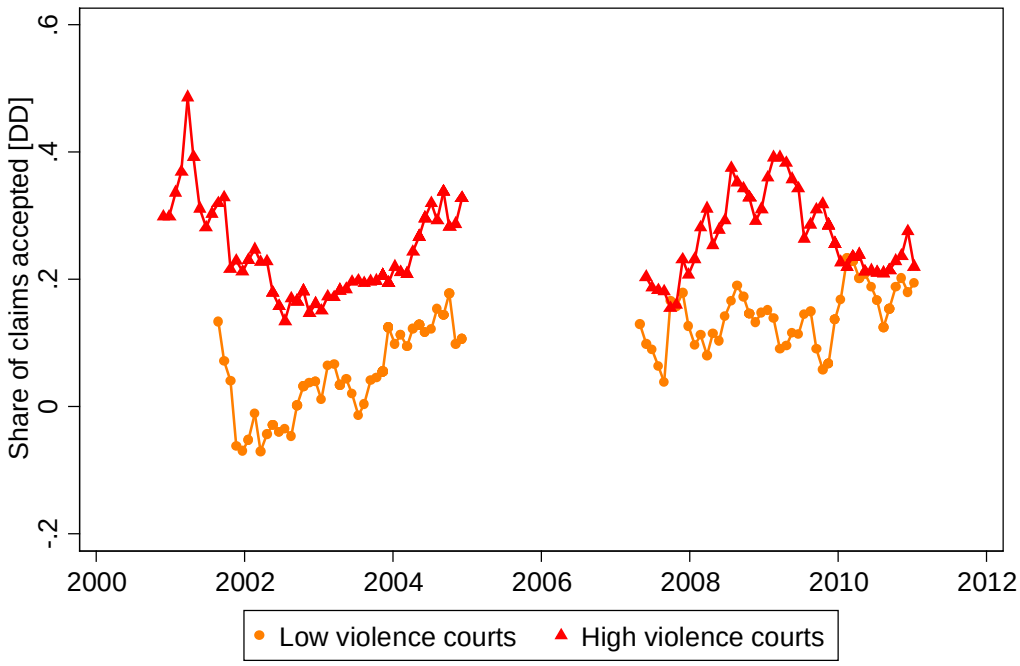
Capped ranges indicate 95% confidence intervals.

Figure 3: The Evolution of Ethnic Bias



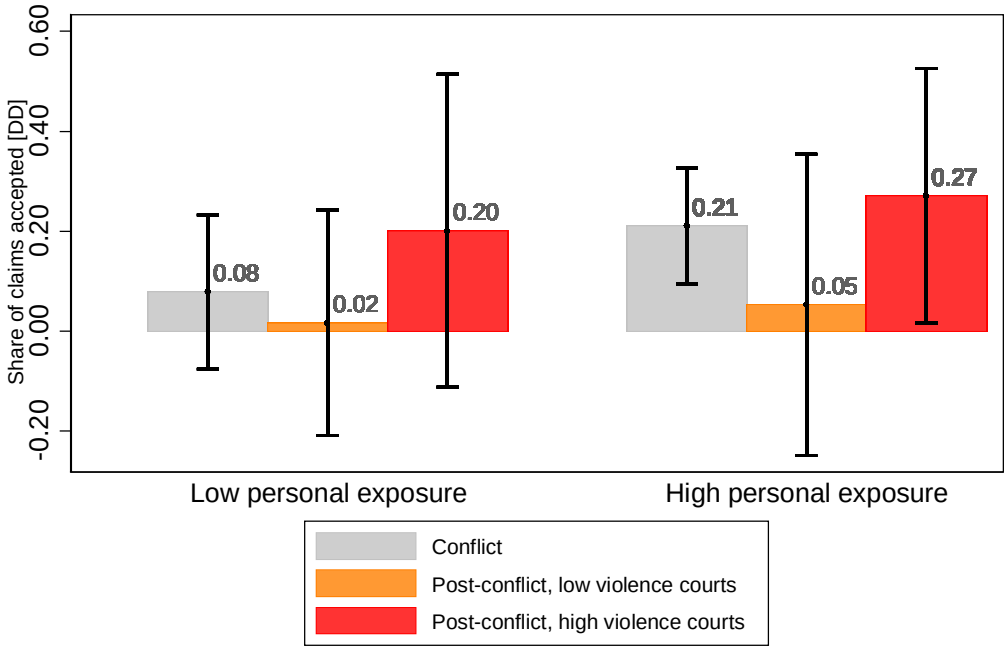
Diff-in-diff estimates in two-year moving windows with at least 100 obs (95% confidence intervals).

Figure 4: Ethnic Bias by Exposure to Violence During the Conflict



Diff-in-diff estimates of bias in two-year moving windows with at least 100 obs.
Based on 3,153 cases, 915 of which in courts that saw no fatalities during the conflict period.

Figure 5: Ethnic Bias by Personal and Court Exposure



Diff-in-diff estimates. Capped ranges indicate 95% confidence intervals.
Based on 233 judges and 3,070 cases, 870 of which decided by 40 judges that saw no fatalities in their place of employment during the conflict period.

ONLINE APPENDIX

Appendix A: Data collection and summary statistics

Data on judicial decisions

As mentioned in the body of the paper, our main source of data is online transcripts of judicial decisions (rulings). These documents first became available online in late 2000 in a handful of courts and over time coverage widened. Online coverage effectively stopped in 2005 and resumed only in 2007. We cover the universe of available documents: 26,444 from 2000–2004 and 28,576 from 2007–2010. Each document records the names of the judge and the litigants and typically includes several paragraphs that sketch the arguments made by the litigants and the ruling made by the judge. For the full set of available documents, we code whether each of the litigants is a private citizen, a business, or a government agency.

If the litigant is private, we code his or her ethnicity (Arab or Jewish) using the procedure detailed in the online Appendix to Shayo and Zussman (2011). Coding ethnicity employs a dataset derived from the Israel Population Registry which allows us to compute the likelihood of any first and family name being associated with an Arab or Jewish citizen. The accuracy of this procedure follows from the fact (apparent in data derived from the Registry) that there is very little overlap between Jewish and Arab names. Consistent with the ethnic breakdown reported in the body of the paper, we assume that all litigants are either Arab or Jewish. We cannot distinguish between sub-groups (e.g., Christian vs. Muslim Arabs and Ashkenazi vs. Sephardi Jews).

Having coded litigants' ethnicities for all available documents, we keep only "mixed cases": those where at least one private plaintiff and one private defendant are of different ethnicities (N=4,038). For these cases we conduct a comprehensive analysis of the documents. Each document is coded independently by two different coders. A third (senior) coder verifies the coding and adjudicates cases where there is an incompatibility across coders in any of the fields.

For the mixed cases, we extract data on (a) court; (b) judge's name (which we use to obtain biographical information); (c) litigants (in addition to information about type – private, business or a government agency – and ethnicity, we use the wording of the decision document and litigants' names to code gender); (d) claim subject (e.g., breach of contract, traffic accident, etc.); (e) timing of decision; (f) monetary compensation requested by the plaintiff and whether a counterclaim was filed; (g) whether the claim was settled outside the court or withdrawn; and (h) monetary transfers (if any), including legal expenses.

The main analysis in this paper excludes cases that were settled outside the court (325 cases) or withdrawn (303) as well as cases that have multiple plaintiffs (or defendants) such

that one plaintiff (or defendant) is Jewish and another is Arab (305). Finally, we exclude cases where the court is located in the Occupied Territories (1). This leaves us with 3,153 cases, 1,748 for 2000–2004 and 1,405 for 2007–2010.

Our main measure of trial outcome is a binary variable that takes the value of one if the claim was accepted and zero otherwise. Out of the 3,153 cases in our main sample, 2,300 (73%) are coded as accepted. We also construct several alternative outcome measures. The first attempts to distinguish between claims that were partly or fully accepted. This distinction is not straightforward: while in all cases we have information on the monetary compensation awarded by the judge, in more than 60% of the cases we do not know the sum requested. Nonetheless, we can sometimes deduce from the wording of the decision that the claim was "fully accepted." This yields an ordered categorical variable that takes three values: rejected (coded 0), partly accepted (1), or fully accepted (2). A second alternative measure of trial outcome is the monetary compensation awarded by the judge to the plaintiff net of the compensation awarded to the defendant (in case there was a counterclaim). A third alternative measure is the legal expenses awarded to the plaintiff net of the expenses awarded to the defendant. Finally, we look at the ratio between the net monetary compensation awarded by the judge to the plaintiff (inclusive of legal expenses) and the sum requested by the plaintiff.

Judges' ethnicities are coded using the same name-based procedure applied to the litigants. The main source for socio-demographic information on the judges is their biographies. Most biographies are available online; the rest were obtained from the court system using freedom of information procedures. We also collect data on judges' employment histories as explained below. Overall, our main sample has 240 judges, 30 of whom are Arab.

Data on exposure to violence

To measure the intensity of violence we collect data on all Palestinian politically motivated fatal attacks inside Israel (i.e., excluding the Occupied Territories).[1] For each attack we have information about date, location, and number of civilian fatalities. We also collect data on civilian fatalities inside Israel during the Second Lebanon War of 2006 to be able to control for possible effects of this conflict. Our fatality dataset uses information from several sources:

[1] We cannot use data on fatalities in the Occupied Territories since our identification strategy relies on variation in the intensity of ethnic violence in the vicinity of the courts or the judges' places of employment. Our data contains only one case from a court located in the Occupied Territories (this case is dropped from the analyses). Furthermore, only one of the judges in our data was employed in the Occupied Territories during the conflict period, and only one case in our final data was handled by this judge.

B'Tselem, the Israeli Information Center for Human Rights in the Occupied Territories; the Israeli National Insurance Institute; and the Israeli Ministry of Defense.[2]

Our first set of measures of exposure to violence is at the level of the court. These measures are based on the number of fatalities from attacks that occurred in the vicinity of the court during the conflict period. Vicinity is defined by three alternative geographical units, defined by the Central Bureau of Statistics. The first is the *natural area* which is the smallest geographical unit around the court. Our data span 24 natural areas, with an average population of around 230 thousand. Two of the 25 courts in our data are located in the same natural area. The two other geographical units are the *sub-district* and the *district* (average population is about 460 thousand per sup-district and 1.1 million per district). Our data spans 15 sub-districts, and 6 districts (shown in Figure 1 in the body of the paper).

Our second set of measures of exposure to violence is at the level of the judge. We compile information on employment history since 2000 for each judge in our dataset. The procedure relies on three main sources. The first is the official biographies mentioned above. These typically list the specific courts in which the judge served after being sworn in. The biographies also provide some information on employment prior to becoming a judge, in the private or public sector. The latter type of information is usually not detailed (e.g., "lawyer in a private firm") and, importantly, does not always include place of employment. Our second data source is the lists of lawyers published annually by the Israel Bar Association.[3] The list includes virtually all members of the association. For most of the members, it provides information about place of employment. Both the official biographies and the list of lawyers provide annual location data. The third source we use is a commercial computerized archive of judicial decisions in Israel.[4] The archive provides us with information about dates and locations of trials in which our judges participated, either as judges or representing litigants. This complements the information available from the first two sources.

The procedure yields monthly location data for the entire Intifada period for 196 (82%) of the 240 judges in our sample. For an additional 37 (15%) of the judges we have partial information (i.e., we have location information for only part of the conflict period) and for 7 judges we have no location information whatsoever for the conflict period. Merging the location information with the fatalities data yields a measure of the number of fatalities each

[2] The B'Tselem data cover fatalities from the Israeli-Palestinian conflict and have been used in most previous studies of the conflict (see, e.g., Gould and Klor 2010). We use the National Insurance Institute and Ministry of Defense data to verify the B'Tselem data and to add information on fatalities from the Second Lebanon War.

[3] The lists are included in *The Lawyer's Calendar* published annually by The Israel Bar Publishing House (from 2002 in collaboration with Martindale-Hubbell Israel).

[4] Accessible at: http://www.nevo.co.il/. This archive does not cover the universe of rulings but is considered the most comprehensive.

judge was exposed to in her place of employment in each month of the conflict. From this measure we construct three variables: (1) mean monthly exposure to fatalities in the natural area of the judge's place of employment during the entire conflict period; (2) maximum exposure in a given month; (3) mean monthly exposure during the last year of the conflict (2004).

Summary statistics

Tables A1-A3 provide summary statistics by cases (A1 and A2) and judges (A3) for the conflict and post-conflict periods. Table A1 shows case characteristics. As mentioned above, around 73% of the claims are accepted in both periods. Net monetary transfers rose from about NIS 3,100 to roughly NIS 4,200 while legal expenses remained roughly the same, at around NIS 180. On average, plaintiffs obtained 80% of the compensation they requested in the first period; the monetary yield declined to 70% in the second period. In terms of case characteristics, traffic accidents account for about two-thirds of the cases in both periods, although this proportion is somewhat lower in the post-conflict period (61% vs. 69%). The share of cases with missing information about the subject of the claim increased from 15% to 23% in the post-conflict period. Some documents note that the ruling was given under a condition of "no defense." This means either that no defense statement was submitted or that the defendant(s) failed to appear in court (it is not possible to distinguish between these two possibilities). This happened in 13% of the cases in the conflict period and 19% in the post-conflict period (with the others coded "defense present"). In both periods, a counterclaim was filed by the defense in roughly 9% of the cases. There is usually only one plaintiff in a case, but often more than one defendant. In both periods almost all cases were filed by private plaintiffs while the share of private litigants out of the total number of defendants is around 73%. The vast majority of litigants are male. Monetary compensation requested rose from about NIS 6,400 in the conflict period to approximately NIS 8,000 in the post-conflict period; note, however, that information on this variable is available only for 660 cases in the conflict period and 510 in the post-conflict period.

Table A2 reports the various measures of court exposure to violence. The first three rows show the average (per case) number of fatalities in the vicinity of the court in the year preceding the trial. The numbers demonstrate again the sharp decline in violence between the conflict period and the post-conflict period. The next six rows show descriptive statistics for court exposure during the conflict period: the first three years and the entire period. The differences here are much smaller and reflect compositional changes: in the post-conflict period a somewhat smaller share of cases come from the high-violence courts. This is important to keep in mind when comparing the conflict and the post-conflict periods: cases in the post-conflict period are not drawn from courts that experienced more violence.

Table A3 shows judge characteristics. The share of Arab judges increased from 11% to 16% from the conflict period to the post-conflict period. On average, judges in these courts are about 48 years old with five to seven years of tenure. About half of the judges are male. Approximately 20% were born outside of Israel. It is also noteworthy that the share of judges with advanced degrees increases across periods. The bottom part of the table reports judges' personal exposure to violence during the conflict. The average (across judges) of the mean monthly number of fatalities a judge was exposed to during the conflict (in the natural area of the judge's place of employment) is about 1. This is true for judges in both periods. The maximal number of fatalities a judge was exposed to in a given month is fourteen on average (and ranges from 0 to 30). Finally, the mean monthly number of fatalities a judge (in both periods) was exposed to during the last year of the conflict is around 0.3 on average.

TABLE A1: SUMMARY STATISTICS
CASE CHARACTERISTICS (N=3,153)

			Mean		Difference
			2000–2004	2007–2010	
			(1)	(2)	(3)
Claim outcome		Claim accepted	0.734	0.724	-0.010
					[0.016]
		-partly accepted	0.530	0.482	-0.048***
					[0.018]
		Net monetary compensation	3,079	4,165	1,086***
			(3,924)	(5,325)	[165]
		Net legal expenses	188.8	178.0	-10.8
			(497.1)	(511.2)	[18.0]
		Monetary yield[1]	0.799	0.690	-0.109***
			(0.427)	(0.577)	[0.029]
Case characteristics	Claim Subject	Breach of sales contract	0.032	0.038	0.006
					[0.007]
		Breach of service contract	0.095	0.081	-0.014
					[0.010]
		Housing-related	0.011	0.012	0.001
					[0.004]
		Private conflict	0.013	0.014	0.001
					[0.004]
		Traffic accident	0.689	0.613	-0.077***
					[0.017]
		Miscellaneous	0.013	0.012	0.000
					[0.004]
		Missing	0.147	0.229	0.082***
					[0.014]
	Defense	Defense present	0.866	0.811	-0.055***
					[0.013]
		Defense made a counterclaim	0.088	0.095	0.007
					[0.010]
	Number of litigants	Plaintiffs	1.113	1.137	0.024**
			(0.318)	(0.350)	[0.012]
		Defendants	1.724	1.757	0.032
			(0.713)	(0.754)	[0.026]
	Private litigants (share of total)	Plaintiffs	0.998	0.996	-0.002
			(0.031)	(0.043)	[0.001]
		Defendants	0.737	0.730	-0.007
			(0.258)	(0.258)	[0.009]
	Male litigants (share of private)	Plaintiffs	0.821	0.812	-0.009
			(0.364)	(0.370)	[0.013]
		Defendants	0.875	0.844	-0.030***
			(0.313)	(0.342)	[0.012]
	Compensation requested[1]		6,424	7,952	1,528***
			(5,085)	(6,529)	[340]

Notes: [1] Data on compensation requested by plaintiff/s and on monetary yield are available for 1,170 cases. Standard deviations in parentheses in columns (1)–(2). Standard errors in brackets in column (3).
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE A2: SUMMARY STATISTICS
FATALITIES (N=3,153)

Time window	Geographical area	Mean		Difference
		2000–2004	2007–2010	
		(1)	(2)	(3)
Previous year	Natural area	0.115	0.001	-0.114***
		(0.167)	(0.008)	[0.004]
	Sub-District	0.132	0.002	-0.130***
		(0.161)	(0.009)	[0.004]
	District	0.212	0.003	-0.209***
		(0.186)	(0.009)	[0.005]
First 3 years of conflict period	Natural area	0.402	0.351	-0.050***
		(0.455)	(0.427)	[0.016]
	Sub-District	0.462	0.411	-0.051***
		(0.418)	(0.393)	[0.015]
	District	0.776	0.735	-0.041***
		(0.439)	(0.419)	[0.015]
Entire conflict period	Natural area	0.421	0.362	-0.059***
		(0.486)	(0.455)	[0.017]
	Sub-District	0.484	0.422	-0.062***
		(0.448)	(0.422)	[0.016]
	District	0.806	0.748	-0.058***
		(0.442)	(0.436)	[0.016]

Notes: Number of civilian fatalities divided by 100. Standard deviations in parentheses in columns (1)–(2). Standard errors in brackets in column (3).
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE A3: SUMMARY STATISTICS
JUDGES (N=240)

	Mean		Difference
	2000–2004	2007–2010	
	(1)	(2)	(3)
Arab	0.114	0.157	0.043
			[0.040]
Age	48.368	49.151	0.784
	(9.478)	(9.248)	[1.090]
Tenure at job	4.959	7.236	2.277***
	(6.432)	(5.945)	[0.719]
Male	0.538	0.494	-0.044
			[0.058]
Immigrant (Jewish)	0.205	0.175	-0.030
			[0.046]
LLB degree from:			
- Hebrew U.	0.447	0.434	-0.013
			[0.058]
- Tel-Aviv U.	0.386	0.307	-0.079
			[0.055]
- Bar Ilan U.	0.129	0.151	0.022
			[0.041]
- Other institutions	0.038	0.108	0.071**
			[0.031]
Highest degree is:			
- LLB	0.818	0.681	-0.137***
			[0.051]
- Master	0.152	0.277	0.126***
			[0.048]
- Doctoral	0.030	0.042	0.012
			[0.022]
Personal exposure to violence during conflict:			
- Mean[1]	0.985	1.086	0.101
	(1.168)	(1.202)	[0.140]
- Maximum[2]	14.106	14.208	0.101
	(10.747)	(10.501)	[1.250]
- Late[3]	0.319	0.283	-0.035
	(0.649)	(0.571)	[0.072]
N	132	166	

Notes: [1] mean monthly exposure to civilian fatalities in judge's place of employment between 9/2000–12/2004; [2] maximum exposure in a given month; [3] mean monthly exposure between 1/2004–12/2004; see text for details. Standard deviations in parentheses in columns (1)–(2). Standard errors in brackets in column (3).
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Appendix B: Cases Withdrawn or Settled Outside the Court

TABLE B1

	Withdrawn		Settled Outside the Court	
	(1)	(2)	(3)	(4)
Arab plaintiff	0.004	0.014	0.014	0.018
	(0.010)	(0.013)	(0.017)	(0.025)
Arab judge*Arab plaintiff	-0.019	-0.034*	-0.019	-0.037
	(0.014)	(0.018)	(0.021)	(0.031)
Arab plaintiff*Court exposure		-0.021		-0.007
		(0.019)		(0.028)
Arab judge*Court exposure		0.014		0.073
		(0.268)		(0.180)
Arab plaintiff*Arab judge*Court exposure		0.044		0.076
		(0.035)		(0.056)
Observations	3,432	3,432	3,451	3,451
R-squared	0.316	0.316	0.284	0.285

Notes: The table follows the methodology of equations (2) and (3) in the body of the paper. In columns 1–2 the dependent variable is an indicator for cases withdrawn by the plaintiff. The sample includes cases decided by a judge or withdrawn. In column 3-4 the dependent variable is an indicator for cases settled outside the court. The sample includes cases decided by a judge or settled outside the court. Court exposure is the cumulative number of civilian fatalities in the natural area of the court during the conflict period (divided by 100). Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include court fixed effects, judge fixed effects and judge tenure, case characteristics, and time controls. Case characteristics include: number of plaintiffs; number of defendants; share of private plaintiffs; share of private defendants; share of male plaintiffs; share of male defendants; monetary compensation requested (and an indicator for missing values); an indicator for "defense present"; and an indicator for cases where the defendant filed a counterclaim. Time controls include indicators for year, month, and day of week.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Appendix C: Balancing tests

Our identification assumption in Section 4 of the paper is that given the court, the ethnicity of the plaintiff, and the ethnicity of the judge, cases assigned to a judge of the same ethnicity as the plaintiff are not systematically different from cases assigned to a judge of a different ethnicity. Recall that we allow plaintiffs from different ethnicities to file cases with different (observed or unobserved) characteristics. We also allow judges of different ethnicities to receive cases with different (observed or unobserved) characteristics. Table C1 below evaluates the validity of our identification assumption using the observed case characteristics.

The first column shows mean characteristics for cases assigned to a judge of the same ethnicity as the plaintiff. Column 2 shows these figures for cases where the judge and the plaintiff are from different ethnic groups. Column 3 shows the simple difference in means. While most of these differences are small in size, a few are statistically significant. This, however, may be due to systematic differences in case characteristics across plaintiffs of different ethnicities. For example, Arab plaintiffs are more likely than Jewish plaintiffs to be male. Since most judges are Jewish, Arab plaintiffs are also more likely than Jewish plaintiffs to be assigned a judge of the other ethnicity. As a result, the proportion of male plaintiffs is higher in different-ethnicity cases (column 2) than in same-ethnicity cases (column 1). However, once we control for the ethnicity of the plaintiff, the difference between same-ethnicity and different-ethnicity cases in fact vanishes (not shown). Similarly, differences across courts or across judges of different ethnicities may yield differences in mean characteristics across same- and different-ethnicity cases. In column 4, we therefore show the difference in case characteristics controlling for judge ethnicity, plaintiff ethnicity, and court fixed effects. Consistent with our identification assumption, column 4 shows little evidence of systematic differences. A broadly similar picture emerges when separately examining the conflict period and the post-conflict period, see Tables C2 and C3 below.

TABLE C1: BALANCING TESTS FOR THE ASSIGNMENT OF CASES: 2000–2010

	Mean		Difference in means		Obs.
	Same ethnicity	Different ethnicity	Without controls	With court FE, judge & plaintiff ethnicity	
	(1)	(2)	(3)	(4)	(5)
Number of plaintiffs	1.125	1.121	0.005	-0.003	3,153
	(0.333)	(0.333)	[0.012]	[0.013]	
Number of defendants	1.722	1.762	-0.04	0.021	3,153
	(0.723)	(0.744)	[0.026]	[0.029]	
Private plaintiffs (share of total)	0.998	0.997	0.001	0.001	3,153
	(0.035)	(0.039)	[0.001]	[0.001]	
Private defendants (share of total)	0.737	0.729	0.008	-0.018*	3,153
	(0.26)	(0.255)	[0.009]	[0.010]	
Male plaintiffs (share of private plaintiffs)	0.782	0.866	-0.084***	-0.015	3,153
	(0.392)	(0.322)	[0.013]	[0.014]	
Male defendants (share of private defendants)	0.892	0.819	0.072***	0.001	3,153
	(0.294)	(0.363)	[0.012]	[0.013]	
Claim subject - Breach of sales contract	0.037	0.031	0.006	0.004	3,153
			[0.007]	[0.007]	
Claim subject - Breach of service contract	0.091	0.086	0.005	0.008	3,153
			[0.01]	[0.011]	
Claim subject - Housing related	0.015	0.007	0.008**	0.007	3,153
			[0.004]	[0.004]	
Claim subject - Private conflict	0.013	0.014	-0.001	-0.002	3,153
			[0.004]	[0.005]	
Claim subject - Traffic accident	0.636	0.682	-0.046***	-0.004	3,153
			[0.017]	[0.018]	
Claim subject - Miscellaneous	0.013	0.012	0.001	-0.001	3,153
			[0.004]	[0.004]	
Claim subject - Missing	0.195	0.167	0.028**	-0.011	3,153
			[0.014]	[0.015]	
Defense present	0.828	0.86	-0.033**	0.000	3,153
			[0.013]	[0.014]	
Defense made a counterclaim	0.074	0.115	-0.041***	-0.025**	3,153
			[0.01]	[0.012]	
Compensation requested	7,173	6,963	210	-148	1,170
	(5,927)	(5,621)	[347]	[378]	

Notes: "Same ethnicity"=judge and plaintiff are of same ethnicity. Standard deviations in parentheses in columns (1)–(2). Standard errors in brackets in columns (3)–(4). Each entry in columns (3)–(4) is derived from a separate OLS regression where the explanatory variable is an indicator for same ethnicity of judge and plaintiff. Column (3) includes no controls and column (4) controls for judge ethnicity, plaintiff ethnicity, and court fixed effects.
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE C2: BALANCING TESTS FOR THE ASSIGNMENT OF CASES: 2000–2004

	Mean		Difference in means		Obs.
	Same ethnicity	Different ethnicity	Without controls	With court FE, judge & plaintiff ethnicity	
	(1)	(2)	(3)	(4)	(5)
Number of plaintiffs	1.119	1.104	0.015	0.011	1748
	(0.324)	(0.31)	[0.015]	[0.017]	
Number of defendants	1.722	1.727	-0.004	0.044	1748
	(0.712)	(0.715)	[0.034]	[0.037]	
Private plaintiffs (share of total)	0.998	0.998	0.001	0.001	1748
	(0.027)	(0.035)	[0.002]	[0.002]	
Private defendants (share of total)	0.736	0.739	-0.003	-0.029**	1748
	(0.261)	(0.255)	[0.012]	[0.013]	
Male plaintiffs (share of private plaintiffs)	0.787	0.867	-0.080***	-0.019	1748
	(0.390)	(0.321)	[0.018]	[0.019]	
Male defendants (share of private defendants)	0.899	0.842	0.057***	-0.004	1748
	(0.287)	(0.342)	[0.015]	[0.016]	
Claim subject - Breach of sales contract	0.035	0.028	0.007	0.004	1748
			[0.009]	[0.009]	
Claim subject - Breach of service contract	0.096	0.093	0.003	0.004	1748
			[0.014]	[0.015]	
Claim subject - Housing related	0.015	0.005	0.010*	0.009	1748
			[0.005]	[0.005]	
Claim subject - Private conflict	0.013	0.013	0.000	-0.001	1748
			[0.006]	[0.006]	
Claim subject - Traffic accident	0.677	0.705	-0.028	0.014	1748
			[0.022]	[0.023]	
Claim subject - Miscellaneous	0.014	0.011	0.003	0.001	1748
			[0.005]	[0.006]	
Claim subject - Missing	0.149	0.144	0.005	-0.030*	1748
			[0.017]	[0.017]	
Defense present	0.863	0.871	-0.008	0.030*	1748
			[0.016]	[0.016]	
Defense made a counterclaim	0.077	0.103	-0.026*	-0.010	1748
			[0.014]	[0.015]	
Compensation requested	6,573	6,214	359	189	660
	(5,281)	(4,798)	[402]	[447]	

Notes: "Same ethnicity"=judge and plaintiff are of same ethnicity. Standard deviations in parentheses in columns (1)–(2). Standard errors in brackets in columns (3)–(4). Each entry in columns (3)–(4) is derived from a separate OLS regression where the explanatory variable is an indicator for same ethnicity of judge and plaintiff. Column (3) includes no controls and column (4) controls for judge ethnicity, plaintiff ethnicity, and court fixed effects.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE C3: BALANCING TESTS FOR THE ASSIGNMENT OF CASES: 2007–2010

	Mean		Difference in means		Obs.
	Same ethnicity	Different ethnicity	Without controls	With court FE, judge & plaintiff ethnicity	
	(1)	(2)	(3)	(4)	(5)
Number of plaintiffs	1.133	1.142	-0.010	-0.027	1405
	(0.343)	(0.360)	[0.019]	[0.022]	
Number of defendants	1.721	1.807	-0.086**	-0.013	1405
	(0.736)	(0.778)	[0.041]	[0.047]	
Private plaintiffs (share of total)	0.996	0.996	0.000	0.002	1405
	(0.042)	(0.044)	[0.002]	[0.003]	
Private defendants (share of total)	0.739	0.716	0.022	0.001	1405
	(0.260)	(0.254)	[0.014]	[0.016]	
Male plaintiffs (share of private plaintiffs)	0.775	0.865	-0.089***	-0.014	1405
	(0.395)	(0.323)	[0.020]	[0.023]	
Male defendants (share of private defendants)	0.882	0.789	0.093***	0.007	1405
	(0.301)	(0.387)	[0.018]	[0.021]	
Claim subject - Breach of sales contract	0.040	0.035	0.005	0.003	1405
			[0.010]	[0.012]	
Claim subject - Breach of service contract	0.084	0.076	0.008	0.015	1405
			[0.015]	[0.017]	
Claim subject - Housing related	0.014	0.009	0.006	0.003	1405
			[0.006]	[0.007]	
Claim subject - Private conflict	0.013	0.016	-0.002	-0.003	1405
			[0.006]	[0.007]	
Claim subject - Traffic accident	0.586	0.651	-0.065**	-0.035	1405
			[0.026]	[0.029]	
Claim subject - Miscellaneous	0.011	0.014	-0.003	-0.002	1405
			[0.006]	[0.007]	
Claim subject - Missing	0.251	0.198	0.053**	0.021	1405
			[0.023]	[0.024]	
Defense present	0.785	0.847	-0.062***	-0.045**	1405
			[0.021]	[0.023]	
Defense made a counter claim	0.070	0.130	-0.060***	-0.043**	1405
			[0.016]	[0.018]	
Compensation requested	7,895	8,049	-154	-566	510
	(6,557)	(6,498)	[599]	[641]	

Notes: "Same ethnicity"=judge and plaintiff are from same ethnicity. Standard deviations in parentheses in columns (1)–(2). Standard errors in brackets in columns (3)–(4). Each entry in columns (3)–(4) is derived from a separate OLS regression where the explanatory variable is an indicator for same ethnicity of judge and plaintiff. Column (3) includes no controls and column (4) controls for judge ethnicity, plaintiff ethnicity, and court fixed effects.
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Appendix D: Ethnic Bias - Alternative Outcome Measures

TABLE D1

Panel A: All Observations

	Claim Outcome {0,1,2}		Net Monetary Compensation		Net Legal Expenses		Monetary Yield	
	Conflict	Post-conflict	Conflict	Post-conflict	Conflict	Post-conflict	Conflict	Post-conflict
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arab plaintiff	-0.369***	-0.537***	-662***	-1,144***	-135**	-174**	-0.052	-0.178**
	(0.107)	(0.103)	(249)	(305)	(54)	(71)	(0.041)	(0.073)
Arab judge*Arab plaintiff	0.587***	0.595***	926**	391	224***	153*	0.101*	0.163*
	(0.151)	(0.142)	(448)	(635)	(85)	(79)	(0.059)	(0.087)
Observations	1,748	1,405	1,748	1,404	1,748	1,405	660	510
R-squared/Pseudo R-squared	0.401	0.313	0.430	0.315	0.229	0.344	0.568	0.497
Panel B: Excluding Outliers								
Arab plaintiff			-615**	-1,072***	-91***	-151**	-0.049	-0.136**
			(251)	(214)	(30)	(60)	(0.045)	(0.056)
Arab judge*Arab plaintiff			824*	814	156***	121*	0.090	0.158*
			(425)	(547)	(54)	(65)	(0.062)	(0.087)
Observations			1,711	1,374	1,705	1,375	646	498
R-squared			0.384	0.308	0.263	0.262	0.548	0.629

Notes: Columns 1–2 are estimated by Ordered Probit and columns 3–8 are estimated by OLS. Panel B excludes the top and bottom 1% of cases in terms of the outcome variable. In columns 1–2 the dependent variable takes the value of 0 if the claim was rejected, 1 if the claim was partly accepted, and 2 if the claim was fully accepted. In columns 3–4 the dependent variable is the net monetary compensation awarded by the judge to the plaintiff (compensation awarded to plaintiff minus compensation awarded to defendant). In columns 5–6 the dependent variable is the net legal expenses awarded by the judge to the plaintiff (expenses awarded to plaintiff minus expenses awarded to defendant). In columns 7–8 the dependent variable is the ratio between the net monetary compensation (including legal expenses) awarded by the judge to the plaintiff and the compensation requested by the plaintiff. All regressions include court fixed effects, judge fixed effects and judge tenure, case characteristics, and time controls. Case characteristics include: number of plaintiffs; number of defendants; share of private plaintiffs; share of private defendants; share of male plaintiffs; share of male defendants; monetary compensation requested (and an indicator for missing values); indicators for claim subjects; an indicator for "defense present"; and an indicator for cases where the defendant filed a counterclaim. Time controls include indicators for year, month, and day of week. In columns 7–8 the monetary compensation requested by the plaintiff is not included in the case characteristics. Standard errors, clustered by judge, are in parentheses.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Appendix E: Is The Estimated Bias Due to Other Judge Characteristics?

TABLE E1

Dependent variable: claim accepted

	(1)	(2)	(3)	(4)	(5)	(6)
Arab plaintiff	-0.140***	-0.032	-0.117***	-0.108***	-0.132***	-0.127***
	(0.025)	(0.089)	(0.034)	(0.035)	(0.030)	(0.025)
Arab plaintiff*Arab judge	0.186***	0.164***	0.176***	0.175***	0.191***	0.182***
	(0.036)	(0.043)	(0.037)	(0.037)	(0.037)	(0.035)
Arab plaintiff*Judge age		-0.002				
		(0.001)				
Arab plaintiff*Judge tenure			-0.003			
			(0.002)			
Arab plaintiff*Male judge				-0.061		
				(0.038)		
Arab plaintiff*Judge HU					-0.022	
					(0.034)	
Arab plaintiff*Judge>LLB						-0.114**
						(0.053)
Observations	3,153	3,153	3,153	3,153	3,153	3,153
R-squared	0.220	0.221	0.221	0.221	0.220	0.221

Notes: Analysis includes cases from both periods. Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include court fixed effects, judge fixed effects and judge tenure, case characteristics, and time controls. Case characteristics include: number of plaintiffs; number of defendants; share of private plaintiffs; share of private defendants; share of male plaintiffs; share of male defendants; monetary compensation requested (and an indicator for missing values); indicators for claim subjects; an indicator for "defense present"; and an indicator for cases where the defendant filed a counter-claim. Time controls include indicators for year, month and day of week. "Judge HU" and "Judge>LLB" are indicators for whether judge attained LLB at the Hebrew University of Jerusalem and whether judge has a master or PhD degree, respectively.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Appendix F: Heterogeneity in Ethnic Bias

TABLE F1

Dependent variable: claim accepted

	(1)	(2)	(3)	(4)	(5)	(6)
Arab plaintiff	-0.140***	-0.061	-0.119***	-0.117***	-0.127***	-0.125***
	(0.025)	(0.093)	(0.036)	(0.037)	(0.033)	(0.025)
Arab plaintiff*Arab judge	0.186***	0.420	0.187***	0.199***	0.169**	0.175***
	(0.036)	(0.269)	(0.052)	(0.043)	(0.066)	(0.038)
Arab plaintiff*Arab judge*Judge age		-0.006				
		(0.006)				
Arab plaintiff*Arab judge*Judge tenure			-0.002			
			(0.007)			
Arab plaintiff*Arab judge*Male judge				-0.061		
				(0.080)		
Arab plaintiff*Arab judge*Judge HU					0.043	
					(0.077)	
Arab plaintiff*Arab judge*Judge>LLB						0.077
						(0.090)
Additional interactions	No	Yes	Yes	Yes	Yes	Yes
Observations	3,153	3,153	3,153	3,153	3,153	3,153
R-squared	0.220	0.221	0.221	0.221	0.220	0.221

Notes: Analysis includes cases from both periods. Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include court fixed effects, judge fixed effects and judge tenure, case characteristics, and time controls. For each judge characteristic *z*, "Additional interactions" include *ArabPlaintiff*z* and (for time varying *z*'s) *ArabJudge*z*. Case characteristics include: number of plaintiffs; number of defendants; share of private plaintiffs; share of private defendants; share of male plaintiffs; share of male defendants; monetary compensation requested (and an indicator for missing values); indicators for claim subjects; an indicator for "defense present"; and an indicator for cases where the defendant filed a counter-claim. Time controls include indicators for year, month and day of week. "Judge HU" and "Judge>LLB" are indicators for whether judge attained LLB at the Hebrew University of Jerusalem and whether judge has a master or PhD degree, respectively.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Appendix G: Balancing Tests for the Assignment of Cases by Past Exposure to Violence, 2007–2010

Our identification assumption in Sections 5 and 6 of the paper is that as local exposure to violence during the conflict period increases, cases assigned to a judge of the same ethnicity as the plaintiff do not become systematically different from cases assigned to a judge of a different ethnicity. Table G1 below examines this assumption with respect to observed case characteristics. For ease of interpretation, column 1 presents the means and standard deviations of the variables. Column 2 presents the results for court exposure (measured by the cumulative number of fatalities in the natural area of the court during the conflict period, divided by 100). Column 4 presents the results for personal exposure (measured by the mean monthly number of fatalities in the natural area of the judge's place of employment during the conflict period).[5] Each entry in columns 2 and 4 is derived from a separate OLS regression where the explanatory variables include court fixed effects, indicators for judge ethnicity, plaintiff ethnicity, and same ethnicity of judge and plaintiff, as well as the exposure variable fully interacted with the ethnicity indicators. The table reports the coefficient on the interaction term *Exposure*SameEthnicity*. That is, we report the estimated α_7 from an equation of the form:

$$characteristic_{ijc} = \alpha_0 + \alpha_1 ArabPlaintiff_i + \alpha_2 ArabJudge_i + \alpha_3 SameEthnicity_i + \alpha_4 Exposure_c + \alpha_5 Exposure_c * ArabPlaintiff_i + \alpha_6 Exposure_c * ArabJudge_i + \alpha_7 Exposure_c * SameEthnicity_i + \delta_c + \varepsilon_{ijc}$$

where i indexes cases, j indexes judges, and c indexes courts. In column 4 we report the corresponding coefficient when exposure is at the personal (j) level.[6] Overall, there is little evidence of a systematic relationship between exposure to violence during the conflict and post-conflict differences in case characteristics between cases assigned to same vs. other ethnicity judges.[7]

[5] Results are qualitatively similar when using the alternative measures of court and personal exposure mentioned above and used in Tables 3-6 in the body of the paper.

[6] The un-interacted exposure variable is dropped in column 2 due to the inclusion of court fixed effects.

[7] The main exception is that cases assigned to a judge of the same ethnicity as the plaintiff seem to have a higher proportion of "defense present" in courts that experienced more fatalities during the conflict (column 2, third-to-last row). Since defense presence lowers the probability of the claim being accepted (this is one of the unreported controls in Tables 2-6 in the body of the paper), then to the extent that "defense present" is positively correlated with unobservables that also lower the probability of a claim being accepted, this might bias downward our estimate of the effect of past exposure to violence on judicial ethnic bias.

TABLE G1: BALANCING TESTS FOR THE ASSIGNMENT OF CASES BY PAST EXPOSURE TO VIOLENCE, 2007–2010

	Mean	Court Exposure	N	Personal Exposure	N
	(1)	(2)	(3)	(4)	(5)
Number of plaintiffs	1.137	0.147**	1,405	0.084	1,322
	(0.35)	[0.061]		[0.057]	
Number of defendants	1.757	0.115	1,405	-0.235*	1,322
	(0.754)	[0.129]		[0.123]	
Private plaintiffs (share of total)	0.996	0.010	1,405	0	1,322
	(0.043)	[0.008]		[0.007]	
Private defendants (share of total)	0.730	-0.068	1,405	0.046	1,322
	(0.258)	[0.044]		[0.041]	
Male plaintiffs (share of private plaintiffs)	0.812	-0.085	1,405	-0.048	1,322
	(0.370)	[0.063]		[0.060]	
Male defendants (share of private defendants)	0.844	0.104*	1,405	0.031	1,322
	(0.342)	[0.057]		[0.054]	
Claim subject - Breach of sales contract	0.038	-0.012	1,405	0.004	1,322
		[0.033]		[0.031]	
Claim subject - Breach of service contract	0.081	-0.016	1,405	0.026	1,322
		[0.047]		[0.044]	
Claim subject - Housing-related	0.012	0.002	1,405	-0.001	1,322
		[0.019]		[0.018]	
Claim subject - Private conflict	0.014	-0.005	1,405	-0.027	1,322
		[0.021]		[0.020]	
Claim subject - Traffic accident	0.613	0.073	1,405	-0.055	1,322
		[0.08]		[0.075]	
Claim subject - Miscellaneous	0.012	0.002	1,405	0.010	1,322
		[0.019]		[0.018]	
Claim subject - Missing	0.229	-0.046	1,405	0.041	1,322
		[0.067]		[0.062]	
Defense present	0.811	0.166***	1,405	0.037	1,322
		[0.063]		[0.059]	
Defense made a counter claim	0.095	0.024	1,405	-0.025	1,322
		[0.051]		[0.048]	
Compensation requested	7,952	998	510	-489	472
	(6529)	[1,513]		[1379]	

Notes: Standard deviations in parentheses in column (1). Standard errors in brackets in columns (2) and (4). Each entry in columns (2) and (4) is derived from a separate OLS regression where the explanatory variables include court fixed effects, indicators for judge ethnicity, plaintiff ethnicity, and same ethnicity of judge and plaintiff, as well as the exposure variable fully interacted with the ethnicity indicators. The table reports the coefficient on the interaction *Exposure*SameEthnicity*. The exposure variable in columns 2–3 is the cumulative number of civilian fatalities in the natural area of the court during the conflict period (divided by 100). The exposure variable in columns 4–5 is the mean monthly number of civilian fatalities in the natural area of the judge's place of employment during the conflict period.
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Appendix H: Placebo Tests

TABLE H1: IS BIAS ASSOCIATED WITH FUTURE EXPOSURE TO VIOLENCE?

Dependent variable: claim accepted

	Natural area		Sub-district		District	
Cases from	2000–2003	2007–2010	2000–2003	2007–2010	2000–2003	2007–2010
	(1)	(2)	(3)	(4)	(5)	(6)
Arab plaintiff	-0.074*	-0.166***	-0.072*	-0.167***	-0.032	-0.174***
	(0.040)	(0.046)	(0.042)	(0.046)	(0.050)	(0.047)
Arab judge*Arab plaintiff	0.133**	0.187***	0.126*	0.169***	0.077	0.165**
	(0.059)	(0.058)	(0.074)	(0.064)	(0.080)	(0.064)
Arab plaintiff*Arab judge* Court exposure in 2004	-0.892	2.026***	-1.039	2.007**	-0.373	0.996
	(0.900)	(0.675)	(0.797)	(1.008)	(0.451)	(0.950)
Observations	1,159	1,405	1,159	1,405	1,159	1,405
R-squared	0.264	0.266	0.264	0.262	0.267	0.264

Notes: Analysis includes cases from the period indicated in the column title. Court exposure is the number of civilian fatalities (divided by 100) in the vicinity (natural area/sub-district/district) of the court during 2004. Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include the same set controls as in the corresponding columns of Table 4 in the body of the paper.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE H2: IS BIAS ASSOCIATED WITH EXPOSURE IN THE FOLLOWING YEAR?

Cases from the conflict period (2000–2004)

Dependent variable: claim accepted

	Natural Area	Sub-district	District
	(1)	(2)	(3)
Arab plaintiff	-0.103***	-0.104***	-0.098**
	(0.033)	(0.035)	(0.045)
Arab judge*Arab plaintiff	0.132***	0.112**	0.112**
	(0.042)	(0.046)	(0.055)
Arab plaintiff*Arab judge*	0.697***	0.739***	0.475**
Court exposure in preceding year	(0.193)	(0.206)	(0.210)
Arab plaintiff*Arab judge*	-0.057	-0.060	-0.040
Court exposure in following year	(0.290)	(0.314)	(0.215)
Observations	1,748	1,748	1,748
R-squared	0.251	0.251	0.251

Notes: Court exposure is the number of civilian fatalities in the vicinity (natural area/sub-district/district) of the court in the year preceding/following the trial (divided by 100 for clarity). Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include the same set controls as in columns 1, 4 and 7 of Table 3 in the body of the paper, as well as the court exposure variables and their interactions with the *Arab plaintiff* and *Arab judge* indicators.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

Appendix I: Judicial Bias and Violence in the Judge's Future Location

TABLE I1

Cases from the conflict period (2000–2004)

Dependent variable: claim accepted

	(1)	(2)
Arab plaintiff	-0.079**	-0.082**
	(0.037)	(0.041)
Arab judge*Arab plaintiff	0.124**	0.132**
	(0.048)	(0.053)
Arab plaintiff*Arab judge*	0.609***	0.877**
Recent court exposure	(0.178)	(0.388)
Arab plaintiff*Arab judge*		-0.289
Recent court exposure in judge's future location		(0.407)
Observations	1,583	1,583
R-squared	0.252	0.252

Notes: Recent court exposure is the number of civilian fatalities in the natural area of the court in the year preceding the trial (divided by 100 for clarity). Recent court exposure in judge's future location is the number of fatalities in the preceding year in the natural area where the judge will work in the post-conflict period (divided by 100; see text for details). Regressions are estimated by OLS. Standard errors, clustered by judge, are in parentheses. All regressions include the same set controls as in column 1 of Table 3 in the body of the paper, as well as the court exposure variables and their interactions with the *Arab plaintiff* and *Arab judge* indicators.

*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.