Judicial Ingroup Bias in the Shadow of Terrorism*

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Abstract

We study ingroup bias – the preferential treatment of members of one's group – in naturally occurring data, where economically significant allocation decisions are made under a strong non-discriminatory norm. Data come from Israeli small claims courts during 2000-04, where the assignment of a case to an Arab or Jewish judge is essentially random. We find robust evidence for judicial ingroup bias. Furthermore, this bias increases with terrorism intensity in the vicinity of the court in the year preceding the ruling. The results are consistent with theory and lab evidence according to which salience of group membership enhances social identification.

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1 Introduction

Traditional economic models assume that people care only about their self interest. However, people may also care about groups to which they belong. We refer to this phenomenon as social identification. One of the most extensively studied manifestations of social identification is ingroup bias: the preferential treatment of members of one's own group. A crucial observation here is that social identification is endogenous: people do not automatically identify with any group they belong to. In particular, social identification has been shown to be affected by the salience of group-specific attributes.

Evidence for the existence of ingroup bias and for its sensitivity to group salience comes mostly from experiments. In this paper we study ingroup bias and saliency effects in naturally occurring data, where professional decision makers make economically significant allocation decisions under a strong non-discriminatory norm. Specifically, we analyze judicial decisions in Israeli small claims courts during 2000-04. These courts handle civil cases between private litigants.

Several features make this setting ideal for investigating ingroup bias. First, when making a decision, a judge in these courts allocates resources between two individuals who may or may not belong to her social group. This feature resembles standard lab experiments which measure ingroup bias. However, unlike allocation decisions in lab experiments, the decisions we study are made by professional judges who are expected to apply the law blindly. Second, the Israeli setting allows us to study social identification with naturally occurring, "real-life", groups: Arabs and Jews. Third, the assignment of cases to judges within a given court is essentially random. This facilitates credible estimation of the extent of ingroup bias. Finally, the period studied is characterized by intense ethnically-based terrorist attacks. Since these attacks are plausibly exogenous to the legal procedure, they allow us to study the effects of ethnic salience on ingroup bias.[1]

The main source of data used in our analysis is transcripts of decisions made by judges in the small claims courts. From these documents we extract information on the court, litigants, subject of the claim, timing of decision, and claim outcome. The ethnicity of judges and litigants is deduced from their names. Our dataset covers the universe of documents available for 2000-04 where a plaintiff of one ethnicity faces a defendant of a different ethnicity. Our main analysis focuses on 1,748 judicial decisions, 31% of which were made by Arab judges and the rest by their Jewish colleagues.

We find robust evidence for the existence of judicial ingroup bias in this period. A claim is

[1] The procedure of allocating cases is described in section 2.2. Randomization tests are in section 4. The exogeneity of terror intensity with respect to case allocation is examined in section 5.1.

between 17% and 20% more likely to be accepted if assigned to a judge of the same ethnicity as the plaintiff. In monetary terms the estimated bias translates to over $200 per case.

The above estimates represent a level of bias that is characteristic of the period as a whole. We next ask whether this bias is an exogenously given feature of inter-ethnic relations in Israel or if, alternatively, it varies with the salience of ethnic cleavages. In particular, we examine whether judicial ingroup bias is related to the intensity of Palestinian politically motivated fatal attacks inside Israel. Results suggest that judicial ingroup bias is positively and significantly associated with terrorism intensity as measured by the number of fatalities per capita in the area surrounding the court in the year preceding the judicial decision. Furthermore, the data seem to indicate that terrorism affects judges of both ethnicities, leading Arab judges to favor Arab plaintiffs and Jewish judges to favor Jewish plaintiffs.

We interpret these findings in terms of a general framework for modeling social identity developed in Shayo (2009). This framework – outlined in Appendix A – attempts to capture both the behavioral effects of social identification and the endogenous determination of the groups people identify with. The basic structure of the model is as follows. A society may have many social groups – "Israeli", "Arab", "middle class" and so on – but in any given situation individuals "identify" with only some of these. Given their social identities, individuals choose courses of action, which determine the aggregate outcome. That outcome forms the social environment that in turn affects the pattern of social identities. A *Social Identity Equilibrium* is a steady state where *(i)* each individual's behavior is optimal given her social identity; *(ii)* social identities are optimal given the social environment; and *(iii)* the social environment is determined by the behavior of the individuals. The present paper seeks to shed light on two of the major components of the model. First, the effect of social identification on behavior (where we focus on ingroup bias); and second, the effect of the social environment on identification patterns (where we focus on the effect of the salience of group-specific attributes).

We note, however, that since this is not a controlled experiment, we cannot completely rule out two alternative interpretations of our findings. First, in our setting the person making the allocation decision (the judge) communicates with the individuals receiving the allocation (the litigants). It is thus possible that what underlies the results is not a preferential treatment of members of one's own group but rather better transmission of information between the judge and the litigant when they belong to the same ethnic group. A Jewish judge may simply better understand the arguments made by a Jewish litigant than those made by an Arab one. While we cannot dismiss this possibility, it seems implausible that terrorism intensity should affect the difference in the quality of communication between judge and litigants.

Second, the results might be driven by litigant behavior rather than by judge behavior. This is a central issue confronted by Price and Wolfers (forthcoming) in their analysis of racial bias in refereeing decisions in the NBA. However, in contrast to the NBA setting, where a player might conceivably behave more aggressively when assigned a refereeing team of the opposite race, in our setting the legally relevant actions take place before the legal procedure starts, and certainly before the parties involved know the identity of the judge that will rule in their case.[2]

The paper relates to two major strands of the literature. The first is the literature on social identity and ingroup bias and the second is the literature on ethnic and racial bias in economic and legal settings. Ingroup bias has been studied extensively using the experimental setting known as the Minimal Group Paradigm. In these experiments an individual allocates some resource between two other individuals, where the only thing she knows about them is whether they belong to her group or not. Starting with Tajfel et al. (1971), this literature has demonstrated that ingroup bias can emerge even in artificially created groups, and has examined various factors which facilitate its emergence. Another prominent line of research looks at how the salience of group membership affects contributions to public goods (e.g. Bornstein 2003, Eckel and Grossman 2005, Orbell et al. 1988). Other settings where ingroup bias has been studied are reported in Bernhard et al. (2006), Chen and Li (2009), Fong and Luttmer (2009) and Klor and Shayo (2010). See Shayo (2009) for a review of this literature.

Beyond the literature on social identity, our paper is closely associated with the extensive literature on discrimination. The economic literature identifies two major types of discrimination: taste-based (Becker 1957) and statistical (Arrow 1973 and Phelps 1972). Our paper is more closely related to the former, but rather than treating the taste for discrimination as exogenously given, we seek to study its determinants. Methodologically, the approach we take in identifying the effect of terrorism on judicial bias is similar to that of earlier studies which examine the effects on economic outcomes of shocks to tastes associated with political events. A recent example is Michaels and Zhi (forthcoming) who examine the effect of a deterioration in relations between the USA and France in 2002-03 on trade between the two countries. In the Israeli context, Miaari et al. (2009) examine how the outbreak of the second Palestinian Intifada (uprising) in September 2000 affected labor market outcomes of Arabs relative to those of Jewish Israelis.

Finally, a large literature studies possible bias against Blacks and Hispanics in the American criminal justice system.[3] A major methodological obstacle in this context is the difficulty

[2]Further, possible differences in litigant behavior in the court might reflect a reaction to perceived discriminatory behavior by the judge (we thank Christine Jolls for suggesting this point).

[3]In the Israeli context, Gazal-Ayal and Sulitzeanu-Kenan (forthcoming) study ethnic ingroup bias in detention decisions in criminal courts.

of ruling out potential correlation between race and ethnicity on the one hand and unobserved case characteristics on the other. Several innovative strategies have been used to tackle this problem. For example, Abrams et al. (2007) rely on the random assignment of cases to judges to examine the between-judge variation in incarceration rates of Blacks relative to Whites. They find large inter-judge disparity, suggesting that at least some judges differentially treat defendants based on their race. Alesina and La Ferrara (2010) use discrepancies in decisions made in lower versus higher courts to provide evidence of bias against minority defendants in capital sentencing. Glaeser and Sacerdote (2003) examine data on vehicular homicides, where the identity of the victim is arguably random, and find that drivers who kill Blacks receive significantly shorter sentences. Finally, McConnell (2010) analyzes judicial decisions in federal courts following 9/11 and finds no change in sentencing outcomes for any ethnic group other than Hispanics. A novel feature of our identification strategy is the combined use of random assignment of judges to cases with exogenous variation in the salience of ethnicity to study judicial bias.

We proceed as follows. The next section describes the historical and institutional setting in which our empirical investigation takes place. In section 3 we explain how the dataset was constructed and provide descriptive statistics. Section 4 estimates the overall level of judicial ingroup bias in the period under investigation while section 5 studies the effect of terrorism on the extent of the bias. Section 6 concludes.

2 The setting

2.1 Time and place

We analyze decisions involving Arabs and Jews in Israel in 2000-2004. In this period Arab citizens of Israel numbered roughly 1.25 million, or about 20% of the country's population. Arab-Jewish relations inside Israel are strongly associated with developments in the Arab-Israeli conflict and in particular with relations between Israel and the Palestinians in the Occupied Territories.

In late September 2000, following a period of relatively calm relations between Israel and the Palestinians, the second Intifada erupted. The ensuing years saw an intense wave of violence between Israelis and Palestinians, claiming the lives of thousands. Suicide bombings resumed in late 2000, peaked in 2002 and subsided in 2004.

In the first days of October 2000 there were mass demonstrations and clashes between Arab Israelis and the police which left twelve protestors dead. These "October Events" are widely considered a turning point in Arab-Jewish relations in Israel, contributing to a rise

in ethnocentric views among both Arabs and Jews.[4]

2.2 Small claims courts

Small claims courts operate in many countries around the world, including Australia, Canada, England and the USA. These courts handle civil cases between private litigants. The amount of monetary judgments they can award is capped: in Israel during the period under investigation the cap was set at 17,800 New Israeli Shekels (NIS), roughly equal to $US 4,000.

The rules of civil procedure and of evidence in the Israeli small claims courts are relatively simple. The procedure starts when the plaintiff files a claim at the court, provides supporting documentation, and pays a small fee. Claims can only be submitted to the court where either: (1) the relevant transaction took place or was supposed to take place; and/or (2) the defendant lives or works. Immediately following the filing of the claim, the defendant is notified and is instructed to provide a defense statement within fifteen days. The defendant has the right to submit a counter-claim to which the original plaintiff needs to respond within seven days.

Once a claim was filed – or, in some courts, after the defendant has responded – the case is assigned a trial date and a judge. Due to a backlog in the system, trials are scheduled several months in advance. Each case is assigned to the first available slot. This means that the assignment of judges to cases within a court is in principle orthogonal to characteristics of the case.[5]

The judge receives the case materials no earlier than a week before the trial. Importantly, the plaintiff and the defendant represent themselves in the trial, i.e. the litigants appear without lawyers. During the trial, which typically lasts only a few minutes, the judge sees the litigants for the first time and hears their arguments.[6] The judge has to issue a ruling in the case within seven days of the trial. Litigants who wish to appeal a ruling need to first request approval from the relevant district court.

Three features of these small claims courts make them especially appealing for analyzing ingroup bias. First, unlike courts which handle criminal cases, in small claims courts the judge decides on monetary transfers between two individuals. Our investigation is restricted to cases where litigants belong to two different ethnic groups. Since the judge belongs to one

[4] It is important to note, however, that the participation of Israeli Arabs in acts of politically motivated violence (either in concert with Palestinians from the Occupied Territories or independently) has remained negligible in scale.

[5] Judges cannot normally decline to rule in a case, unless they are personally acquainted with one of the litigants. In such cases they need to notify the court management of the circumstances.

[6] Litigants have a right to ask for an interpretor to be present in the court if they are not proficient in Hebrew.

of these groups, this generates a situation resembling standard (Minimal Group) experiments measuring ingroup bias. However, there are three crucial differences between our setting and typical experimental settings: (a) decision makers are professional and operate under a strong non-discriminatory norm (equality before the law); (b) monetary stakes are quite significant – the average compensation requested by plaintiffs in our sample is roughly $1,460; and (c) the groups in our setting are natural, i.e. they are not formed by the researcher.

A second important feature of small claims courts is that judges receive the case materials at most a week before the trial, meet the litigants only once, and are forced to produce decisions within a week. This means that the proximate timing of the decision is known (as opposed to cases involving protracted procedures, where it is hard to tell at what stage of the trial the judicial decisions were actually made). Another possible implication of the need to make decisions quickly is that it can make judges more susceptible to stereotyping and bias (on implicit bias see e.g. Bertrand et al. 2005 and Jolls and Sunstein 2006).

Finally, since the ability of litigants to appeal decisions is limited and since the decisions do not attract public attention – these are after all *small claims* – judges in these courts enjoy almost complete discretion.

3 Data

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 coverage widened over time. The documents record the names of the judge and each of the litigants and typically include several paragraphs which sketch the arguments made by the litigants and the ruling of the judge. We cover the universe of available decision documents until December 31, 2004 (N=26,444). For each document, 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 a procedure detailed in Appendix B. The accuracy of this procedure derives from the fact (apparent in data from the Israel Population Registry) that there is little overlap between Jewish and Arab names.[7]

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=2,027). For these cases we conduct a comprehensive analysis of the documents.[8] Focusing on mixed cases allows us to examine the situations that are of prime

[7]Note that the data only allows us to distinguish between Jews and Arabs, and not between subgroups (e.g. Moslem and non-Moslem Arabs).

[8]Each document is coded independently by two different coders (law students at the Hebrew University). A third (senior) coder verifies the coding and adjudicates cases where there is an incompatibility across

interest and that resemble standard lab experiments which study ingroup bias.[9]

For the mixed cases, we extract data on the following:

- Court.
- Judge's name (which we later link with biographical information).
- 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.
- Claim subject (e.g. breach of contract, traffic accident etc.).
- Timing of decision (trial dates are not reported in the decision documents, but as mentioned above, the decision is made within seven days of the trial).
- Monetary compensation requested by the plaintiff and whether a counter claim was filed.
- Claim outcome: whether the claim was accepted (partly or fully), rejected, settled outside the court or withdrawn; the monetary transfers; and the legal expenses awarded (if any).

The main analysis in this paper excludes cases that were settled outside the court (121 cases) or withdrawn (58).[10] We also exclude cases with multiple plaintiffs (defendants) such that one plaintiff (defendant) is Jewish and another is Arab (107). Finally, we exclude cases where the court is located in the Occupied Territories (1). This leaves us with 1,748 cases.

Table 1 shows, for each court, the percentage of cases by the ethnicity of the judge, plaintiff and defendant. Most of the cases are in the two northernmost districts (Northern and Haifa). This is largely due to the combination of two factors. First, the Israeli Arab population is concentrated in the north of the country. Second, online coverage of cases began earlier in the north than in other parts of the country. Overall, 31% of the cases in our data were ruled by Arab judges. Arabs make up 44% of the plaintiffs and 56% of the defendants. In several courts there are no Arab judges while in others most of the cases are ruled by Arab judges.

coders in any of the fields (this happened in 14% of the cases).

[9] A comprehensive analysis of the universe of cases would have been prohibitively costly and would not drastically alter our ability to address the questions at hand.

[10] In section 4 we examine the possibility that litigants strategically decide to settle cases outside the court or withdraw them. It might be interesting to note that the share of cases settled outside of court (6%) seems rather low. If this is indeed the case, it could suggest a difficulty in reaching settlement when the litigants are from different ethnic groups (we thank Andrew Daughety, Jennifer Reinganum and Kathryn Spier for making this point).

[**Table 1**]

We use five different measures of the trial outcome. The main measure is a binary variable which takes the value of one if the claim was accepted and zero otherwise. A second outcome variable 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 third 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 counter claim). A fourth 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.

Additional information on judges is obtained from their biographies. Most biographies are available online. The rest were obtained from the court system using freedom of information procedures. Overall, we have 132 judges, fifteen of whom are Arab.

Table 2 provides summary statistics. In terms of outcomes, 73% of the claims were accepted (53% partly). Mean net monetary compensation is NIS 3,079 (roughly $700) and mean net legal expenses is NIS 189. On average, plaintiffs receive 80% of the amount they request. As noted above, 31% of the cases were ruled by Arab judges. Cases are evenly split between male and female judges, with the typical judge around fifty years old and with five years of tenure. There is little variation in terms of judge education, with relatively few judges holding a degree higher than LLB.

[**Table 2**]

Turning to case characteristics, we see that traffic accidents account for almost seventy percent of the claims in our data, while thirteen percent have to do with a breach of contract.[11] In about fifteen percent of the cases the subject of the claim cannot be deduced from the decision document. 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 the trial (it is not possible to distinguish between these two

[11] The high percentage of traffic accidents might be a sign of ethnic segregation: Arabs interact with Jews during this period mostly on the road.

possibilities). This happened in thirteen percent of the cases (with the others coded "defense present"). A counter claim was filed by the defense in nine percent of the cases. There is usually only one plaintiff in a case, but often more than one defendant. Almost all cases were filed by private plaintiffs while the share of private litigants out of the total number of defendants is 74% on average. The vast majority of litigants are male. Finally, for the 660 cases for which we have information on the compensation requested by the plaintiff, the average amount is NIS 6,424 ($1,460).

In terms of timing, there are relatively few cases in 2000-01, as online coverage of decision documents was still limited. Cases are uniformly distributed over the year, with very few cases decided on a weekend.

4 Judicial ingroup bias

In this section we estimate the extent of judicial bias for the entire period under study (2000-04). Based on the court procedures described in section 2.2, our identification strategy assumes that within each court judge assignment is orthogonal to case characteristics. We start by assessing the validity of this assumption.

Table 3 examines differences in observed characteristics of cases assigned to Arab and Jewish judges. The first two columns show mean characteristics for each set of cases, while the third column presents the difference between the two means. The fourth column reports this difference controlling for court fixed effects. Comparing raw overall means, we find some statistically significant differences (column 3). For example, cases assigned to Arab judges are 11 percentage points more likely to have an Arab plaintiff.[12] Similarly, the share of private defendants and the share of male defendants are lower, while the share of male plaintiffs is higher, in cases assigned to Arab judges. However, consistent with the assignment procedure described above, *within* courts there is no significant difference across Arab and Jewish judges in any observable case characteristic (column 4). This lends support to our identification strategy: there is little evidence to suggest that cases assigned to Arab judges in a given court are systematically different from those assigned to Jewish ones.

[Table 3]

The results in Table 3 notwithstanding, a potential concern arises from the fact that since in principle plaintiffs can find out the identity of the judge prior to the trial, they may

[12] The Arab population is geographically concentrated in certain regions, and Arab judges mostly serve in courts in these regions (see Table 1). If plaintiffs tend to file claims in the area in which they live, courts where there is a high concentration of Arab judges will also have a high concentration of Arab plaintiffs.

withdraw their claim if they were assigned a judge of the opposite ethnicity. This could affect our estimates of judicial bias. If – as may seem reasonable – weaker claims are more likely to be withdrawn, then claims handled by a judge of a different ethnicity from that of the plaintiff would be stronger on average than claims handled by a judge of the same ethnicity as the plaintiff. This would produce a downward bias in the estimated judicial ingroup bias. In Appendix Table A1 we test whether cases are more likely to be withdrawn when assigned to a judge of the opposite ethnicity to that of the plaintiff. We find no evidence for such an association. We similarly find no evidence that cases are more (or less) likely to be settled outside of court when assigned to a judge of the opposite ethnicity to that of the plaintiff.

A final potential concern is that under certain circumstances plaintiffs may have the opportunity to choose in which court to submit their claim (see section 2.2). Since we generally do not know where litigants reside, we cannot tell in which cases plaintiffs had such a choice. To the extent that such opportunities were present, one might worry that plaintiffs would tend to file their claims in courts where there is a relatively high proportion of judges from their own ethnic group (call this proportion p). This may bias our estimate of judicial ingroup bias if there is an association between p and the strength of the claim. However, it is again reasonable to assume that such an association – if it exists – will produce a downward biased estimate, as plaintiffs are more likely to choose courts strategically when their claims are weaker.[13]

4.1 Results

We start by presenting general patterns of judicial decisions in the raw data. Figure 1 displays the share of claims accepted by judge and plaintiff ethnicity. The left pair of bars pertains to cases where the plaintiff is Jewish and the defendant is Arab. Seventy nine percent of these claims are accepted when the judge is Jewish while only 72% are accepted when the judge is Arab. This in itself is not necessarily evidence for ingroup bias: for example, it may be

[13] To see this more clearly, consider a plaintiff with a choice between two courts, one closer to her place of residence than the other (call these courts Home and Away). Suppose that the plaintiff knows the p in each court and believes that her chances of winning are higher the higher is p. This plaintiff would incur the cost of submitting her claim in the Away court if and only if this sufficiently improves her chances of winning the case. There are two possibilities. If p is at least as high in the Home as in the Away court, she would submit at Home. In contrast, if p is higher in the Away court she may file there. Now, if the case is "airtight" (i.e. the probability of winning at Home is close to 1) there is little reason to file in the Away court since this cannot significantly improve the chances of winning. However, if the case is sufficiently weak, and the expected gain from filing at the Away court is independent of the strength of the case, there may be sufficient incentive to file at the Away (high-p) court. (A similar conclusion obtains if the expected gain from filing at the Away court decreases with the strength of the claim. Results are ambiguous if the gain increases with the strength of the claim). This would mean that claims filed in courts with a high proportion of judges from the same ethnic group as the plaintiff would be weaker on average.

the case that compared to their Arab colleagues, Jewish judges are somewhat more inclined towards plaintiffs. However, if this was 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 shows that when the plaintiff is Arab, the pattern is reversed: Jewish judges accept 65% of these claims while Arab judges accept 75%.

[Figure 1]

Table 4 presents a differences-in-differences analysis of the raw data. As the top row shows, Arab judges are 3.7 percentage points more likely to accept a claim when the plaintiff is Arab rather than Jewish. Again, in itself this is no evidence for ingroup bias: Arab plaintiffs might on average file stronger claims than Jewish plaintiffs. However, Jewish judges (second row) are 14.4 percentage points *less* likely to accept a claim when the plaintiff is Arab rather than Jewish. The difference in these differences – 18% – provides an indication of the extent of ingroup bias (i.e. by how much are Arab judges more likely than their Jewish colleagues to accept a claim filed by an Arab plaintiff rather than by a Jewish one). It should be emphasized that, absent an ethnicity-free benchmark, it is impossible to speculate on whether and to what extent Jewish judges favor Jewish litigants and Arab judges favor Arab litigants. We revisit this issue in section 5.2 below.

[Table 4]

We now turn to an econometric investigation. Our baseline specification is of the form:

$$\begin{aligned} y_{ijct} &= \alpha_0 + \alpha_1 ArabPlaintiff_i + \alpha_2 ArabJudge_i + \alpha_3 ArabPlaintiff * ArabJudge_i \\ &\quad + \delta_c + \epsilon_{ijct} \end{aligned} \tag{1}$$

where y_{ijct} is the outcome of case i, assigned to judge j, in court c, at time t; δ_c is a court fixed effect; and ϵ_{ijct} is an error term clustered within judge.[14] *ArabPlaintiff, ArabJudge* and the interaction term *ArabPlaintiff*∗*ArabJudge* are indicator variables.

Equation (1) allows for two possible differences across ethnic groups which, as mentioned above, do not necessarily indicate ingroup 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 ingroup bias. Second, it is possible

[14] Notice that while the judge is the relevant treatment and we allow for clustering at this level, the clustering problem is not very central in our setting since the main explanatory variable – *ArabPlaintiff*∗*ArabJudge* – varies within the treatment group. Nonetheless, we allow for clustering at the judge level to address possible within-judge correlations (which might exist even with the judge fixed effects in equation (2) below). This yields slightly higher standard errors than either uncorrected or heteroskedasticity-robust standard errors.

that Arab judges are differently inclined towards plaintiffs than their Jewish colleagues. In other words, α_2 may be nonzero even in the absence of ingroup bias. Our interest is in α_3, which captures ingroup bias.[15]

Column 1 in Table 5 presents the results using the binary outcome measure, i.e. whether the claim was accepted or rejected. The estimates suggest that Arab plaintiffs are fifteen percentage points less likely than Jewish plaintiffs to win a case and that Arab judges are eight percentage points less likely than Jewish judges to accept claims. The main result is in the third row, which shows a positive and highly statistically significant degree of ingroup bias. A claim is seventeen percentage points more likely to be accepted if assigned to a judge of the same ethnicity as the plaintiff.

[Table 5]

We next augment the baseline specification with additional controls. Specifically, we estimate:

$$\begin{aligned} y_{ijct} &= \alpha_0 + \alpha_1 ArabPlaintiff_i + \alpha_3 ArabPlaintiff * ArabJudge_i \\ &\quad + \gamma_j + \lambda tenure_{jt} + X_i'\beta + \eta_t + \delta_c + \epsilon_{ijct} \end{aligned} \tag{2}$$

where γ_j is a judge fixed effect and $tenure_{jt}$ is judge's tenure at the job.[16] The vector X_i is a list of case-specific controls that includes: the number of plaintiffs; the number of defendants; the share of private plaintiffs; the share of private defendants; the share of male plaintiffs; the share of male defendants; the amount of 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. η_t is a vector of year, month and day of week dummies.

In columns 2-4 of Table 5 we progressively add these sets of controls. This significantly increases the explanatory power of the regression. The degree of ingroup bias is, however, robust to the inclusion of the additional variables and has a point estimate of 19.2% in

[15] This coefficient captures a difference-in-differences controlling for court fixed effects (and potentially other factors). To see this note that

$$E(y|ArabJudge,\ JewishPlaintiff,\ controls) - E(y|JewishJudge,\ JewishPlaintiff,\ controls) = \alpha_2$$

$$E(y|ArabJudge,\ ArabPlaintiff,\ controls) - E(y|JewishJudge,\ ArabPlaintiff,\ controls) = \alpha_2 + \alpha_3.$$

[16] The judge fixed effect picks up any time invariant judge characteristics which may affect her rulings. Note that adding the judge fixed effect implies dropping the *ArabJudge* indicator from the model. Note also that we keep the court fixed effect δ_c as a few judges rule in two (and in one case three) nearby courts.

the full specification. This is a slightly stronger effect than that of filing a counter claim (included in the unreported case controls and estimated at -0.154 with a 0.046 standard error).

To check the extent to which these results are driven by the decisions of a single judge, we repeatedly estimate the full regression (column 4 of Table 5), each time removing from the sample cases ruled by a different judge. The point estimate of α_3 in these 132 regressions ranges from 0.158 to 0.207 (and is always highly statistically significant).

We next estimate the extent of ingroup bias using equation (2) with the four alternative outcome measures described above. The results are reported in Table 6. In the first two columns the dependant variable takes three values according to whether the claim was rejected (0), partly accepted (1), or fully accepted (2). The qualitative results, using either OLS or Ordered Probit, are the same as those obtained in Table 5 using the binary outcome measure.

[Table 6]

In column 3 the dependent variable is the net monetary compensation awarded by the judge to the plaintiff (compensation awarded to plaintiff minus compensation awarded to defendant). The results indicate that a plaintiff facing a judge of the same ethnicity receives on average NIS 926 (roughly \$210) more than a similar plaintiff facing a judge of the opposite ethnicity. To put this figure in perspective, recall that the maximum compensation that can be requested in these courts is NIS 17,800 while – in the 660 cases where we have this information – the average compensation requested by the plaintiffs is NIS 6,424.

We next examine the net legal expenses awarded by the judge to the plaintiff (expenses awarded to plaintiff minus expenses awarded to defendant). Legal expenses were awarded in 76% of the cases. The decision on legal expenses is plausibly even more discretionary than the decision to accept or reject the claim and the decision on the amount of compensation to award. The decision to accept a claim is in principle grounded in the judge's reading of the facts of the matter while the compensation awarded is based on the documents (e.g. a car damage assessment) submitted to the court. In contrast, it is hard to establish the appropriate legal expenses, e.g. the amount and value of time expended on the legal procedure (recall no lawyers are allowed in small claims courts). The results (column 4) indicate an ingroup bias of NIS 224 (\$50) in legal expenses. This is roughly 0.45 of the standard deviation of net legal expenses (see Table 2) whereas the bias in net monetary compensation reported in column 3 is 0.24 of the standard deviation of this variable.

Finally, in column 5 the dependent variable is the monetary yield of the claim, defined as the ratio between the net monetary compensation (including legal expenses) awarded

to the plaintiff and the compensation requested by the plaintiff. As mentioned above, the denominator in this ratio is only available for 660 cases. Consequently, the bias is not estimated very precisely. Nonetheless, the point estimate suggests that a plaintiff receives on average 10% more of the amount requested when facing a judge of the same ethnicity.

5 The shadow of terrorism

The previous section establishes the existence of judicial ingroup bias in Israeli small claims courts during 2000-04. An interesting and important question is whether and to what extent this bias is affected by the social environment. In particular, the period under study is characterized by intense levels of ethnically-based violence, which may well lead to stronger ethnic identification. In this section we examine whether variations in terrorism intensity across space and time – which are plausibly exogenous to the legal procedure – affect the extent of judicial ingroup bias. Such an effect would be consistent with the extensive literature on the effects of group salience on ingroup bias.

5.1 Data

We use data on all Palestinian politically motivated fatal attacks inside Israel (i.e. excluding the Occupied Territories). For each attack we have information about date, location, and number of civilian and security forces fatalities.[17] We merge these data with the judicial decision data used above.

Table 7 reports the number of fatalities from terrorist attacks by district and year. Panel A reports civilian fatalities only, while panel B reports total fatalities (civilian and security forces). These figures are normalized by the population in each district and year. The table reveals substantial variation across districts with the most severely hit districts being Jerusalem and Haifa. The intensity of violence increased until 2002 and subsided in the following years. Overall there were 615 fatalities, 514 of them civilian.

[Table 7]

[17] The dataset combines information from several sources: B'Tselem, the Israeli Information Center for Human Rights in the Occupied Territories; The Israeli Ministry of Foreign Affairs; the Israeli National Insurance Institute; and the Israeli Ministry of Defense. See Romanov et al. (forthcoming) for details. Our identification strategy relies on variation in the intensity of ethnic violence in the vicinity of the courts. Hence we cannot use data on (predominantly Palestinian) fatalities in the Occupied Territories. As mentioned above, there was only one case in this period handled in a court located in the Occupied Territories which involved litigants of opposite ethnicities. This case is dropped from our analyses.

In the analysis below, our measure of terrorism intensity is the (population adjusted) number of fatalities from attacks that occurred in a given geographical area around the court during the year preceding the judicial decision. We examine three alternative geographical areas around the court. Natural area is the smallest geographic unit examined, followed by sub-district and district. Our data span 24 natural areas, 15 sub-districts, and 6 districts. Descriptive statistics on fatalities are in the last panel of Table 2.

The use of within-country temporal and spatial variation in terrorism fatalities to identify the effects of terrorism follows a long list of previous studies. A key advantage of this strategy is that it controls for any developments at the national level which are correlated with the country-wide intensity of terrorism and may affect the outcome of interest (see Gould and Klor, forthcoming, for a recent discussion). In our setting, one might imagine that terrorist attacks lead to (or follow) various actions and statements by government officials which could affect judicial decision making country-wide.

Before turning to the results, Table 8 examines whether cases assigned to Arab judges become different from cases assigned to Jewish judges as the number of fatalities in the vicinity of the court increases. For ease of interpretation, the first column shows the overall mean and standard deviation of each of the case characteristics. In columns 2-7 we regress each case characteristic on: (1) an indicator for Arab judge; (2) the per-capita number of fatalities in the vicinity of the court in the year preceding the judicial decision; (3) an interaction between the Arab judge indicator and the number of fatalities; and (4) court fixed effects. The table reports the coefficient on the interaction term (3), which represents the differential effect of terrorism intensity on the characteristics of cases assigned to an Arab versus a Jewish judge. As the table plainly shows, there is little evidence of such differential effects.[18]

[Table 8]

5.2 Results

Figure 2 compares case outcomes (share of claims accepted) when there are no civilian fatalities in the close vicinity (natural area) of the court to outcomes obtained when the number of civilian fatalities is positive. As in Figure 1, both panels indicate the existence of ingroup bias. However, the extent of the bias is significantly smaller in the "No-fatalities" cases, which make up 41% of the total (left panel). A simple difference-in-difference calculation

[18] The only case characteristic that is consistently associated with a differential effect is whether or not the claim was related to a private conflict. There are only 23 (1.3%) such cases in our data.

suggests an ingroup bias of only 6% in these cases.[19] By contrast, the bias in the "Positive number of fatalities" cases is 25%.[20]

[Figure 2]

Another way to examine the effect of fatalities is to augment equation (2) with measures of terrorism intensity interacted with the ethnicity variables. Specifically, we estimate an equation of the form:

$$\begin{aligned} y_{ijct} &= \alpha_0 + \alpha_1 ArabPlaintiff_i + \alpha_3 ArabPlaintiff * ArabJudge_i \\ &\quad + \theta_0 Fatalities_{ct} + \theta_1 Fatalities_{ct} * ArabPlaintiff_i + \theta_2 Fatalities_{ct} * ArabJudge_i \\ &\quad + \theta_3 Fatalities_{ct} * ArabPlaintiff * ArabJudge_i \\ &\quad + \gamma_j + \lambda tenure_{jt} + X_i'\beta + \eta_t + \delta_c + \epsilon_{ijct} \end{aligned} \tag{3}$$

where $Fatalities_{ct}$ is the number of fatalities (per 10,000 population) in the vicinity of court c in the year preceding the judicial decision. Our main interest is in θ_3: the effect of terrorism intensity on judicial ingroup bias.

Table 9 reports the results. The first column replicates the results from the full regression without controlling for terrorism (column 4 of Table 5). Columns 2-7 report results of the augmented regressions for the different measures of $Fatalities_{ct}$. We find strong evidence that terrorism intensity is associated with higher levels of judicial ethnic ingroup bias. The estimated θ_3 in columns 2-4 imply that an additional civilian fatality per 100,000 population in the vicinity of the court is associated with a 2.6-3.9 percentage points larger bias. A similar but somewhat weaker effect is observed when examining total rather than only civilian fatalities (columns 5-7).

[Table 9]

Note also that the estimated α_3 in the second row captures the expected judicial ingroup bias when the number of fatalities is zero (under the econometric specification in equation (3) which assumes linearity of the bias in the number of fatalities). A comparison of the α_3 estimates in columns 2-7 with that reported in column 1 again indicates that in the absence of terrorism the extent of judicial bias is substantially lower.[21]

[19] $(0.789 - 0.764) - (0.720 - 0.757) = 0.062$.

[20] $(0.792 - 0.674) - (0.617 - 0.748) = 0.249$.

[21] An alternative way to estimate ingroup bias in the absence of terrorism is to use an indicator variable for positive number of fatalities (rather than the number of fatalities) as the measure of terrorism intensity.

Finally, the variation in terrorism intensity allows us to address an issue that could not be resolved when estimating the overall judicial bias in section 4. As noted there, in the absence of an ethnicity-free benchmark, one cannot establish whether and to what extent Jewish judges favor Jewish litigants and Arab judges favor Arab litigants. As argued above, however, terrorism intensity is not related to differences in case characteristics across Arab and Jewish judges. Thus, if terrorism increases the salience of ethnicity and thereby strengthens ethnic identification, we can use variations in terrorism intensity to estimate the marginal effect of ethnic identification on judicial bias.[22] Crucially, we can do this separately for Arab and Jewish judges. In Table 10 we hence estimate an equation of the following form, separately for judges of each ethnicity:

$$\begin{aligned} y_{ijct} &= \alpha_0 + \alpha_1 ArabPlaintiff_i + \theta_0 Fatalities_{ct} + \theta_1 Fatalities_{ct} * ArabPlaintiff_i \\ &\quad + \gamma_j + \lambda tenure_{jt} + X_i'\beta + \eta_t + \delta_c + \epsilon_{ijct} \end{aligned} \tag{4}$$

where all the variables are defined as before. Our interest is in θ_1: the effect of terrorism intensity on the differential treatment of Arab versus Jewish plaintiffs.

[Table 10]

Columns 1 and 5 show benchmark results (without controlling for the effect of terrorism). Jewish judges are 11 percentage points less likely, and Arab judges are 7 percentage points more likely, to accept a claim filed by an Arab rather than by a Jewish plaintiff. As emphasized above, we cannot tell whether these estimated effects represent bias on the part of Jewish judges, Arab judges or both. Columns 2-4 examine the effect of terrorism on Jewish judges. While the coefficients are imprecisely estimated, they suggest that terrorism makes Jewish judges *less* likely to accept claims filed by Arab plaintiffs. Similarly, columns 6-8 indicate that terrorism makes Arab judges *more* likely to accept claims filed by Arab plaintiffs. These results seem to indicate that judicial ingroup bias exists on both sides.

6 Conclusion

The voluminous literature on ingroup bias and its determinants has largely relied on lab experiments and (to a lesser extent) on structured field experiments. Two of the major

Estimating equation (3) using binary versions of the fatality variables used in Table 9 yields estimates of α_3 which are for the most part smaller than 0.1 and statistically indistinguishable from zero (the only exception is when using civilian fatalities at the district level, in which case the estimated α_3 is 0.18 with *p-value*=0.09).

[22] This approach is similar to that taken by Benjamin et al. (forthcoming) who identify the marginal behavioral effects of social identities by manipulating the salience of ethnic identities of laboratory subjects.

results in this literature relate to (1) the effect of group membership on individual behavior toward ingroup and outgroup members, and in particular the display of ingroup bias; and (2) the sensitivity of this effect to the salience of group membership. While these results are quite robust, concerns regarding external validity of experimental studies, especially if conducted in the lab, are widespread (e.g. Levitt and List 2007). This paper contributes to our understanding of ingroup bias by examining behavior in naturally occurring data. Using a unique dataset of judicial decisions in Israeli courts, we find support for both of the above experimental results.

Our identification strategy, which relies on plausibly exogenous variation both in the assignment of judges and in the salience of ethnicity, allows us to overcome a major challenge facing the literature on ethnic and racial bias in judicial decisions, namely the potential correlation between ethnicity and unobserved case characteristics.

The overall level of judicial ingroup bias we uncover in the period studied is arguably quite substantial. A claim is 17% to 20% more likely to be accepted if assigned to a judge of the same ethnicity as the plaintiff. While this poses a challenge to the Israeli judicial system, perhaps more important is the fact that the bias is hardly an exogenously given fact. In areas which experienced relatively little ethnic strife the bias is substantially lower. This may suggest the feasibility of debiasing efforts.

From the perspective of the literature on ethnic conflicts, our results shed light on the poorly understood effect of such conflicts on institutions and social norms (see Blattman and Miguel 2010 for a discussion). Specifically, it highlights a possibly important effect of ethnic conflicts, often overlooked in the conflict literature. Indeed, as we have seen, even if the conflict does not directly involve the domestic ethnic groups, by intensifying ethnic identities it can produce distortions in judicial decisions, thus potentially eroding property rights and public trust in the rule of law.

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APPENDIX

A Theoretical framework

This appendix outlines a general model of social identity and relates it to judicial decisions in an ethnically heterogeneous society. The model attempts to capture empirical regularities documented in three well-established strands of research that study behavior in groups: the minimal group paradigm; public goods experiments; and the study of conformity.[23] A more thorough discussion of the model and the underlying evidence can be found in Shayo (2009).

Consider an economy with a set N of individuals and a given set G of *social groups:* $G = \{J|J \subseteq N \text{ is a social group}\}$. For the present purposes it suffices to say that a social group is not any arbitrary subset of the population but an existing category that individuals learn to recognize when living in a society. Denote by G_i the set of social groups to which individual i belongs: $G_i = \{J : J \in G \text{ and } i \in J\}$. We will say that an individual i *identifies* with group J if she prefers outcomes where (1) group J's status is high and (2) her perceived distance from typical members of group J is low (we make the terms status and distance precise in equations 5 and 6 below). Specifically, let T be the set of outcomes of individuals' actions and let $\pi_i(t)$ be i's material payoff from outcome $t \in T$ (e.g. i's monetary transfer resulting from litigation). Let $S_J(t)$ be group J's status and $d_{iJ}(t)$ be i's perceived distance from group J.

Definition 1 *Individual i is said to identify with group J if her preferences over outcomes can be ordered by a utility function of the form:*

$$U_i(t; J) = u(\pi_i(t), S_J(t), d_{iJ}(t))$$

such that u is increasing in $S_J(t)$ and decreasing in $d_{iJ}(t)$.

Given this definition, identification is inferred from individual behavior by revealed preference.

Of course, people do not necessarily care about (or seek to resemble) members of any group they belong to. The following equilibrium concept attempts to capture the endogenous determination of the groups people identify with. For simplicity we assume that each

[23] Standard two-person economic experiments (e.g. dictator, ultimatum and prisoner's dilemma games) have mostly abstracted from group-related issues. Recently, however, a number of studies began incorporating groups into the design of these games. Results, while still relatively scarce, are consistent with the social identity model proposed here. See Bernhard et al. (2006), Charness et al. (2007), Chen and Li (2009), Fowler and Kam (2007) and Goette et al. (2006). Klor and Shayo (2010) study one component of the model in a political economy setting.

individual identifies with a single group. Denote by A_i the set of actions available to individual i and suppose the outcome of individual actions is given by some function $f : A \to T$ (where $A = \times_{i \in N} A_i$ is the set of possible action profiles).

Definition 2 *A Social Identity Equilibrium (SIE) is a profile of actions $a = (a_i)_{i \in N}$ and a profile of social identities $g = (g_i)_{i \in N}$ such that for all $i \in N$ we have $a_i \in A_i$, $g_i \in G_i$ and*

$$U_i\left(f(a_i, a_{-i}); g_i\right) \geq U_i\left(f(a_i', a_{-i}); g_i'\right)$$

for all $a_i' \in A_i$ and all $g_i' \in G_i$.

Thus, SIE requires not only that actions be optimal given what others are doing, but also that each individual's social identity be optimal given her social environment. Specifically, an individual is more likely to identify with a group the higher is its social status and the smaller is the perceived distance between herself and that group.

So far, we have been rather vague about the meaning of perceived distance and status. We now offer specific ways to operationalize these concepts and provide some brief motivation for their role in Definition 2.

Perceived distance. People are less likely to categorize themselves into a given group the higher the difference they perceive between themselves and that group (Turner et al. 1987). A convenient way to model perceived difference is to use the notion of "distance in conceptual space" (e.g. Nosofsky 1986, Gärdenfors 2000). Each individual is characterized by a vector of attributes (or qualities) $q_i = (q_i^1, q_i^2, ..., q_i^H)$. A social group is characterized by the "typical" attributes of its members, denoted q_J. For simplicity assume q_J is the mean across group members, i.e. $q_J = \frac{1}{|J|} \sum_{i \in J} q_i$. q_J is called the *prototype* of group J. The *perceived distance* between individual i and social group J is represented by a weighted Euclidean distance function:

$$d_{iJ} = \left(\sum_{h=1}^{H} w_h (q_i^h - q_J^h)^2 \right)^{1/2} \tag{5}$$

where $0 \leq w_h \leq 1$ and $\sum w_h = 1$. The w's are *attention weights* (Nosofsky 1986): the more salient is attribute h relative to other attributes, the more attention is devoted to it, which is captured by a higher w_h.

This specification allows the social environment to affect perceived distances in two distinct ways. First, distances may change as the attributes of the agents (namely the values of q_i and q_J) change. For example, the higher the fraction of people in a group that speak my language, the more similar I perceive myself to that group. Second – and this is the effect

studied in this paper – perceived distances can change as the attention paid to the various dimensions changes, e.g. as ethnicity becomes more salient relative to other attributes.

As a specific example, consider a binary attribute – call it attribute e – shared by all members of group J and only by them. For concreteness think of e as a specific ethnicity and of J as the ethnic group. That is $q_i^e = 1$ if $i \in J$ and $q_i^e = 0$ otherwise. This means that $q_i^e - q_J^e = 0$ for all members of J. Suppose that there are also other attributes (e.g. rich/poor) which characterize some but not all members of J, such that $q_i^h \neq q_J^h$ for $i \in J$ and some attribute h. From equation (5) we know that in this case $d_{iJ} > 0$ for $i \in J$. Now, consider an exogenous increase in the salience of attribute e, reducing the salience of all other attributes (w_e increases while w_h decreases for all $h \neq e$). This means an increase in the attention paid to an attribute shared by all group members and a decrease in the attention paid to other attributes, which implies that d_{iJ} decreases.

Group status. Studies in social psychology argue that the evaluation of a group is often performed by social comparisons to other groups along valued dimensions of comparisons (Tajfel and Turner 1986). In our setting, one such dimension is material payoffs. Thus, we can think of group status in terms similar to standard treatments of individual status in economics. That is, we can represent the status of group J as a function

$$S_J(t) = \mathcal{S}_J(\overline{\pi}_J(t), \overline{\pi}_{-J}(t)), \tag{6}$$

where $\overline{\pi}_J$ is the mean material payoff of individuals that belong to group J and $-J$ is the reference-group of group J (which in a two-group setting is simply the other group). We assume that the status of group J is strictly increasing in $\overline{\pi}_J$ and is weakly decreasing in $\overline{\pi}_{-J}$.[24] Given equation (6), identification with a group implies caring about the material payoffs of other group members.

Application to Judicial decisions

The above model is grounded primarily in results from lab experiments that document behavior in groups. This paper attempts to shed light on two major implications of the model in naturally occurring data. First, that members of a social group may behave in a way that takes into account the payoffs of other members of their group. Second, that such behavior is more likely to be observed when group-specific attributes become salient.

Consider a judge who is also a member of some ethnic group. The judge is faced with a plaintiff and a defendant, and needs to decide the outcome of the trial, namely the monetary transfer t from defendant to plaintiff (which could be negative). The material payoffs of the

[24] If the status function is constant in $\overline{\pi}_{-J}$, group J's status depends on the group's mean absolute, rather than relative, payoff. As our data consist of decisions that transfer money from an ingroup member to an outgroup member, we cannot distinguish in this paper between actions that enhance relative and absolute group payoffs.

litigants are then simply:

$$\begin{aligned} \pi_{plaintiff} &= t \\ \pi_{defendant} &= -t. \end{aligned}$$

The judge's own material payoff is not directly affected by her ruling, but one might suspect that wrong decisions can entail loss of utility for various reasons (e.g. the existence of strong non-discriminatory norms or reduced prospects of promotion). Denoting by $\widehat{t}$ the "correct" decision, we write

$$\pi_{judge} = -(t - \widehat{t})^2.$$

Let $E, E' \in G$ be two ethnic groups that partition N. Let $q_i^e = 1$ if $i \in E$ and $q_i^e = 0$ otherwise, and denote by w_e the associated attention weight. As in the example above, assume that the groups are not homogeneous, i.e. there are attributes which characterize some but not all members of each group. Finally, assume that attributes and attention weights are not affected by the judicial decision. Denoting by t^* the transfer determined by the judge, we make two observations.

Observation 1 *Suppose plaintiff $\in E$ and defendant $\in E'$. Then $t^* > \widehat{t}$ if the judge identifies with group E; and $t^* < \widehat{t}$ if the judge identifies with group E'.*

Observation 2 *The higher is the relative salience of ethnicity (w_e), the more likely it is that in equilibrium the judge identifies with her ethnic group.*

Using Observation 1, Section 4 in the paper examines the extent of ethnic identification among judges. Section 5 examines Observation 2.

B Coding litigant ethnicity

This appendix describes the procedure we use to code litigant ethnicity. The legal documents do not consistently order first and last names. We therefore decompose each litigant name into its components (separated by spaces) such as "Abraham"+"Benjamin"+"Cohen". There may be up to four such components. We do not impose any assumption regarding the gender of the litigant, nor whether a particular component represents a first, middle or last name. Using an external database derived from the Israel Population Registry, we compute for each component the following conditional probabilities of it being an Arab name:[25]

[25] In our calculations below we assume that all litigants are either Arab or Jewish (without distinguishing between subgroups). According to the Israeli Central Bureau of Statistics, at the end of 2002 76.8% of the Israeli population were Jewish and 19.1% were Arab. The rest are classified as "other": these are mostly immigrants from the Former Soviet Union who are not formally classified as Jewish.

$$
\begin{aligned}
p_{fm} &= p(Arab|first\ name\ and\ male) \\
p_{ff} &= p(Arab|first\ name\ and\ female) \\
p_{lm} &= p(Arab|last\ name\ and\ male) \\
p_{lf} &= p(Arab|last\ name\ and\ female).
\end{aligned}
$$

A name component is designated "Arab" if $\max\{p_{fm}, p_{ff}, p_{lm}, p_{lf}\} > 0.95$ and $\min\{p_{fm}, p_{ff}, p_{lm}, p_{lf}\} > 0.05$. That is, we designate a component as Arab if at least one of the conditional probabilities is very high (i.e., the name component is highly likely to belong to an Arab individual) and none of the conditional probabilities is very low (that is, none of the conditional probabilities suggests that the name component is highly likely to belong to a Jewish individual). Similarly, a component is designated "Jewish" if $\min\{p_{fm}, p_{ff}, p_{lm}, p_{lf}\} \leq 0.05$ and $\max\{p_{fm}, p_{ff}, p_{lm}, p_{lf}\} \leq 0.95$.

A litigant is coded as Arab if at least one of his or her name components is designated as "Arab" and *none* of the other components is designated as "Jewish". Similarly, a litigant is coded as Jewish if at least one of his or her name components is designated as "Jewish" and *none* of the other components is designated as "Arab". This procedure assigns an ethnicity to the vast majority of private litigants (50,294 out of 53,029). The fact that the share of names that are not assigned an ethnicity is very small is consistent with the fact that in Israel there is little overlap in naming conventions across ethnicities and there are virtually no marriages across ethnic lines.[26] To assign ethnicity to the remaining litigants we search for their names in an electronic directory service. This allows us to locate the exact addresses of people bearing these names. Relying on the fact that in Israel Arabs and Jews tend to live in different communities (either different towns and villages, or different neighborhoods within integrated towns), we are able to assign ethnicities to almost all litigants. The few remaining cases are not coded.

[26] For example, in the data derived from the Israel Population Registry, 62.5% of first names are exclusively Jewish (i.e. the empirical probability that the name is associated with an Arab citizen is zero). At the same time, 28.2% of first names are exclusively Arab (i.e. the empirical probability that the name is associated with an Arab citizen is one).

APPENDIX TABLE A1: CLAIMS WITHDRAWN AND SETTLED OUTSIDE COURT

Dependent variable	Claim withdrawn		Claim settled outside court		Claim withdrawn or settled outside court	
	(1)	(2)	(3)	(4)	(5)	(6)
Arab plaintiff	-0.012	0.003	-0.013	-0.005	-0.025	-0.002
	(0.014)	(0.013)	(0.008)	(0.010)	(0.017)	(0.014)
Arab judge	0.056		0.037*		0.093	
	(0.060)		(0.020)		(0.067)	
Arab plaintiff * Arab judge	0.006	-0.010	-0.004	0.004	0.002	-0.005
	(0.024)	(0.019)	(0.012)	(0.014)	(0.026)	(0.020)
Court fixed effects	No	Yes	No	Yes	No	Yes
Judge fixed effects and tenure	No	Yes	No	Yes	No	Yes
Case characteristics	No	Yes	No	Yes	No	Yes
Time controls	No	Yes	No	Yes	No	Yes
Observations	1,919	1,919	1,919	1,919	1,919	1,919
R-squared	0.0135	0.5156	0.0279	0.2385	0.0245	0.6118

Notes: The table reports regressions similar to those in Table 5 (see text for details), replacing the outcome variable with an indicator for whether the claim was withdrawn (columns 1-2), settled outside the court (columns 3-4), or either (columns 5-6). The sample is larger as it includes cases withdrawn and settled outside court. Regressions were estimated by OLS. Standard errors, clustered by judge, are reported in parentheses.
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE 1: CASES BY ETHNICITY OF THE JUDGE, PLAINTIFF AND DEFENDANT

Percent in each category by district and court

District	Court	Judge: Plaintiff: Defendant:	Jewish Jewish Arab	Arab Jewish Arab	Jewish Arab Jewish	Arab Arab Jewish	Cases
Jerusalem	Bet Shemesh		100.0	0.00	0.00	0.00	1
	Jerusalem		51.89	0.00	48.11	0.00	106
Northern	Afula		31.19	31.19	13.76	23.85	109
	Akko		42.34	16.06	35.04	6.57	137
	Bet She'an		83.33	0.00	16.67	0.00	12
	Nazareth		3.88	29.84	11.24	55.04	258
	Qazrin		100.0	0.00	0.00	0.00	3
	Qiryat Shemona		67.65	0.00	32.35	0.00	34
	Tiberias		80.00	0.00	20.00	0.00	5
	Zefat		83.33	16.67	0.00	0.00	12
Haifa	Hadera		63.48	0.00	36.52	0.00	178
	Haifa		34.72	26.39	22.22	16.67	216
	Krayot		42.86	14.29	32.14	10.71	336
Central	Netanya		65.63	0.00	34.38	0.00	32
	Petah Tiqwa		75.00	0.00	25.00	0.00	56
	Ramla		44.90	0.00	55.10	0.00	49
	Rehovot		83.33	0.00	16.67	0.00	12
	Rishon Leziyyon		100.0	0.00	0.00	0.00	11
Tel Aviv-Yafo	Tel Aviv-Yafo		48.89	6.67	41.11	3.33	90
Southern	Ashdod		100.0	0.00	0.00	0.00	4
	Ashqelon		0.00	0.00	100.0	0.00	1
	Be'er Sheva		14.29	30.16	11.11	44.44	63
	Dimona		50.00	0.00	50.00	0.00	4
	Elat		42.86	0.00	57.14	0.00	7
	Qiryat Gat		83.33	0.00	16.67	0.00	12
Total			41.08	15.16	27.75	16.02	1,748

TABLE 2: SUMMARY STATISTICS FOR MAIN VARIABLES

(N=1,748)

Variable category		Variable	Mean	Std. Dev.
Claim outcome		Claim accepted	0.7340	
		– partly accepted	0.5297	
		Claim outcome (0/1/2)	0.9382	0.6832
		Net monetary compensation	3079.3	3923.6
		Net legal expenses	188.8	497.1
		Monetary yield[1]	0.7993	0.4268
Judge characteristics		Arab	0.3118	
		Male	0.5040	
		Age	48.753	12.349
		Tenure at job	4.9897	6.8061
		Immigrant (Jewish)	0.2540	
		LLB degree – Hebrew U.	0.4943	
		– Tel Aviv U.	0.3902	
		– Bar Ilan U.	0.0715	
		– other institutions	0.0441	
		Highest degree – LLB	0.9033	
		– master	0.0664	
		– doctoral	0.0303	
Case characteristics	Claim subject	Breach of sales contract	0.0320	
		Breach of service contract	0.0950	
		Housing related	0.0109	
		Private conflict	0.0132	
		Traffic accident	0.6894	
		Miscellaneous	0.0126	
		Missing	0.1470	
	Defense	Defense present	0.8661	
		Defense made a counter claim	0.0881	
	Number of litigants	Plaintiffs	1.1127	0.3181
		Defendants	1.7243	0.7134
	Private litigants (share of total)	Plaintiffs	0.9982	0.0311
		Defendants	0.7369	0.2584
	Male litigants (share of private)	Plaintiffs	0.8212	0.3643
		Defendants	0.8747	0.3128
	Compensation[1]	Requested by plaintiff/s	6423.9	5085.4
Timing of judicial decision	Year	2000	0.0023	
		2001	0.0864	
		2002	0.2294	
		2003	0.3450	
		2004	0.3370	

TABLE 2 – CONTINUED

Variable category		Variable	Mean	Std. Dev.
Timing of judicial decision	Month	January	0.0721	
		February	0.0864	
		March	0.0921	
		April	0.0727	
		May	0.0841	
		June	0.0830	
		July	0.0755	
		August	0.0864	
		September	0.0921	
		October	0.0841	
		November	0.0984	
		December	0.0732	
	Weekday	Sunday	0.2117	
		Monday	0.1842	
		Tuesday	0.2088	
		Wednesday	0.1665	
		Thursday	0.1768	
		Friday	0.0509	
		Saturday	0.0011	
Terrorism fatalities[2]	Natural area	Civilian	0.2817	0.3518
		Total	0.3231	0.4034
	Sub district	Civilian	0.2510	0.2580
		Total	0.3068	0.3374
	District	Civilian	0.2299	0.2206
		Total	0.2900	0.2789

Note:
[1] Data on compensation requested by plaintiff/s and monetary yield are available for 660 cases.
[2] "Terrorism fatalities" = civilian or total (civilian and security forces) fatalities from terrorist attacks in the natural area/sub-district/district of the court in the year preceding the judicial decision per 10,000 population; data on lagged fatalities are available for 1,744 cases.

TABLE 3: BALANCING TESTS FOR ASSIGNMENT OF JUDGES

	Mean		Difference in means Arab vs. Jewish judge		Obs.
	Arab judge	Jewish judge	Without court FEs	With court FEs	
	(1)	(2)	(3)	(4)	(5)
Arab plaintiff	0.514	0.403	0.111***	-0.013	1,748
			[0.025]	[0.032]	
Number of plaintiffs	1.112	1.113	-0.001	-0.010	1,748
	(0.316)	(0.319)	[0.016]	[0.021]	
Number of defendants	1.756	1.710	0.046	-0.033	1,748
	(0.708)	(0.716)	[0.037]	[0.047]	
Private plaintiffs (share of total)	0.998	0.998	0.000	0.001	1,748
	(0.030)	(0.031)	[0.002]	[0.002]	
Private defendants (share of total)	0.719	0.745	-0.026**	-0.006	1,748
	(0.255)	(0.259)	[0.013]	[0.017]	
Male plaintiffs (share of private plaintiffs)	0.850	0.808	0.041**	0.036	1,748
	(0.341)	(0.374)	[0.019]	[0.024]	
Male defendants (share of private defendants)	0.846	0.888	-0.042***	-0.031	1,748
	(0.344)	(0.297)	[0.016]	[0.021]	
Claim subject - Breach of sales contract	0.024	0.036	-0.012	-0.003	1,748
			[0.009]	[0.012]	
Claim subject - Breach of service contract	0.095	0.095	0.001	0.032	1,748
			[0.015]	[0.020]	
Claim subject - Housing related	0.004	0.014	-0.010*	-0.009	1,748
			[0.005]	[0.007]	
Claim subject - Private conflict	0.011	0.014	-0.003	0.007	1,748
			[0.006]	[0.008]	
Claim subject - Traffic accident	0.692	0.688	0.003	-0.043	1,748
			[0.024]	[0.029]	
Claim subject - Miscellaneous	0.009	0.014	-0.005	-0.005	1,748
			[0.006]	[0.008]	
Claim subject - Missing	0.165	0.139	0.026	0.022	1,748
			[0.018]	[0.022]	
Defense present	0.848	0.874	-0.027	-0.031	1,748
			[0.018]	[0.021]	
Defense made a counter claim	0.077	0.093	-0.016	-0.001	1,748
			[0.015]	[0.019]	
Compensation requested (NIS)	6,481	6,401	80	942	660
	(5,260)	(5,018)	[437]	[619]	

Notes: 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 Arab judge.
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE 4: DIFFERENCES IN DIFFERENCES

share of claims accepted

	Arab plaintiff	Jewish plaintiff	Difference
Arab judge	0.754 (0.026) [N=280]	0.717 (0.028) [N=265]	0.037 (0.038) [N=545]
Jewish judge	0.647 (0.022) [N=485]	0.791 (0.015) [N=718]	-0.144*** (0.026) [N=1,203]
Difference	0.106*** (0.035) [N=765]	-0.074** (0.030) [N=983]	0.180*** (0.046) [N=1,748]

Notes: Standard errors are reported in parentheses.
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE 5: JUDICIAL INGROUP BIAS

Dependent variable: claim accepted				
	(1)	(2)	(3)	(4)
Arab plaintiff	-0.151***	-0.150***	-0.121***	-0.117***
	(0.026)	(0.029)	(0.030)	(0.031)
Arab judge	-0.077*			
	(0.044)			
Arab plaintiff * Arab judge	0.170***	0.166***	0.199***	0.192***
	(0.054)	(0.056)	(0.049)	(0.049)
Court fixed effects	Yes	Yes	Yes	Yes
Judge fixed effects and tenure	No	Yes	Yes	Yes
Case characteristics	No	No	Yes	Yes
Time controls	No	No	No	Yes
Observations	1,748	1,748	1,748	1,748
R-squared	0.0439	0.1383	0.2377	0.2479

Notes: Regressions are estimated by OLS. Standard errors, clustered by judge, are reported in parentheses. Missing values for monetary compensation requested by plaintiff are dummied out.

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

TABLE 6: JUDICIAL BIAS – ALTERNATIVE OUTCOME MEASURES

Dependent variable	Claim outcome {0,1,2}		Net monetary compensation	Net legal expenses	Monetary yield
Estimation methodology	OLS	Ordered Probit	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
Arab plaintiff	-0.138***	-0.369***	-662.3***	-135.3**	-0.052
	(0.040)	(0.005)	(248.8)	(53.7)	(0.041)
Arab plaintiff * Arab judge	0.208***	0.587***	925.7**	224.3***	0.101*
	(0.057)	(0.009)	(448.3)	(84.5)	(0.059)
Court fixed effects	Yes	Yes	Yes	Yes	Yes
Judge fixed effects and tenure	Yes	Yes	Yes	Yes	Yes
Case characteristics	Yes	Yes	Yes	Yes	Yes
Time controls	Yes	Yes	Yes	Yes	Yes
Observations	1,748	1,748	1,748	1,748	660
R-squared/Pseudo R-squared	0.5473	0.4014	0.4298	0.2291	0.5683

Notes: 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 column 3 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 column 4 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 column 5 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. In columns 1-4 missing values for monetary compensation requested by the plaintiff are dummied out. In column 5 the monetary compensation requested by the plaintiff is not included in the case characteristics. Standard errors, clustered by judge, are reported in parentheses.

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

TABLE 7: FATALITIES FROM TERRORIST ATTACKS

per 10,000 population

Panel A: Civilian fatalities

	2000	2001	2002	2003	2004	2000-04
Jerusalem	0.04	0.39	1.06	0.71	0.23	2.44
Northern	0.00	0.09	0.16	0.05	0.00	0.30
Haifa	0.04	0.34	0.49	0.44	0.00	1.30
Central	0.00	0.10	0.36	0.03	0.00	0.49
Tel Aviv	0.00	0.19	0.14	0.23	0.03	0.58
Southern	0.00	0.00	0.00	0.01	0.33	0.36
Total	0.01	0.17	0.33	0.21	0.08	0.80

Panel B: Total fatalities

	2000	2001	2002	2003	2004	2000-04
Jerusalem	0.04	0.40	1.11	0.73	0.26	2.55
Northern	0.04	0.14	0.20	0.07	0.03	0.47
Haifa	0.04	0.39	0.88	0.46	0.00	1.77
Central	0.00	0.11	0.37	0.09	0.00	0.57
Tel Aviv	0.00	0.26	0.15	0.27	0.03	0.70
Southern	0.00	0.00	0.07	0.02	0.33	0.44
Total	0.02	0.20	0.41	0.23	0.10	0.96

Notes: Total fatalities refer to the sum of civilian and security forces fatalities. In the last column of both panels the cumulative number of fatalities in 2000-04 is divided by the average population in that period. Fatality data from Romanov et al. (2010). Population data from Israel Central Bureau of Statistics.

TABLE 8: BALANCING TESTS FOR ASSIGNMENT OF JUDGES IN THE PRESENCE OF TERRORISM

	Mean	Civilian fatalities			Total fatalities			Obs.
		Natural Area	Sub-District	District	Natural Area	Sub-District	District	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arab plaintiff	0.438	0.224**	0.170	0.147	0.194*	0.172	0.093	1,744
		[0.111]	[0.130]	[0.162]	[0.102]	[0.116]	[0.119]	
Number of plaintiffs	1.113	-0.043	-0.050	-0.033	-0.037	-0.030	-0.004	1,744
	(0.318)	[0.072]	[0.085]	[0.106]	[0.066]	[0.076]	[0.078]	
Number of defendants	1.724	0.098	0.183	0.191	0.056	0.131	0.055	1,744
	(0.713)	[0.160]	[0.189]	[0.235]	[0.147]	[0.168]	[0.173]	
Private plaintiffs (share of total)	0.998	0.008	0.002	0.006	0.008	0.004	0.006	1,744
	(0.031)	[0.006]	[0.007]	[0.009]	[0.006]	[0.006]	[0.007]	
Private defendants (share of total)	0.737	0.038	-0.002	0.014	0.040	0.004	0.025	1,744
	(0.258)	[0.058]	[0.068]	[0.085]	[0.053]	[0.061]	[0.062]	
Male plaintiffs (share of private plaintiffs)	0.821	0.035	0.037	0.134	0.045	0.032	0.094	1,744
	(0.364)	[0.083]	[0.098]	[0.122]	[0.077]	[0.087]	[0.090]	
Male defendants (share of private defendants)	0.875	0.100	0.005	0.038	0.084	0.007	0.046	1,744
	(0.313)	[0.072]	[0.084]	[0.105]	[0.066]	[0.075]	[0.077]	
Claim subject - Breach of sales contract	0.032	-0.017	-0.011	0.013	-0.011	-0.010	0.017	1,744
		[0.040]	[0.047]	[0.059]	[0.037]	[0.042]	[0.043]	
Claim subject - Breach of service contract	0.095	-0.026	-0.064	-0.008	-0.033	-0.057	-0.016	1,744
		[0.067]	[0.079]	[0.098]	[0.061]	[0.070]	[0.072]	
Claim subject - Housing related	0.011	0.017	0.023	0.041	0.012	0.013	0.023	1,744
		[0.023]	[0.028]	[0.034]	[0.022]	[0.025]	[0.025]	
Claim subject - Private conflict	0.013	0.086***	0.124***	0.160***	0.090***	0.120***	0.137***	1,744
		[0.026]	[0.031]	[0.038]	[0.024]	[0.027]	[0.028]	
Claim subject - Traffic accident	0.689	-0.098	-0.001	-0.099	-0.110	0.011	-0.105	1,744
		[0.100]	[0.118]	[0.147]	[0.092]	[0.105]	[0.108]	
Claim subject - Miscellaneous	0.013	0.011	0.015	0.058	0.011	0.010	0.056**	1,744
		[0.026]	[0.030]	[0.038]	[0.024]	[0.027]	[0.028]	
Claim subject - Missing	0.147	0.027	-0.087	-0.165	0.041	-0.087	-0.112	1,744
		[0.075]	[0.088]	[0.109]	[0.068]	[0.078]	[0.008]	
Defense present	0.866	-0.037	0.073	0.125	-0.051	0.069	0.088	1,744
		[0.071]	[0.084]	[0.104]	[0.065]	[0.075]	[0.077]	
Defense made a counter claim	0.088	0.004	0.016	0.032	-0.019	-0.024	-0.043	1,744
		[0.065]	[0.076]	[0.095]	[0.059]	[0.068]	[0.070]	
Compensation requested (NIS)	6,424	1,298	2,232	2,822	898	1,401	938	658
	(5,085)	[2,022]	[2,822]	[3,580]	[1,866]	[2,560]	[2,823]	

Notes: Standard deviations in parentheses in column 1. Standard errors in brackets in columns 2-7. Each entry in columns 2-7 is derived from a separate OLS regression where the explanatory variables are: (1) an indicator for Arab judge; (2) the number of civilian/total (civilian and security forces) fatalities from terrorist attacks in the natural area/sub-district/district of the court in the year preceding the judicial decision per 10,000 population; (3) an interaction between the Arab judge indicator and the number of fatalities; and (4) court fixed effects. The Table reports the coefficient on the interaction term (3).
*, **, *** represent statistical significance at the 10, 5, and 1 percent levels.

TABLE 9: TERRORISM AND JUDICIAL BIAS

Dependent variable: claim accepted							
		Civilian fatalities			Total fatalities		
		Natural Area	Sub-District	District	Natural Area	Sub-District	District
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arab plaintiff	-0.117***	-0.085**	-0.089**	-0.084**	-0.084**	-0.095**	-0.093**
	(0.031)	(0.035)	(0.037)	(0.041)	(0.036)	(0.039)	(0.041)
Arab plaintiff * Arab judge	0.192***	0.128**	0.109*	0.112*	0.125**	0.115*	0.135**
	(0.049)	(0.050)	(0.057)	(0.058)	(0.052)	(0.061)	(0.063)
Fatalities		0.075*	0.022	0.040	0.052	0.012	0.005
		(0.044)	(0.067)	(0.105)	(0.039)	(0.040)	(0.069)
Fatalities * Arab plaintiff		-0.088	-0.090	-0.112	-0.080*	-0.057	-0.064
		(0.059)	(0.086)	(0.106)	(0.048)	(0.057)	(0.070)
Fatalities * Arab judge		-0.152	-0.113	-0.216	-0.123	-0.081	-0.110
		(0.123)	(0.145)	(0.163)	(0.104)	(0.116)	(0.081)
Fatalities * Arab plaintiff * Arab judge		0.256***	0.353***	0.387***	0.240***	0.288***	0.213*
		(0.089)	(0.118)	(0.142)	(0.077)	(0.101)	(0.110)
Court fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects and tenure	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,748	1,744	1,744	1,744	1,744	1,744	1,744
R-squared	0.2479	0.2468	0.2465	0.2464	0.2467	0.2463	0.2458

Notes: "Fatalities" is the number of civilian/total (civilian and security forces) fatalities from terrorist attacks in the natural area/sub-district/district of the court in the year preceding the judicial decision per 10,000 population. Regressions are estimated by OLS. Missing values for monetary compensation requested by plaintiff are dummied out. Standard errors, clustered by judge, are reported in parentheses.

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

TABLE 10: TERRORISM AND JUDICIAL BIAS BY JUDGE ETHNICITY

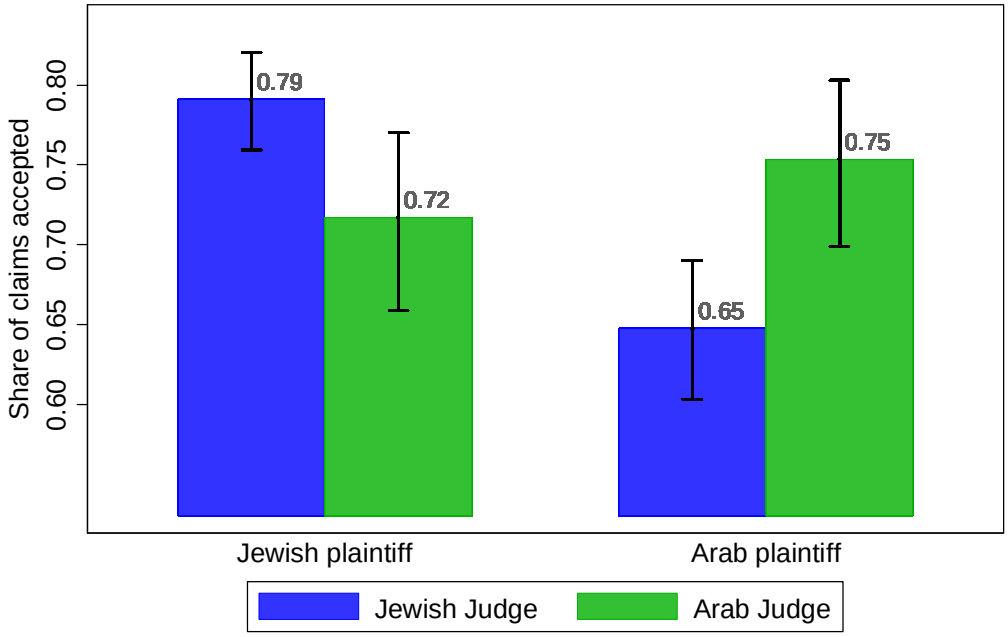
Dependent variable: claim accepted (outcome=1 or 2)

	Jewish Judge				Arab Judge			
		Natural Area	Sub-District	District		Natural Area	Sub-District	District
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arab plaintiff	-0.113***	-0.082**	-0.086**	-0.078*	0.072*	0.048	0.029	0.032
	(0.034)	(0.036)	(0.037)	(0.041)	(0.039)	(0.033)	(0.043)	(0.038)
Fatalities		0.073	0.011	0.038		-0.071	-0.095	-0.193
		(0.047)	(0.073)	(0.119)		(0.105)	(0.132)	(0.188)
Fatalities * Arab plaintiff		-0.085	-0.086	-0.116		0.125	0.207**	0.234***
		(0.059)	(0.085)	(0.103)		(0.074)	(0.073)	(0.065)
Court fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects and tenure	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,203	1,199	1,199	1,199	545	545	545	545
R-squared	0.2857	0.2834	0.2827	0.2828	0.2241	0.2253	0.2261	0.2260

Notes: "Fatalities" is the number of civilian/total (civilian and security forces) fatalities from terrorist attacks in the natural area/sub-district/district of the court in the year preceding the judicial decision per 10,000 population. Regressions are estimated by OLS. Missing values for monetary compensation requested by plaintiff are dummied out. Standard errors, clustered by judge, are reported in parentheses.

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

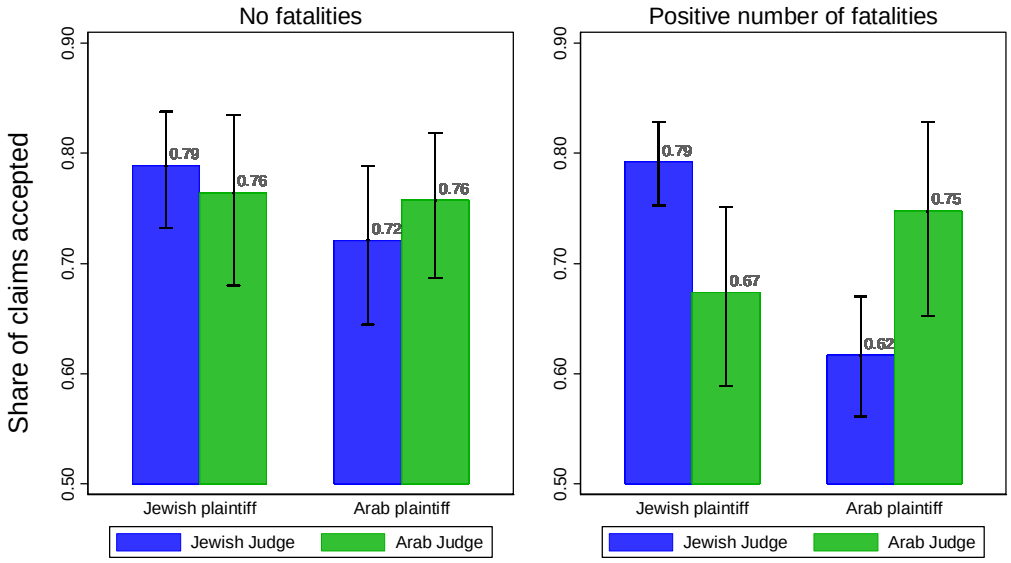
Figure 1: Baseline Ingroup Bias



Based on 1748 mixed cases. Capped ranges indicate 95% confidence intervals.

Figure 2: Terrorism and Ingroup Bias

By number of civilian fatalities in natural-area in the preceding year



Based on 1744 mixed cases, 711 with no fatalities in the natural area in the year preceding the trial.
Capped ranges indicate 95% confidence intervals.