Does Privacy Matter? Evidence from a Legal Reform*

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Abstract

We investigate the impact of a unique reform that granted anonymity to plaintiffs in court rulings for personal injury claims. Our analysis measures changes in payments for personal injury versus property damage claims (which were not affected by the reform) for car accident cases handled by the major insurance firms in Israel between 2011-2020. Using a difference-in-differences framework, we find that the reform led to a 17% increase in payments for personal injury claims. We interpret this estimate as the cost claimants were prepared to bear prior to the reform, through accepting lower settlements, to avoid the privacy loss incurred by a court ruling. This result underscores the economic value individuals place on privacy, revealing that concerns about public exposure can significantly influence decision-making and negotiation outcomes. Our findings demonstrate the importance of privacy considerations in customer interactions where sensitive information is often involved.

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1. Introduction

The emergence of firms such as Google and Meta, which utilize personal information, has raised growing concerns regarding privacy protection. Data that were previously unavailable or could have been obtained only through significant investments are now often accessible or can be exposed with minimal effort. In response, legislators, regulators, and courts have devised various rules aimed at enhancing the protection of personal information.

But are these concerns justified? Existing research presents a complex picture. Surveys that investigate people's attitudes to privacy-related policies indicate that privacy protection is broadly supported. At the same time, however, laboratory experiments show that privacy often matters little; people have been consistently found to disclose personal information in exchange for small payments or minor benefits. This gap between stated privacy preferences and actual behavior, known as the privacy paradox, has sometimes led policymakers and researchers to question the need for extensive privacy protection.[1]

In this paper, we examine the implications of a unique legislative reform in Israel that granted protection of litigants' privacy. The names of parties involved in court-resolved legal disputes and the relevant ruling are conventionally publicly available. Simple (and often free) online search allows members of the public to trace and then read court decisions containing personal information on the litigants. The reform we study represents a significant departure from the entrenched principle of public legal proceedings. This legislation, which was passed in July 2014 and came into effect in August 2015, stipulated that in all lawsuits involving personal injury, the plaintiff would remain anonymous.

We were able to obtain proprietary data from the top five insurance companies in Israel. These data contain all payments – for personal injury and property damage claims – made through a settlement or following a court judgment between the years 2011 and 2020. In our analysis, we focus on claims arising from traffic accidents because they provide two advantages. First, traffic accidents constitute the main category of personal injury claims. Second, traffic accidents give rise to both personal injury and property damage claims. While the reform grants anonymity to plaintiffs in personal injury lawsuits, similar lawsuits for property damage remain under the traditional rule in which the parties' names are not

[1] Based on the privacy paradox, Joshua Wright – former FTC commissioner – noted that the "FTC's enforcement posture is likely to be too aggressive by failing to consider this empirical evidence ... FTC is using its bully pulpit to cajole companies into supplying too much privacy" (Cooper and Wright, 2018). See also Fuller (2019). This view of empirical research as providing consistent evidence of low privacy valuations is strongly debated in Acquisti et al. (2023) and further discussed in our review of the literature.

concealed. Thus, property damage claims resulting from road accidents provide a natural control group.

We examine how the legal reform affected payments from insurance companies to claimants in personal injury cases due to car accidents relative to payments from these same insurance companies in property damage cases. We find that the reform resulted in a significant and consistent 17% increase in the payments made by insurance companies for personal injury claims. Given that the vast majority (98%) of personal injury claims result in a settlement, the increase in payments is driven by an increase in settlement amounts and not by a change in court rulings.

Personal injury claims often contain sensitive personal information, both regarding claimants' income (before and after the accident) and their health status. If privacy is important, one may expect the new anonymity regime to strengthen claimants' bargaining leverage vis-à-vis their insurer, resulting in more favorable settlements. As court rulings no longer include the plaintiff's name, claimants who have been offered low settlement amounts may now credibly threaten to proceed with their claim to a judgment. On the other hand, if privacy is not a central value, as some of the literature on the privacy paradox suggests, settlement amounts would be unaffected by the reform. We thus interpret our primary finding as indicating that privacy has a real impact.

Following this interpretation, we analyze the reform's impact on claimants who face different levels of privacy disclosure. First, we find that the change in payments is mainly driven by claimants of working age. These claimants are likely to be sensitive to privacy disclosure for two key reasons: fear that information about their physical injury would negatively impact their career, and desire to keep their income information confidential. Both concerns are of limited relevance for older, retired claimants. Second, we find that the overall effect is more pronounced when claimants are represented by more prestigious lawyers, and when the time it takes for payments to be made is longer. One plausible interpretation of this result is that both factors are correlated with the severity of claimants' injury.[2] These findings are thus consistent with the view that privacy matters more when the injuries are severe. Finally, we do not find the overall effect to systematically vary by gender or income, but we find a more pronounced (albeit not statistically significant) effect of the reform on non-Jews and ultra-Orthodox Jews.

[2]As prior literature has suggested, victims with more severe injuries tend to be represented by higher-quality lawyers (see, e.g., Zamir, Medina and Segal (2014)).

We view our main contribution as offering empirical evidence on the economic value individuals place on privacy protection in a high-stakes real-world setting. As Preibusch (2013) and Kokolakis (2017) point out, the empirical literature on privacy is predominantly predicated on the results of survey questionnaires and laboratory experiments. Surveys exploring public sentiments toward privacy-related policies consistently reveal broad support for policies safeguarding privacy (Rainie et al., 2013; Morey, Forbath and Schoop, 2015; Acquisti, Brandimarte and Loewenstein, 2020).[3] However, findings from laboratory experiments present a different picture, revealing inconsistencies in privacy valuations and indicating that individuals often ascribe minimal value to safeguarding their privacy.[4]

One suggested explanation for this privacy paradox is that privacy breaches explored in the experimental literature may not align with the types of privacy-related policies analyzed in the survey literature. In other words, perhaps the findings that suggest that individuals place minimal value on privacy relate primarily to the exposure of non-meaningful information. Thus, for example, Athey et al.'s study concerns the disclosure of names of students' friends which may be of little value in the eyes of students (see Athey, Catalini and Tucker (2017)). However, other experimental studies have also reported low values of privacy even when focusing on more meaningful information, such as their home address or monthly income (Spiekermann, Grossklags and Berendt, 2001; Beresford, Kübler and Preibusch, 2012). Additionally, recent work in a non-experimental setting on GDPR regulation suggests that most users ascribe little weight to keeping their browsing history private, despite it being used to elicit detailed information on their economic and health status (Aridor, Che and Salz (2023)).This finding is also supported in work by Kummer and Schulte (2019) who studied the market for mobile apps and Johnson, Shriver and Du (2020) who consider data collection by advertising agencies. Kummer and Schulte conclude that consumers' revealed valuations for privacy are so low that offering privacy protection would not be profitable for most businesses. Johnson et al. document the low opt-out rates of consumers in allowing advertising agencies to collect their data.

Another explanation for the privacy paradox is that the valuation individuals give to privacy is context dependent. A field experiment by Acquisti, John and Loewenstein (2013) found that the amount individuals are willing to pay for privacy protection is highly sensitive to

[3]In line with these privacy attitudes, Goldfarb and Tucker (2012) find that between 2001 and 2008, survey respondents have become more cautious in releasing their private information.

[4]See Norberg, Horne and Horne (2007) for an overview of this privacy paradox and Tomaino, Wertenbroch and Walters (2023) and Lee and Weber (2024) for evidence of preference reversals when individuals set monetary prices for disclosing personal information.

endowment and ordering effects. Despite previous work in the mobile app market showing that individuals are unwilling to pay for privacy, Bian, Ma and Tang (2021) find that when Apple required app developers to release a privacy "nutrition" label, it resulted in a 14% decrease in downloads compared to downloads of the Android app where this information remained hidden. Additionally, in an RCT run on a Chinese Fintech lending platform, Tang (2019) finds that when loan applicants are required to provide their social network id (or employer contact information), they are 11% (7%) less likely to complete a loan application. In a back-of-the-envelope calculation, she concludes that individuals value this information at $33. Our analysis complements this literature by providing findings that are based on the behavior of individuals concerning critical private information, over an extended period, and in a context with direct significant economic incentives.

We are aware of one other empirical paper that examines the implications of granting anonymity to litigants. Arrington (2019) shows that victims of hepatitis C-tainted blood products in Japan were more likely to take part in litigation attempts than in Korea, where the rules regarding plaintiffs' anonymity were stricter. This study primarily relies on interviews and suggests that anonymity may impact litigant behavior. Our paper also relates to studies exploring variations in attitudes concerning the value of privacy. Prior studies have demonstrated that contingencies such as wealth and gender may affect people's attitudes to privacy protection, suggesting that privacy might be a primary concern to some, but not all, social groups (Sheehan, 1999; Park, 2015).

There is also a body of theoretical literature in management and economics that focuses on privacy in market interactions. This literature analyzes how firms use private consumer information to extract surplus through price discrimination (see, for example, Jones and Tonetti (2020); Farboodi and Veldkamp (2020); Cong, Xie and Zhang (2021); Fainmesser, Galeotti and Momot (2023)). Ke and Sudhir (2023) argue that because collecting consumer information enables personalization, which can benefit consumers, privacy legislation such as GDPR may inadvertently harm consumers. Similarly, Casadesus-Masanell and Hervas-Drane (2015) examine the trade-offs firms face in markets where revenue depends on both consumer purchases and selling consumer information to third parties. These studies differ from the current study as, in our context, the party triggering the disclosure of private information (insurance companies) is not interested in selling it to a third party. Instead, the concern lies in the risk that such information could become accessible to third parties (e.g., future employers, and social acquaintances) who might exploit it, thereby granting the firm power over the consumer. Our paper seeks to empirically establish the value of privacy

that includes, but is not limited to, concerns about future market interactions.

Finally, our study contributes to the law and economics literature on litigation incentives. The question of whether, and under what circumstances, parties' names in legal proceedings should be anonymized has resulted in conflicting decisions among courts (Ressler, 2004; Balla, 2010; Volokh, 2022). Our study demonstrates the economic significance of this judicial debate, specifically its impact on claimants' bargaining power during settlement negotiations. In cases involving personal injuries, protecting plaintiffs' anonymity may hold specific significance due to the extensive details about their physical condition and income present in court rulings.[5] While the economics literature has extensively analyzed factors influencing settlements (for a review, see Shavell (2004); Kobayashi (2017)), it has not yet considered the potential influence of the right to privacy.

In most prior work, the consequences of revealing personal data are often unclear – as in the case of targeted advertising or the sharing of friends' contact information. Even when more sensitive information is collected, individuals may trust that it will remain confidential. In contrast, our setting involves a well-defined and highly visible privacy risk in a real-world setting: court proceedings that publicly disclose private health and income details. Here, the privacy loss has immediate and tangible consequences, impacting future job opportunities and personal relationships. By analyzing behavior in this context, we provide evidence on privacy preferences when individuals are more clearly aware of the risks. More broadly, our findings suggest that the low privacy valuations observed in other settings may reflect individuals' underestimation of the likelihood or consequences of information disclosure, rather than their true economic valuation of privacy protection.

The remainder of the paper is organized as follows. The next section provides institutional background on the insurance claims process in Israel and an overview of the claims database created for this project. In Section 3 we outline our empirical strategy. Section 4 estimates the average impact of the reform and explores heterogeneity in the response of claimants to the reform as well as robustness tests. Section 5 concludes.

[5]Judge Richard Posner has famously observed that litigants "are reluctant to disclose their net worth in any circumstances." Kemezy v. Peters (1996).

2. Setting and Data

2.1. Institutional Background

Car Insurance Under Israeli law, car owners must carry insurance for bodily injury, which is legally required and covers all individuals harmed in an accident - drivers, passengers, and pedestrians - regardless of fault. Property damage, by contrast, is insured through an optional comprehensive policy that covers both the insured vehicle and third-party property. Many vehicle owners obtain these two policies from different insurers.

Processing claims The procedural framework in Israel for initiating a claim subsequent to a traffic accident, whether concerning property damage or personal injury, closely parallels that of the United States. The claimant typically engages with their insurer and presents substantiating evidence of the incident. Subsequently, the insurance company decides whether to make an offer. If an offer is extended and accepted, prompt resolution of the claim ensues. However, in the absence of an offer or if the proposed amount is deemed insufficient by the claimant, the involved parties enter into a negotiation phase. At any juncture, the claimant can take the case to court. Following the initiation of legal proceedings, negotiations may continue until the court issues its final judgment.

Claimants may opt for legal representation. As in many other countries, the majority of claimants who suffer personal injury seek legal counsel. In cases of personal injury, attorneys' payment is in the form of a contingent fee and is subject to statutory regulation (capped at 13% of total payments). In instances of property damage, legal representation is significantly less common. One contributing factor is that when claimants sue in a small-claims court, they are mandated to personally advocate their position, with legal representation precluded.

Claimants' payments - whether for property damage or personal injury – follow the rules for calculating damages in tort law. The underlying principle is to restore the claimant to their pre-harm position. In property damage cases, this is done by an appraiser who determines the cost of fixing or replacing the damaged property. In personal injury cases, damages are based on three major categories: lost earnings, medical expenses, and non-pecuniary losses (e.g., pain and suffering). This requires the claimant to provide data on their income before and after the incident, as well as information about their physical and mental condition following the incident. Consequently, claimants are compelled to disclose information that is often considered private.

Privacy in legal proceedings Courts have long emphasized that the public nature of legal proceedings, including the disclosure of parties' names, constitutes a fundamental characteristic of democratic legal systems.[6] It is thus not surprising that in Western countries courts' decisions are accessible. With the emergence of online services, interested parties such as employers or potential social acquaintances can now readily search and find this information.

Yet the principle of public legal proceedings combined with the availability of information on litigants comes at a price. Litigants may be deterred from pursuing their claims in court in order to protect private information, such as their income and health status. Specifically, plaintiffs concerned about their privacy may agree to settle for lower amounts in order to avoid the costs associated with disclosing their personal data in a court ruling. The Israeli Supreme Court, in a personal injury case adjudicated a few weeks prior to the enactment of the reform, noted the concerns that "insurance companies or defendants may pressure plaintiffs to settle in order to prevent their names and personal affairs from being exposed on the front pages of newspapers or on internet websites" (Ploni v. Hapool, 2014). Similarly, US courts have acknowledged the potential impact of anonymity on the bargaining power of litigants during settlement negotiations. For example, a US federal judge recently noted that "naming [the defendant] may pressure him into a settlement. If the defendant were named, he would likely feel significant pressure to settle this case regardless of the merits of the plaintiff's allegations" (Doe v. Doe, 2020).

Similar to the US, litigants in Israel have the option to file a motion requesting the court to keep their names concealed. Although the granting of such motions is at the judge's discretion, they are typically allowed only in special circumstances, adhering to the principle of public legal proceedings. Prior to the reform, anonymity was primarily granted in personal injury cases involving minors.

The legal reform In 2013, a decision by Justice Zylbertal suggested the potential value of permitting litigants to sue anonymously under specific circumstances (Eliyahu Insurance Co. v. Plonit, 2013). This decision prompted the legislature to introduce a bill aimed at providing anonymity to all plaintiffs filing a personal injury claim. The legislative process

[6] See, for example, Doe v. Village of Deerfield (2016) "Anonymous litigation runs contrary to the rights of the public to have open judicial proceedings and to know who is using court facilities and procedures funded by public taxes"; Gibson v. Pfizer, Inc. (2020) "The Court is a public institution and the public has a right to look over our shoulders and see who is seeking relief in public court.".

was expedited. Introduced in parliament by an opposition member on February 19, 2014, the bill received broad cross-party support and was passed unanimously – with no votes against – on July 28, 2014. According to the new law, the names of *all* plaintiffs filing a personal injury claim after August 2015 were to be kept confidential. Claims filed before that date continued under the old regime, where anonymity was subject to the judge's discretion. The rule for claims concerning property damage remained unchanged.

Panel (a) of Figure 1 presents the change in anonymity rates for closed cases regarding both personal injury and property damage incidents.[7] While prior to the legislation, only 10% to 20% of personal injury cases with court rulings were granted anonymity, beginning in 2016 (once the reform was implemented) this rate jumped to over 40% and reached almost 100% by the end of the sample period. The reason the anonymity rate grew over time for personal injury cases after the reform is the gradual expansion of the reform's coverage.[8] Throughout the entire period of our data, anonymity rates for property damage remained constant at below 5%.

2.2. Data

We collaborated with the Israeli Capital Market, Insurance, and Savings Authority to gain access to data on all disputed car accident claims submitted to any of the five major insurance providers in Israel.[9] Each of the five firms provided a database with a separate distinct record of all disputed claims that closed between January 1, 2011 and December 31, 2020.[10] A disputed claim is defined as one where the insured did not accept the initial payment provided by the insurance company. Whether or not a case becomes disputed is primarily driven by the complexity of the case. Personal injury cases are often disputed by insurance providers because they tend to rely on medical diagnosis that can vary from specialist to specialist as well as a loss in future earnings that can be difficult to calculate. The property

[7] To measure the rate of anonymous court rulings between the years 2011 and 2020, we utilized extensive data from Nevo (see, e.g., Anidjar, Katz and Zamir (2020)). Nevo is the Israeli equivalent of Westlaw & Lexis – a database collecting court decisions over time (Nevo, 2011–2020).

[8] The reform stipulated that only claims filed after 2015 were required to be anonymized. Accordingly, court rulings in personal injury cases occurring after the reform that were filed prior to the reform could still expose the plaintiff's name. As time progressed, the reform's coverage expanded, leading to a higher rate of claims falling under its purview. Consequently, more personal injury disputes became subject to the rule of anonymity.

[9] This authority audits the business activities of all insurance providers who are active in Israel.

[10] A case is defined as closed after a payment is made from the insurance company to the claimant. The size of the payment can be determined by a settlement or a court hearing.

damage cases appearing in our data are primarily cases where the insured chose to use their own appraiser to assess the damage resulting from the incident as opposed to an appraiser who is recognized by the insurance company. Another common factor driving disputes in property damage cases are disagreements on the size of the deductible owed.

Our database includes information on type of claim (personal injury or property damage), date of incident, whether the claim was litigated in court, whether the claimant was represented by a lawyer (and if so, the lawyer's name), date of settlement or judgment, amount of settlement or judgment, and claimant's address, age, and gender.[11] We define a claim as "post" legislation if the date of settlement or judgment occurred after July 2014, when the legislative reform was passed in Parliament. The reason we focus on the date of legislation is that the claimants bargaining power may have changed as soon as it became public knowledge that the claimant could take the insurance company to court without facing any risk of privacy loss once the reform in enacted.[12]

We collect additional data on lawyer prestige from the Dun's 100 ranking of the top 100 Israeli law firms between 2013 and 2023 (Duns100, 2013-2023). Similar to many other countries, Israeli law restricts lawyer fees in personal injury claims to a fixed percentage (13%) of the claimant's payment. As noted in the literature, this payment structure creates an equilibrium where claimants with severe injuries are typically represented by top personal injury lawyers (see, e.g., Zamir, Medina and Segal (2014)). Thus, lawyer prestige can serve as a proxy for case severity. Lastly, we accessed data from the Israeli Central Bureau of Statistics on demographic characteristics of the population by statistical areas (CBS, 2008). [13] We matched addresses to statistical areas and pulled in information on income, ethnicity, education, and degree of religiosity in the area where the individual resides.

We summarize the characteristics of the claims included in our database in Table 1.[14] Generally, the payments from personal injury cases are more than double that of property damage cases. The primary reason for this is that payments from personal injury cases cover the medical expenses from physical injuries, lost earnings, and pain and suffering. Thus, personal

[11] All NIS and dollar figures are reported in real 2023 terms. The average exchange rate during our sample period is 3.69 NIS to 1 USD.

[12] An alternative is to define the "post" period as after the reform came into effect (August 2015), or to remove cases that closed after the legislation came into effect but had been initiated prior to its enactment. In section 4.3 we show that our results are robust to alternative definitions of the cutoff date of the reform.

[13] These statistical areas were defined by the Central Bureau of Statistics to create areas that are relatively homogeneous in terms of the characteristics of the population residing within them. Israel is divided into 1,624 statistical areas with roughly 3,000–5,000 residents in each.

[14] See Appendix Table A1 for a list of variables in our database and their definitions.

injury payments are a direct function of earnings and age. These higher, more complicated to calculate payments could also explain the high rate of lawyer representation in personal injury cases (over 95%) versus property damage cases (roughly 50%). On average, personal injury cases close roughly 2.5 years after they are opened, while property damage cases close closer to 1.5 years after they are opened.

Table 1 illustrates a significant increase in payments made by insurance companies for personal injury claims after the legislative change was introduced in parliament. In the first column, we observe that in the pre-period the average payment from a personal injury claim was 33,000 NIS (roughly $8,950). This payment increased by almost 40% in the post-reform period and grew to 45,300 NIS (roughly $12,286). During this same period, payments from property damage increased by only 8% or 1,200 NIS ($300). In order for these numbers to provide some indication of the implications of the reform we must ensure that prior to the legislation, the gap in payments between personal injury cases and property damage cases remained relatively constant. We explore this in the next section.

3. Empirical Strategy

Our analysis focuses on how the protection of litigant privacy impacted the payment that litigants received in personal injury cases following the announcement of the reform (July 2014). Thus, we model case outcome $log(payment_{ict})$ in a car accident involving individual i who submitted a claim to insurance provider c, that was closed on date t using the following regression model:

$$log(payment_{ict}) = X_{ict}\alpha + \beta_1 injury_{ict} \times post_t + \beta_2 injury_{ict} + \mu_c + \mu_t + \varepsilon_{ict} \qquad (1)$$

The vector X_{ict} includes individual characteristics of the claimant that may be correlated with payment outcomes such as gender, age, and age squared. X_{ict} also includes characteristics at the statistical area level as defined by the Central Bureau of Statistics such as income, ethnicity, education, and degree of religiosity. The coefficient on the interaction term β_1 estimates the change in payments that was driven by the legislative privacy reform as long as we are able to control for general trends in insurance payments occurring over time. This DID strategy provides an unbiased estimate of the effect of the reform as long as property damage claims provide a valid control group for personal injury claims during this period.

Panel (b) of Figure 1 illustrates the rise in payments for personal injury claims (marked by

the solid line) following the announcement of the reform. This increase closely mirrors the trend observed in anonymity (see Panel (a)), albeit with an earlier response. Specifically, as anonymity was expected to become more common due to the announced change in legislation, payments in personal injury claims surged. Importantly, the payment trend observed for property damage cases closely resembles that of personal injury cases pre-reform and remains relatively stable post-reform.

To account for the fact that the reform did not result in immediate anonymity for all cases but instead took effect gradually – as more cases fell under the jurisdiction of the reform – we also estimate an event study specification:

$$log(payment_{ict}) = \sum_{\substack{t=2011 \\ t\neq 2014}}^{2020} \alpha_t \, \mathrm{D}_t \times \mathrm{injury}_{ict} + \sum_{\substack{t=2011 \\ t\neq 2014}}^{2020} \rho_t \, \mathrm{D}_t + \beta \mathrm{injury}_{ict} + \Omega' X_{ict} + \epsilon_{ict} \qquad (2)$$

We control for changes in payments over time by including a set of time fixed effects (D_t), specifically including dummy variables for each 6-month period between January 2011 and December 2020. The first half of 2014 – the period immediately preceding the announcement of the reform – serves as the baseline period and is omitted from the regression. The estimated coefficients $\hat{\alpha}_t$ allow us to separately measure the effect of the reform as anonymity rates evolved over time. This specification also provides a direct test of the plausibility of the parallel trends assumption, which is critical for the causal interpretation of our difference-in-differences analysis. If the coefficients for the pre-reform periods are close to zero and statistically insignificant, this supports the assumption that, in the absence of the reform, trends in payments would have evolved similarly across property damage and personal injury claims.

Sample Selection Our database includes only disputed claims, raising concerns of sample selection if prior to the legislative change insurers disputed more personal injury claims on the premise that claimants would likely accept reduced payments out of fear of privacy breaches. Appendix Figure A1 shows the yearly density of disputed cases (Panel (a)) and the difference between property damage and personal injury cases (Panel (b)). We find no significant decrease in the relative fraction of disputed personal injury cases.

Another concern could be that our database is constructed based on case closure dates rather than submission dates. For each year, we have data on all disputed cases closed by Israel's

five major insurers. If we focused on case submission dates, we would see a mechanical decline in payments, particularly in personal injury cases, as higher payment cases generally take longer to close and wouldn't appear in later years. To avoid this issue, we analyze cases by their closing dates.

Lastly, the reform may have impacted case duration (from submission to payment) if, before the reform, insurers took longer to settle claims by offering low initial payments due to unclear privacy valuations. Specifically, if insurance providers were applying a trial-and-error technique, starting with very low offers that prior to the reform incorporated a noisy estimate for the claimants value of privacy, this could increase case lengths in the pre-period. Because our database is structured based on closed cases, this could raise the concern that we would observe more cases in the post-period than in the pre-period. However, we observe no change in the annual closure rate of personal injury cases (Appendix Figure A1) or in the duration of personal injury cases that average roughly 2.5 years pre- and post-reform (see Table 1).

Strategic Withholding of Claims The legislative reform was passed in July 2014 and enacted in August 2015, mandating anonymization of personal injury cases filed after 2015. While one might argue that only cases filed after 2015 should be included, claimants may have delayed filing to benefit from the new rules. It is also possible that the announcement itself resulted in a shift in power as the threat of future privacy loss to the insured decreased significantly. Therefore, we define the post-reform period as starting in July 2014.[15]

SUTVA A car accident can involve both a property damage claim and a personal injury claim. While these claims arise from separate insurance policies (see discussion of Institutional Background in Section 2.1) with disconnected court proceedings one could be concerned that the decision to pursue a larger payment from the insurance provider on a personal injury claim as a result of better privacy protection may also impact behavior regarding a property damage claim. In this case SUTVA would be violated as payment on property damage claims may also respond to the reform. To ensure this is not biasing our results, we re-run our analysis when dropping all observations where a property damage claim and a personal injury claim were both filed for the same incident, and find very similar results (see Table 4).

Other Changes We also consider whether other changes during this period could bias

[15] We also consider different alternatives for defining the pre- and post-reform periods in Section 4 and obtain very similar results.

our estimates from equation (1). One concern is the capitalization rate set by the courts, the rate at which future lost earnings are discounted to a current lump sum. This rate plays a crucial role in determining payments for personal injury cases. While Israeli courts historically maintained a capitalization rate of 3%, the low-interest rates of the early 2010s prompted some courts to reconsider. Notably, between 2016 and 2019, some trial courts ruled that the capitalization rate in personal injury cases should be adjusted to 2%, thereby raising victims' compensations for personal injuries.which discounts future lost earnings to a current lump sum.[16] Since the capitalization rate has limited impact on property damage compensation, there is concern that this could bias our estimates upwards. To address this, in robustness tests we constrain the sample to periods with a constant capitalization rate and find no evidence that changes in this rate affect our results.

Another potential concern is that an increase in wages in the post-reform period could mechanically drive up personal injury payments (which partially reflect lost wages), independent of the reform. Indeed, there was an increase in real wages in Israel that coincided with the timing of the reform, so that wages were roughly 11% higher in the post-reform period (see Finance-Ministry (2019)). To address this, in robustness tests, we re-weight personal injury payments to account for changes in the wage distribution over time.[17] The reweighted results show that the estimated effect of the reform decreases by 4 percentage points to a 12.5 percent increase in payments after the reform and remains significant at the 1 percent level.

4. Results

In this section we first discuss the payment trends observed over the period of our analysis and the extent to which the parallel trends assumption holds. We then estimate equation (1) both for the full data and different subgroups that we might expect to be differentially affected by the reform. We also run a series of robustness tests to ensure that our results measure the effect of the legislative change regarding anonymity.

[16] The first decision was made in August 2016 by the Tel Aviv district court and was followed by courts in three other districts between 2016 and 2019. For a detailed account of these decisions, as well as opposing rulings by other trial courts, see the Supreme Court decision in Ha-pool v. Amutat Ofek Chazara L'Chaim (2019).

[17] We apply findings from a recent study examining personal injury payments in court rulings on car accidents in Israel showing that on average 47% of personal injury payments are driven by lost wages (see Biton (2023)).

4.1. Baseline Results

Figure 2 shows the event study results (equation (2)), tracking changes in the payment gap between personal injury and property damage cases over time, relative to early 2014 (immediately prior to the announcement of the legislative change). The gap remained stable from 2011 to 2014 but increased by 13% about six months after the reform was announced, continuing to grow until late 2020, when it reached a 35% increase compared to 2014. By this period, anonymity rates in court rulings for personal injury claims had risen to nearly 100%, up from 20% in 2014 (see Panel (a) of Figure 1).

In Table 2, we summarize our results from a difference-in-differences analysis as outlined in equation (1). In specification (1), we report the results of the difference-in-differences analysis without introducing additional controls. In specification (2), we measure a slightly larger response to the legislative privacy reform when including additional controls for claimants' age and gender, and the specific insurance firm making the payment. We find that generally payments are 16% higher for men than women and payments from claims increase with age. The likely explanation for this is that in personal injury cases wages play a significant role in determining payment from insurance companies and wages are higher among men and more experienced workers. The coefficient on $injury_{ict} \times post_t$ when including location fixed effects as well as claim characteristics in specification (3), suggests that after the reform the increase in payments made by insurance companies for personal injury claims was roughly 17% higher than the increase in payments observed for property damage claims. Prior to the reform, the average payment from a personal injury case was 33,000 NIS; thus a 17% increase translates to roughly 5,610 NIS or 1,520 USD. Our analysis suggests that protecting plaintiffs' privacy led to a significant increase in the payments made by insurance companies for personal injury claims.

4.2. Heterogeneity in Response to the Privacy Reform

We might expect the value of private information to differ across individuals, as the sensitivity of information disclosure could hinge on specific characteristics of the claim or accident. Additionally, differences may arise among claimants based on factors such as gender, education, or religion. To examine whether the reform had heterogeneous effects, we estimate equation (1) separately for different claimant groups (defined by age, gender, income, etc.). In addition, we report the coefficient and standard error on the triple interaction

term $group_i \times injury_{ict} \times post_t$ from a pooled regression including both groups. This allows us to formally test whether the reform had a significantly different impact across claim characteristics.[18]

In Panel A of Table 3, we first compare the response of retired (over 60) versus working age (ages 21-60) claimants to the privacy reform.[19] The level of privacy exposure from a court decision was much higher for younger workers. In order to determine the payment received by the litigant, the litigant must reveal both the full extent of his/her injuries and the perceived loss of future earnings due to the injury. For younger workers publicizing the extent of an injury can have implications for future opportunities in terms of their career and personal life. Additionally, previous paychecks must be submitted for the courts to calculate perceived loss of future earnings for working-age individuals. Both concerns are largely irrelevant for retirees. Retirees, for obvious reasons, have little concern about future career prospects or promotions and are therefore less sensitive to injury disclosure. Additionally, since retirees' income typically comes from pensions, personal injuries do not affect their earnings. As a result, filing a claim does not require them to reveal private salary information. The reform increased insurance payments by 21% (s.e. 2.4) for working-age individuals, but had no significant effect on older workers (over age 60), with an estimated increase of 3.4% (s.e. 5.2).

Another factor that is likely to impact the value of privacy protection is the injury severity. If the physical injury did not impact an individual's ability to work then the litigant would not need to provide information on his/her earnings to the court. Thus, we might expect that in these cases involving less severe injuries loss of privacy is less of a concern both in terms of employment disclosure and injury disclosure. While, we do not have data on the severity of the injury, we are able to identify the name of the lawyer pursuing the case. If more prominent lawyers are more likely to argue more severe cases, lawyer expertise can serve as a proxy for severity. As noted, lawyer fees in personal injury claims stemming from car accidents are limited to 13% of the payment received by the claimant. As pointed out by the literature, this payment structure leads to an equilibrium where claimants with severe injuries are usually represented by top personal injury lawyers (see, e.g., Zamir, Medina and Segal (2014)). We therefore use the Dun's data on the top 100 law firms in Israel to

[18] In our main analysis (Table 3) we include only observations where the panel characteristic is observed. We find similar results when running the analysis on the full sample and controlling for missing information (see Appendix Table A2).

[19] We were only able to receive data on age of litigant for roughly 45 percent of our sample of cases. Claimants under age 21 are excluded from the analysis as most Israelis complete 2-3 years of mandatory military service after high school, which postpones higher education and entry into the labor force.

determine the expertise of the lawyer arguing the case in an effort to differentiate between cases where the loss of privacy after a court decision may be more detrimental.

Panel B of Table 3 illustrates the growth in payments after the legislative privacy reform in cases represented by lawyers in top-ranked firms versus other firms.[20] In the first specification we observe that payments increased by roughly 26% in personal injury cases argued by law firms below the top 100 ranking after the change in privacy legislation. The effect was significantly larger for cases argued by lawyers in the 100 top-ranked firms (a 37% increase in payments for personal injury cases). This could suggest that the monetary value of privacy is highest specifically in cases that would have faced the largest privacy loss absent the reform.

An alternative explanation for the results involving top law firms is that both types of claimants valued privacy similarly, but more experienced lawyers were more aware of the reform and its impact on their bargaining power. In Appendix Table A3, we observe that the largest payment gap between top lawyers and other lawyers occurred directly after the reform was implemented. In 2016, personal injury cases handled by top lawyers saw a 48% increase in payments, compared to 17% for other lawyers. Although this gap narrows over time, payments in cases argued by top law firms remain 20 percentage points higher at the end of our period, suggesting that differences in the expected cost of privacy loss contributed to the large effects observed in cases argued by more prominent law firms.

In Panel C of Table 3 , we provide heterogeneity results across cases that remained open for different durations of time. If more severe injury cases responded more to the privacy legislation than other cases, we might expect to find a larger impact on cases that took longer to be resolved. We find that cases taking over 18 months to close saw a 27% increase in payments after the reform. By contrast, cases that closed more quickly showed no significant change in payments.

Previous research has investigated the potential correlation between gender and income with respect to privacy valuations.[21] In Panels D–G of Table 3, we run our analysis when

[20] These specifications exclude cases without lawyer representation from the analysis.

[21] Blank, Bolsover and Dubois (2014) provide a review of research relating demographic characteristics to privacy valuations. Much of this work has focused on surveys, e.g., Johnson, Shriver and Du (2020), Park (2015), O'Neil (2001), Sheehan (2002), and Sheehan (1999). Additionally, Lewis, Kaufman and Christakis (2008) document that women are more likely than men to keep their profile's private on Facebook.

splitting the sample by gender, income, and religion.[22] We find that men and women benefit similarly from the privacy protection added after the reform. When splitting the data by income levels we see a slightly larger difference where individuals residing in areas with above median income gain a 16% increase in payments from personal injury cases after the reform, while those below median income gain a 21% increase. Although this could suggest that less affluent people put a higher valuation on privacy (i.e., accepted lower settlements prior to the reform than those in more affluent areas), the difference is not statistically significant.[23] These findings suggest that privacy is an interest that transcends social sectors.

Panels F and G of Table 3 examine heterogeneity across religious groups.[24] Indeed, the difference we observe in privacy payments between high- and low-income neighborhoods mimics the results we observe when comparing Jewish ultra-Orthodox neighborhoods to other Jewish neighborhoods. While the difference is not statistically significant, we find a larger impact in ultra-Orthodox neighborhoods than in other Jewish neighborhoods. The most notable difference was between Jewish and non-Jewish neighborhoods, with individuals in non-Jewish neighborhoods seeing a 26% payment increase after the reform – 10 percentage points higher than those in Jewish neighborhoods. Though not statistically significant, these findings may be linked to the significance placed on privacy, especially regarding health related impairments within traditional communities (see, e.g., Rosen et al. (2008), Ciftci, Jones and Corrigan (2013)).

These findings contrast with prior research suggesting that women, and higher-income individuals are more concerned about privacy. A distinguishing feature of our setting is the role of intermediaries – specifically, lawyers who advice their clients. These lawyers may attenuate the effect of personal characteristics on attitudes regarding privacy protection. While attributes such as gender, income, and religion may affect individuals' sensitivity to the exposure of private information, their impact may diminish when decisions regarding privacy are made following the advice of professional intermediaries (i.e., lawyers).

[22] These specifications are limited to subsamples for which the relevant data is recorded. Gender is observed for 45% of the full sample (roughly 240,000 observations). Information on earnings and religion is available for a subsample of approximately 100,000 observations for which we were able to obtain address-level data.

[23] This finding is robust to alternative definitions of high- and low-income localities (e.g. the 25th percentile or 10th percentiles of income). We consistently find that lower income localities seem to respond more to increased privacy protection, but the difference is not statistically significant.

[24] When exploring heterogeneity between individuals residing in ultra-Orthodox neighborhoods and those in other Jewish neighborhoods, we exclude individuals who are missing address level data and claimants residing in Arab neighborhoods from the analysis.

4.3. Robustness Tests

In Table 4, we provide additional specifications to ensure the robustness of our main results. In Panel A of Table 4 we examine whether outliers in the sample of the treatment versus control groups can be driving our results. In the first specification, we constrain our sample to only include incidents submitted by individuals over age 21, as these younger drivers are almost nonexistent in property damage cases (see Appendix Figure A2). Excluding younger individuals from our sample results in a slightly higher estimate of the effect of the reform (19% (s.e. 0.9)). In the second specification, we exclude all claimants from the data who submitted more than 5 claims over the duration of 2011–2020. Because the property damage data is submitted by the insured, these claims can also be submitted by leasing firms or companies that may have different norms or capabilities in terms of insurance payments. When excluding these cases, we continue to find that the reform resulted in an 18% increase in payments for personal injury cases.

The third specification of Panel A of Table 4 focuses on payment size. To address concerns about the control group's validity at the extremes – since personal injury payments are generally higher than property damage payments – we exclude the top and bottom 5% of cases. Although we continue to find a significant effect at the 1% level, it is smaller in magnitude (a 12% increase in payments following the reform). The observed decrease stems from the exclusion of large payments (exceeding 114,288 NIS) and is consistent with our previous findings that the impact of the reform is most pronounced in high-cost cases, which are likely to involve heightened privacy concerns.

The final specification of Panel A addresses the concern that some property damage claims may have been indirectly affected by the privacy reform (see the discussion of SUTVA in Section 3). Although property damage claims and personal injury claims are covered by separate insurance policies and processed independently – even when they arise from the same car accident – we cannot fully rule out the possibility of spillover effects. For example, individuals who feel more empowered to dispute a personal injury claim due to increased privacy protection after the reform may also negotiate more aggressively in their property damage claims. This could lead to higher payments for those claims, violating SUTVA and potentially biasing our estimates downward. To address this concern, we exclude all cases involving both a property damage and personal injury claim and estimate that the reform increased payments for personal injury cases by 24%.

In Panel B of Table 4, we examine whether the time periods defined as pre- and post-reform

are driving our results. In our main specification, any cases that were closed after July 2014, when the legislative reform was announced, are defined as post-reform. This definition is based on the assumption that once both the claimants and the insurance companies were aware that privacy loss would cease to be a risk for claimants in the future, this would immediately impact the negotiation process. In the first specification of of Panel B of Table 4, we run our analysis with the alternative cutoff point of August 2015 once privacy was mandated for all personal injury cases. We find a slightly smaller effect of the reform, 14% (s.e. 0.7) compared to the 17% increase in payments that we report in Table 2. An alternative way to address this period of ambiguity when defining pre versus post reform, is to drop cases that were opened pre reform but closed afterwards. When excluding these cases in the second specification of Panel B, we estimate that the privacy reform resulted in a 24% increase (s.e. 0.8) in payments for personal injury cases.

In the third specification of Panel B of Table 4 we run our analysis when dropping periods where trial courts made rulings that lowered the capitalization rates (see discussion on capitalization rates in Section 3). The first ruling of this type occurred in a local Tel Aviv court in May 2016, where the court decreased the capitalization rate to 2%. Thus, we run our analysis when constraining the post period to end before May 2016. We continue to find a significant 12% (s.e. 1.1) increase in payments for personal injury cases as a result of the reform. The ambiguity regarding the capitalization rate was resolved in August 2019 when the Supreme Court determined it should remain at 3%. Our dataset extends to December 2020, fourteen months after the Court's ruling. In the fourth specification, we run our analysis when dropping all cases that were ruled on during this intermediate capitalization period (May 2016 through August 2019). We estimate an even larger effect of the privacy reform on payments for personal injury cases of 18% (s.e. 1).

Panel C of Table 4 addresses the concern that rising wages in the post-reform period may increase personal injury payments (which partially reflect lost earnings), while leaving property damage payments unaffected (see discussion in Section 3). To account for this, in the first specification we reweight personal injury payments using average real wages in each year, assuming that lost wages account for 47% of total personal injury compensation – based on estimates from Biton (2023). In the second specification, we use real wage growth in the private sector, which experienced higher growth in this period, again assuming a 47% wage share. In both cases, the estimated effect of the reform decreases to roughly 12% (s.e. 0.8), remaining significant at the 1 percent level. To further test the sensitivity of our results, the third and fourth specifications reweight personal injury payments using average real wages

and vary the assumed wage share to 40% and 60%, respectively. These adjustments alter our estimated effect of the reform by one percentage point in either direction, suggesting that a change in wages on its own cannot explain the significant increase in personal injury payments observed during the post-reform period.

5. Conclusion

The privacy paradox has long been a central theme in the literature, fostering uncertainty about the true value of privacy. Analyzing the impact of a large-scale legislative change in privacy protection provided a unique opportunity to investigate the importance of privacy in a high-stakes real-world context.

Our findings indicate that the elimination of privacy risks related to claimants' health status and income led to a 17% increase in payments from insurance companies for personal injury cases. Given an average pre-reform payment of 33,000 NIS, this implies that claimants valued keeping their injury and earnings information private at at-least 5,610 NIS (approximately $1,520) – equivalent to about 60% of the average monthly salary in Israel during this period. This is a substantial implied valuation, especially when compared to prior work. For example, individuals in lab or survey contexts have relinquished private information for minimal incentives: a slice of pizza in exchange for friends' email addresses (Athey, Catalini and Tucker, 2017), more personalized shopping (Spiekermann, Grossklags and Berendt, 2001), or a modest price discount (Beresford, Kübler and Preibusch, 2012). Among existing field studies, only Tang (2019) finds comparably high valuations, with Chinese loan applicants willing to forgo $33 – roughly 70% of daily income – to protect information about their employer and social network.

The significant amount of money that claimants are willing to forgo to protect their privacy is consistent across different population segments. Both men and women, along with individuals residing in both more and less affluent neighborhoods, were similarly affected by the reform. We have also identified that the primary mechanism driving the increase in payment was insurance companies offering higher settlement amounts.

The high valuation of privacy that we measure is likely a result of the context we study. Publicized information on physical injuries and income may carry long-term reputational or financial consequences, such as influencing access to future employment, insurance, or credit. This likely intensifies individuals' aversion to exposure and increases their willingness to pay

for privacy. In lower-stakes contexts, where disclosure has fewer or less tangible consequences, or high-stakes contexts where the consequences are harder to grasp, privacy valuations may be far lower. Additionally, the presence of a third-party, lawyers in this context, may both make individuals more aware of the cost of privacy disclosure and decrease heterogeneity across claimant groups.

Our results suggest that the privacy reform increased the costs faced by insurance providers, who could no longer use the threat of public exposure to reduce settlement amounts. In a recent study conducted on the car insurance market in Israel, Weiss and Belfer analyze trends in Israel's car insurance market between 2013 and 2022 (Weiss and Belfer, 2024). They document that underwriting revenues from the mandatory insurance (which covers personal injury claims) rose between 2013 and 2015, but fell sharply in 2016 – the first full year after the privacy reform came into effect. Their study also shows that while premiums for the mandatory insurance had been declining between 2013 and 2017, in 2018 these premiums stabilized. These patterns are consistent with a shift in insurance pricing strategies in response to increased claimant bargaining power. Although we do not directly estimate pass-through effects, the timing and direction of these industry trends supports the conclusion that the privacy reform had economic consequences for both consumers and insurers.

Existing studies on privacy protection primarily focus on how firms exploit information leakage to their advantage, often through practices of price discrimination. This study sheds light on a distinct context where information privacy carries significant economic implications. Insurance companies leverage individuals' concerns regarding potential personal information leaks. Contrary to adjusting policy premiums based on this information, insurers capitalize on claimants' worries that exposure might harm future relationships with third parties. This dynamic enables insurers to offer lower settlement amounts, demonstrating the significant power that firms with access to private information hold over their consumers. Our findings suggest that privacy protection could yield more extensive benefits than conventionally assumed.

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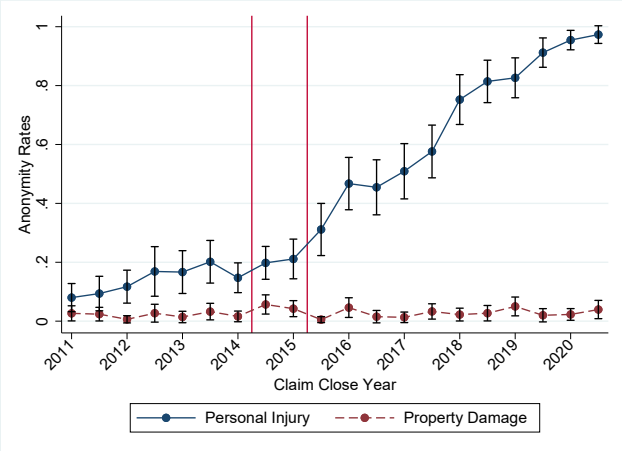
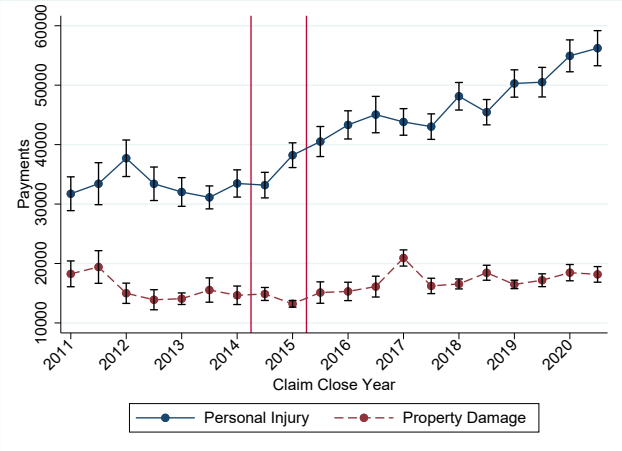


(a) Court Decision - Anonymity Rate



(b) Payment Trend

Figure 1: Changes in Anonymity Rates & Payments in Personal Injury Cases

Notes: Panel (a) maps the fraction of anonymous court cases out of total court rulings for personal injury and property damage cases over time. Panel (b) maps average payments for all claims submitted to the top 5 insurance companies in Israel that were resolved by a settlement or court judgment. The two vertical lines delineate the announcement of the legal reform (July 2014) and its implementation (August 2015).

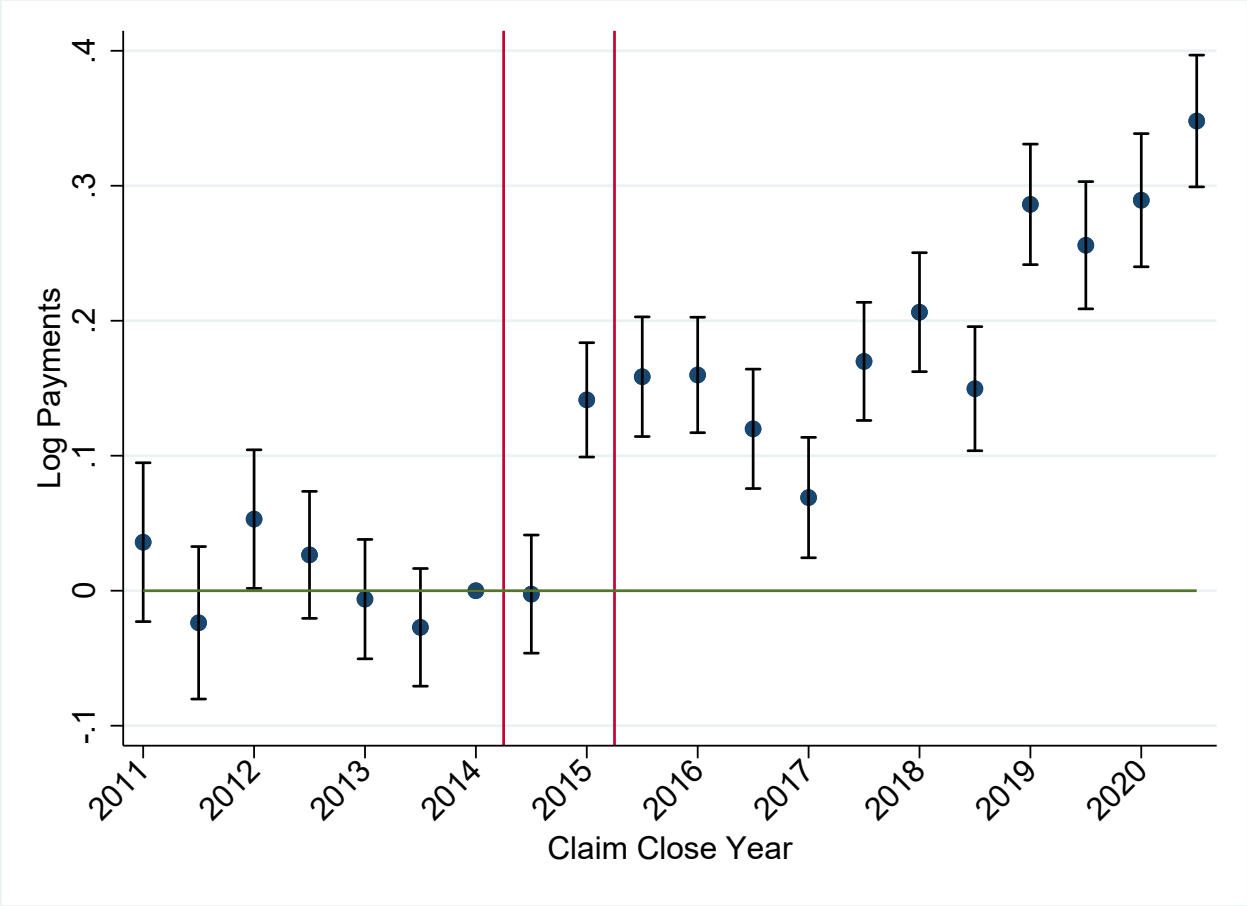


Figure 2: The Effect of the Privacy Legislation on Payments

Notes: This event study figure maps the change in the log payment gap between personal injury and property damage cases relative to the gap observed in the first 6 months of 2014 (prior to the announcement of the reform). The figure displays the coefficients on the interaction terms Personal Injury x 6-Month-Period and their 95% confidence interval for all claims submitted between January 2011 and December 2020. An indicator for the first 6 months of 2014 is excluded from the regression and thus, serves as a baseline. The two vertical lines delineate the announcement of the legal reform (July 2014) and its implementation (August 2015).

Table 1: **Summary Statistics**

	Personal Injury			Property Damage		
	Pre	Post	Diff.	Pre	Post	Diff.
Payment (1,000s of NIS)	33.0	45.3	-12.3	15.4	16.5	-1.2
	(152.8)	(165.0)	[0.6]	(63.0)	(49.0)	[0.4]
Represented by Lawyer (%)	95.7	95.1	0.6	40.7	51.1	-10.4
	(20.4)	(21.6)	[0.1]	(49.1)	(50.0)	[0.3]
Top Law Firm (%)	3.5	3.5	0.0	3.4	3.2	0.2
	(18.5)	(18.4)	[0.1]	(18.1)	(17.5)	[0.1]
Case Duration (Years)	2.5	2.5	0.0	1.4	1.6	-0.2
	(2.4)	(2.2)	[0.0]	(1.3)	(1.3)	[0.0]
Claim Litigated in Court (%)	20.9	25.2	-4.2	72.0	74.9	-2.9
	(40.7)	(43.4)	[0.2]	(44.9)	(43.3)	[0.3]
Settlement (%)	97.6	98.7	-1.1	61.2	49.4	11.8
	(15.4)	(11.5)	[0.0]	(48.7)	(50.0)	[0.3]
Claimant Age	33.8	34.6	-0.8	44.0	46.2	-2.2
	(18.3)	(19.0)	[0.1]	(14.8)	(14.8)	[0.3]
Male (%)	55.9	50.8	5.2	74.1	75.7	-1.6
	(49.6)	(50.0)	[0.3]	(43.8)	(42.9)	[0.7]
Avg. Income Locality (1,000s of NIS)	71.5	70.4	1.1	74.3	74.6	-0.3
	(24.1)	(23.1)	[0.2]	(25.9)	(25.7)	[0.6]
Socioeconomic Rank of Locality	832.4	807.3	25.1	923.1	926.0	-3.0
	(421.2)	(418.5)	[3.3]	(431.8)	(436.6)	[10.4]
Jewish Locality (%)	81.7	77.8	3.8	89.4	91.5	-2.0
	(38.7)	(41.6)	[0.3]	(30.8)	(28.0)	[0.6]
Ultra-Orthodox Locality (%)	10.9	11.9	-1.0	7.7	9.2	-1.5
	(31.1)	(32.3)	[0.3]	(26.7)	(28.9)	[0.6]
College Educated at Locality (%)	39.0	37.6	1.4	40.2	39.8	0.4
	(16.0)	(16.0)	[0.1]	(17.1)	(16.5)	[0.4]
Observations	91,865	232,823	324,688	32,697	70,709	103,406

Notes: The table displays the average characteristics of personal injury cases and property damage cases both before and after the legislative reform, along with their standard deviation. The Diff column reports the difference in means between the pre- and post- periods and the standard error of this difference.

Table 2: **The Effect of Privacy Legislation on Payments**

	(1)	(2)	(3)
Personal Injury X Post	0.132***	0.160***	0.166***
	(0.008)	(0.008)	(0.008)
Personal Injury	0.335***	0.411***	0.403***
	(0.007)	(0.008)	(0.008)
Male		0.157***	0.153***
		(0.006)	(0.006)
Claimant Age		0.027***	0.027***
		(0.000)	(0.000)
Claimant Age Squared		-0.000***	-0.000***
		(0.000)	(0.000)
Socioeconomic Rank of Locality		0.000	
		(0.000)	
Jewish Locality		0.002***	
		(0.000)	
Ultra-Orthodox Locality		0.068***	
		(0.017)	
Higher Education Attainment at Locality		-0.002***	
		(0.000)	
Year FE	Yes	Yes	Yes
Firm FE	No	Yes	Yes
Location FE	No	No	Yes
Mean Payments	35641.981	35641.981	35641.981
Observations	428094	428094	428094
Adjusted R-squared	0.044	0.092	0.098

Notes: The dependent variable is log(payment). Column (1) contains a personal injury case indicator, year fixed effects, and the interaction between the personal injury case indicator and post-reform announcement (July 2014). Column (2) adds controls for claimant gender, age, and age squared, as well as the insurance company where the claim was submitted, and characteristics of the locality where the claimant resides such as socioeconomic rank, Jewish/Arab, ultra-Orthodox, and average education attainment. Column (3) adds location fixed effects. Robust standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: **The Effect of Privacy Legislation on Payouts - Heterogeneity Results**

	No	Yes	Difference
Panel A: Working Age			
Personal Injury X Post	0.034	0.205***	0.204***
	(0.052)	(0.024)	(0.056)
Observations	27,353	162,833	190,186
Panel B: Represented by Top Lawyer			
Personal Injury X Post	0.261***	0.371***	0.095**
	(0.012)	(0.049)	(0.047)
Observations	340,182	14,699	354,881
Panel C: Long Case Duration			
Personal Injury X Post	-0.009	0.270***	0.264***
	(0.009)	(0.015)	(0.018)
Observations	214,309	213,785	428,094
Panel D: Male			
Personal Injury X Post	0.214***	0.226***	0.010
	(0.037)	(0.023)	(0.043)
Observations	87,046	103,464	190,510
Panel E: High Income			
Personal Injury X Post	0.210***	0.163***	-0.035
	(0.037)	(0.034)	(0.050)
Observations	47,338	47,050	94,388
Panel F: Jewish			
Personal Injury X Post	0.259***	0.158***	-0.092
	(0.078)	(0.026)	(0.083)
Observations	18,766	75,622	94,388
Panel G: Ultra-Orthodox			
Personal Injury X Post	0.156***	0.207**	0.042
	(0.027)	(0.090)	(0.093)
Observations	67,136	8,486	75,622

Notes: The dependent variable in all specifications is log(payment). All regressions include a personal injury case indicator, and its interaction with a post reform indicator (July 2014), as well as controls for claimant gender, age, age squared, and year, insurer, and location fixed effects. Each panel uses a subsample based on a specific characteristic and includes only observations where that characteristic is observed. The first specification reports the coefficient on the interaction term between the personal injury case indicator and post for cases where the panel characteristic is true. The second specification reports this same coefficient for cases where the panel characteristic is false. The third specification reports the coefficient on the triple difference interaction term (panel characteristics x personal injury case indicator x post). Robust standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: **Robustness Tests**

Panel A: Alternative Samples				
	Age>21	Excluding Claims>5	2120<Payments<114,288	Excluding Dual Claims
Personal Injury X Post	0.187***	0.179***	0.121***	0.244***
	(0.009)	(0.009)	(0.007)	(0.009)
Observations	368,814	424,215	385,281	314,684
Panel B: Alternative Periods				
	2015 Cutoff	Excluding 2014-2015	Period = 2011-2016	Excluding 2016-2019
Personal Injury X Post	0.144***	0.243***	0.115***	0.177***
	(0.007)	(0.008)	(0.011)	(0.010)
Observations	428,094	312,210	215,017	271,053
Panel C: Reweighting Payments				
	Average Wage	Private Sector	Wages (40% of Payments)	Wages (60% of Payments)
Personal Injury X Post	0.125***	0.120***	0.132***	0.113***
	(0.008)	(0.008)	(0.008)	(0.008)
Observations	428,094	428,094	428,094	428,094

Notes: The dependent variable in all specifications is log(payment). The right-hand side variables contain a personal injury case indicator, year fixed effects, and the interaction between the personal injury case indicator and the post-reform announcement (July 2014). Additional controls include claimant gender, age, age squared, a fixed effect for the insurance company where the claim was submitted, and location fixed effects. Each column of Panel A defines a different subsample of the population on which the regression is run. Each column of Panel B defines a different time period on which the regression is run. The coefficient on the interaction term between personal injury and post-reform is reported for each specification. Each column of Panel C defines a different reweighting formula for personal injury payments. Average wage refers to reweighting payments by the average wage as constituting 47% of payments (see Biton (2023)). Private sector re-weights according to private sector wages. Wages (40% (60%) of payments) uses averages wages as constituting 40% (60%) of payments. Robust standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Internet Appendix

This Internet Appendix provides supplemental and robustness tests to accompany the results presented in the paper:

Does Privacy Matter? Evidence from a Legal Reform

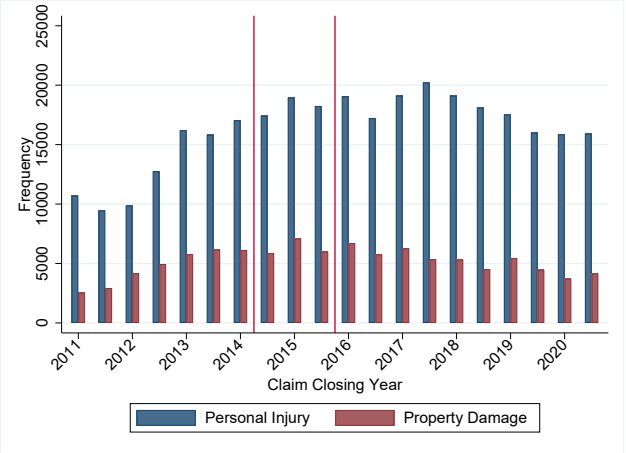
Liran Einav
Stanford University

Ehud Guttel
The Hebrew University

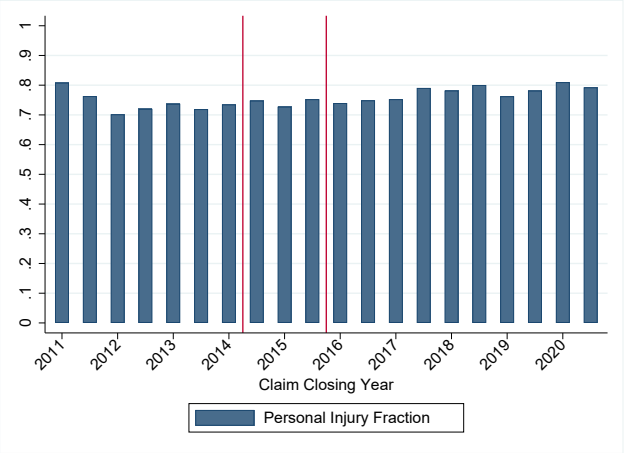
Ilan Kremer
University of Warwick and The Hebrew University

Guy Lakan
The Hebrew University

Sarit Weisburd
The Hebrew University



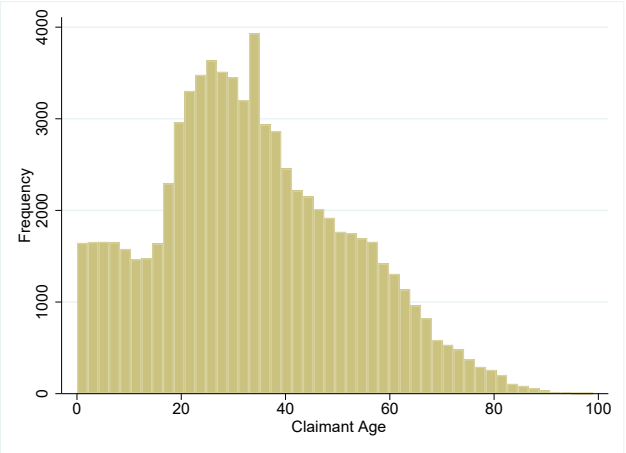
(a) The Number of Disputed Cases



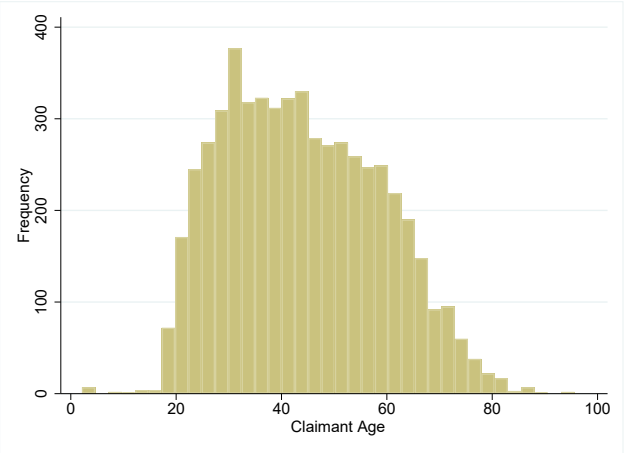
(b) The Fraction of Disputed Personal Injury Cases

Figure A1: Disputed Personal Injury & Property Damage Cases over Time

Notes: Panel (a) maps the number of closed disputed personal injury and property damage claims over time. Panel (b) maps the fraction of personal injury cases out of all closed disputed car accident cases over this period. The two vertical lines delineate the announcement of the legal reform (July 2014) and its implementation (August 2015).



(a) Personal Injury



(b) Property Damage

Figure A2: Age histograms

Notes: Panel (a) provides a histogram of the distribution of claimant age in personal injury cases. Panel (b) provides a histogram of the distribution of claimant age in property damage cases.

Table A1: **Database Variables**

Panel A: Insurance Databases	Definition
Insurance Company	Insurance company where claim was submitted
Claim ID	Unique ID number of claim
Date submit	Date claim was submitted
Date close	Date claim was closed and payment was made
Post reform	Defined as 1 if claim was closed after July 2014
Claim type	Defined as property damage or personal injury
Payment	Payment on claim in 2023 NIS
Lawyer representation	Defined as 1 if claimant was represented by lawyer
Lawyer name	First & last name of lawyer
Claimant ID	Unique ID number of claimant
Claimant address	Address of claimant
Claimant statistical area	Statistical area where claimant resides
Claimant age	Age of claimant
Claimant gender	Gender of claimant
Litigated in court	Defined as 1 if claim was ever litigated in court
Settlement	Defined as 1 if claim was resolved in a settlement
Panel B: Central Bureau of Statistics	Definition
Statistical area	CBS Statistical area
Average income	Average income at statistical area
Higher education	Fraction of men in statistical area with at least 13 years of education
Socioeconomic rank	Socioeconomic ranking of statistical area as defined by CBS based on demographic characteristics, education, income, housing characteristics, employment, and economic difficulties.
Ethnicity	Arab or Jewish is defined by the majority religion in statistical area
ultra-Orthodox	Defined as 1 if the majority of residents in statistical area define themselves as ultra-Orthodox
Panel C: Dun's 100 Top Law Firms	Definition
Lawyer name	First & last name of lawyer

Notes: This table lists the variables available in each of the original datasets used to compile our database.

Table A2: **Heterogeneity Results - Alternative Sample Definitions**

	Full Sample Difference	Subsample Difference
Panel A: Working Age		
Working Age X Personal Injury X Post	0.218***	0.204***
	(0.057)	(0.056)
Observations	428,094	190,186
Panel B: Represented by Top Lawyer		
Top Lawyer X Personal Injury X Post	0.076	0.095**
	(0.047)	(0.047)
Observations	428,094	354,881
Panel C: Long Case Duration		
Long Duration X Personal Injury X Post	0.264***	0.264***
	(0.018)	(0.018)
Observations	428,094	428,094
Panel D: Male		
Male X Personal Injury X Post	0.005	0.010
	(0.043)	(0.043)
Observations	428,094	190,510
Panel E: High Income		
High Income X Personal Injury X Post	-0.041	-0.035
	(0.049)	(0.050)
Observations	428,094	94,388
Panel F: Jewish		
Jewish X Personal Injury X Post	-0.097	-0.092
	(0.082)	(0.083)
Observations	428,094	94,388
Panel G: Ultra-Orthodox		
Ultra-Orthodox X Personal Injury X Post	0.047	0.042
	(0.092)	(0.093)
Observations	428,094	75,622

Notes: The dependent variable in all specifications is log(payment). Each estimate reports the coefficient from the the triple interaction (panel characteristic=1 x personal injury x post). The first column includes all observations and controls for missing values on the panel characteristic and interactions with personal injury and post. The second column restricts the sample to observations with non-missing values for the panel characteristic and corresponds to the difference specification reported in Table 3. Robust standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: **The Effect of Privacy Legislation on Payments (by Lawyer Expertise)**

	Regular Firms	Top Firms	DDD
Personal Injury	0.317***	0.392***	0.313***
	(0.012)	(0.068)	(0.012)
Personal Injury X 2015	0.154***	0.225***	0.153***
	(0.018)	(0.071)	(0.018)
Personal Injury X 2016	0.171***	0.477***	0.170***
	(0.018)	(0.078)	(0.018)
Personal Injury X 2017	0.262***	0.578***	0.261***
	(0.018)	(0.082)	(0.018)
Personal Injury X 2018	0.285***	0.429***	0.283***
	(0.019)	(0.080)	(0.019)
Personal Injury X 2019	0.423***	0.540***	0.421***
	(0.019)	(0.081)	(0.019)
Personal Injury X 2020	0.387***	0.582***	0.386***
	(0.019)	(0.084)	(0.019)
Personal Injury X 2015 X Top Law Firm			0.048
			(0.069)
Personal Injury X 2016 X Top Law Firm			0.266***
			(0.075)
Personal Injury X 2017 X Top Law Firm			0.279***
			(0.078)
Personal Injury X 2018 X Top Law Firm			0.165**
			(0.078)
Personal Injury X 2019 X Top Law Firm			0.086
			(0.078)
Personal Injury X 2020 X Top Law Firm			0.196**
			(0.081)
Mean Payments	38121.693	71784.925	39516.862
Observations	340182	14699	354881
Adjusted R-squared	0.099	0.168	0.103

Notes: The dependent variable in all specifications is log(payment). The right-hand side variables contain a personal injury case indicator, year fixed effects, and the interaction between the two for all years after the reform announcement (2014). Additional controls include claimant gender, age, age squared, a fixed effect for the insurance company where the claim was submitted, and location fixed effects. The first specification reports the coefficients when including only claims litigated by lawyers outside the Dunn's 100 list of top law firms in Israel. The second specification reports these same coefficients for claims litigated by lawyers listed within the Dunn's 100 list of top law firms in Israel. The third specification reports the coefficient on interaction terms and the triple difference interaction terms (top firm x personal injury case indicator x year). Robust standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$