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Deterrence and liability for intentional torts☆

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ABSTRACT

Law and economics suggests that liability for intentional torts is motivated by deterrence. The tortfeasor's investments in undertaking the intentional act and the victim's investments in precautions against the harm arising from the act are likely to be socially wasteful. Further, especially in the case of battery, the benefit of committing the intentional act will generally fall short of the loss to the victim. For these reasons, it makes sense to impose liability on the tortfeasor for the full loss experienced by the victim of an intentional tort. This deterrence theory requires that intentional tortfeasors are sensitive to liability exposure. To test this assumption, we examine changes in state-level homicide rates in response to caps on non-economic damages in tort. We find that murder rates increase by more than 5 percent when states adopt caps on non-economic damages.

1. Introduction

Since at least Landes and Posner's (1981) article, law and economics scholars have viewed liability for damages arising from intentional torts in a deterrence framework. Given the overlap between intentional torts and crime, this approach is not surprising. From an efficiency perspective, the activities that fall within both the crime and intentional tort categories, are likely to be socially wasteful, or, at least, we might want to induce a potential criminal/tortfeasor to bargain with his intended victim to ensure that the intentional act only occurs when the benefit to the criminal/tortfeasor exceeds the loss experienced by the victim. Further, as Tullock (1967) pointed out, the investments made by the criminal/tortfeasor to undertake the intentional act and any precautions undertaken by their victims will be socially wasteful. Efficiency, then, calls for full liability for all of the harms arising from intentional torts so as to deter the acts in the first place.

However, as with any deterrence based argument, a necessary assumption for the legal system to achieve these efficiency ends is that criminals/tortfeasors are aware of and sensitive to the penalties they face. In a standard Beccaria/Bentham/Becker type model, the relevant penalties can be criminal, civil, or both. While the sensitivity of individuals to criminal penalties has been studied extensively, much less has been done on the civil side. To the extent anyone has examined the empirical effect of civil liability on individual behavior, the focus has been entirely on unintentional or accidental harms.

We examine the sensitivity of individuals to liability for intentional torts by analyzing how the murder and non-negligent manslaughter rate changes when tort liability exposure is exogenously lowered by the adoption of caps on non-economic damages in tort litigation. If these caps are binding, and individual behavior is responsive to liability exposure, we should see an increase in intentional tortious acts when caps are adopted.

We find that when non-economic damage caps are adopted, the state murder rate increases proportionately by more than 5 percent, and this effect is statistically significant. Although there is no obvious story why tort reforms would be endogenous to changes in the incidence of murder, our result could be coincidental to unobserved variation within states over time. However, we show that while we observe the statistically significant increase in murder rates when states pass damage caps that include intentional acts, we do not observe the decline in states that adopt other kinds of liability caps, such as those that exempt intentional acts or those that are specific to medical malpractice. This finding increases our confidence that the effect we find is causally related to the change in liability exposure. Our confidence is bolstered by the fact that we find no effect on assault rates and property crime rates (while both of these categories do include tortious acts, the damages involved in these categories will almost always be well below the level set by damage caps). We also find no systematic effect on suicide rates. Further,

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we also provide evidence that the effect we identify is larger when tortfeasors are more likely to have assets at risk.

Our results add to the notion that criminal and tortious behavior is sensitive to expected costs and penalties. That this effect would be present in the tort context is potentially surprising given the growing consensus in criminology that certainty and celerity are necessary for punishments to generate deterrence. Given that the tort system is relatively slow and haphazard, our results raise some doubt regarding that consensus.

2. Nobody sues and tortfeasors are broke anyway

Conventional wisdom regarding who commits intentional torts and who actually sues when they are the victim of intentional torts may generate some skepticism about our results. There is a belief among legal scholars that intentional torts do not generate much litigation; therefore, the story goes, no one will be deterred by liability for intentional torts. The primary reason for this belief is the view that plaintiffs (and perhaps more importantly, plaintiffs' lawyers) generally only litigate in situations where it is easy to get paid if the case is won. As a practical matter, tortfeasors who are covered by insurance will be more attractive targets, but damages arising from intentional torts will not generally be covered by a tortfeasor's liability insurance.

Absent the deep pockets of an insurer, conventional wisdom suggests litigation is rare. Qualitative data collected by Baker (2001) indicate that plaintiffs are reluctant to pursue a defendant's personal assets either because doing so is seen as being unfair in many circumstances or because it is simply difficult to gain access to those assets because of legal protections enjoyed in bankruptcy, property, or trust law (see Baker et al., 2016). Beyond that, there is a general belief that the kind of individuals who engage in intentional torts have few assets anyway, making it impossible to collect from so-called judgment proof defendants.

Data on the US tort system are not great (see Helland et al., 2005). However, the best available data tell a different story from the conventional wisdom. The Civil Justice Survey of State Courts[1] provides data about the nature of civil litigation, including information about the claims, the parties, and the outcomes of state court cases that proceed to trial. The most recent data collected involve disputes disposed of by a bench or jury trial in 2005. These data provide a nationally representative sample of jurisdictions and cover 16,397 tort cases resolved through a trial in state courts.

While it is true that only 725 (or 4.4 percent of the tort claims)[2] of these cases involve intentional torts (not including libel, which is included in its own category in the data, or fraud claims which are counted as contract disputes in the data even if they are actually tort claims), as a point of comparison, there are only 2449 medical malpractice claims in the dataset.[3] Further, it is well known that the vast majority of disputes settle before a trial outcome is reached. If one were to assume that the share of total trials involving intentional torts is comparable to the share in overall filed claims, intentional tort claims would number about 200,000 in 2005. This suggests that lawsuits involving intentional torts are not particularly rare.

Whether tortfeasors are judgment proof is more difficult to ascertain. Data from the National Crime Victimization Survey indicate that the modal perpetrator of violent crime is white (based on victim perceptions) and in his late 20 s or older (again based on victim perceptions). Taking these characteristics as given for the general demographics of intentional tortfeasors, if we examine wealth using the Federal Reserve's Survey of Consumer Finance, in 2013 white households with a male respondent aged 25 or greater had average wealth of $859,016. This is well above the modal damage cap ($250,000) and the average damage cap ($422,000). More than 43 percent of male headed households in this race category and age range have a net worth exceeding the modal damage cap and more than 30 percent have a net worth that exceeds the average damage cap. While it is impossible to know if the individuals at risk to engage in an intentional tort exhibit similar wealth to the individual's represented by these data, it is at least possible that many potential tortfeasors have assets that do exceed the damage caps.

Further, based on the state court data, the average plaintiff judgment for an intentional tort case involving a death is about $371,000, so caps will be relevant in many of these cases.[4] The $106,000 award for intentional tort cases not involving death suggests that liability exposure arising from less severe assaults and the like (e.g., torts involving only harm to property) will be unaffected by damage cap changes.

3. Do caps affect murder rates?

To examine the sensitivity of murder rates to changes in liability exposure, we estimate difference-in-difference models where the outcome variable is the natural log of murders and non-negligent manslaughters per 100,000 population. The control variables include state and year fixed effects, as well as state specific trends. Our treatment variable is the fraction[5] of the year in state X year cells in which non-economic damages have been capped in tort cases and where intentional acts are not exempted.[6] We call this variable "effective damage cap." We weight all regressions by state population[7] and we cluster standard errors by state. We provide separate estimates for the entire sample and for the subset of states that pass any damage caps at some point in our sample period which runs from 1980 to 2012. In subsequent specifications, we include a variety of control variables.

Table 1 provides descriptive statistics.

Table 2 provides a starting point where we examine the difference in difference specification, alternately allowing for no state-specific trend, as well as linear and quadratic state-specific trends. The quadratic trend guards against the concern raised in Wolfers (2006) about subsuming the treatment effect into the generalized trend. As a general matter, the damage caps are associated with increased murder rates. Although there is a difference between the no state-specific trend results and the results for the specifications with state specific trends included, linear and quadratic trends give comparable results.[8]

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1 http://www.ncsc.org/services-and-experts/areas-of-expertise/civil-justice/civil-justice-survey/civil-justice-survey-data.aspx

2 https://www.bjs.gov/content/pub/pdf/cbjtsc05.pdf

3 There is a large literature examining the effect of medical malpractice liability on physician behavior, suggesting that changing liability affects physician behavior on both the extensive (see Klick and Stratmann, 2007; Helland and Showalter, 2009) and the intensive (see Frakes, 2012) margins.

4 More than 11 percent of the tort cases won by plaintiffs covered in the 2005 civil justice survey resulted in damages greater than $250,000.

5 Results are similar if a 0–1 indicator is used where 1 is assigned if the state had a cap in place during any part of the year.

6 The timing for these reforms was coded based on Avraham (2014).

7 See Klick et al. (2012) for an extended discussion of why population weights are appropriate in these situations. In short, when averaging over state to state heterogeneity in estimated treatment effects, it makes little sense to treat New Hampshire as equally important as California.

8 Using tvdiff (Cerulli and Ventura, 2017), it appears as though the specification without state-specific trends does not pass the test for parallel trends using five pre-reform period dummies to account for a difference in potentially non-linear pre-trends, while the specifications including linear and quadratic state-specific trends do pass the parallel trends test using five pre-reform period dummies. All three specifications pass the parallel trends test if pre-reform differential trends are

Table 1
Descriptive Statistics.

	All States		Reform States Only		
Variable	Mean	S.D.	Mean	S.D.	Source
Murders (UCR) per 100,000	7	4	7	3	BJS
Assaults per 100,000	329	125	345	148	BJS
Suicides per 100,000	12	3	12	2	CDC
Property Crimes per 100,000	4107	1235	4282	1164	BJS
Effective Damage Cap	0.12	0.32	0.15	0.36	Avraham
Damage Cap Excluding Intentional Acts or for Medical Malpractice	0.45	0.50	0.59	0.49	Avraham
Young Male Pop/Total Pop	0.11	0.01	0.12	0.01	SEER
Black Pop/Total Pop	0.13	0.08	0.13	0.08	SEER
Employed Pop/Total Pop	0.47	0.03	0.47	0.03	UKCPR
Pop Below Poverty Line/Total Pop	0.14	0.03	0.14	0.03	UKCPR
Real Per Capita Personal Income	38760	7402	37965	6630	UKCPR
Democratic Governor	0.48	0.50	0.46	0.50	UKCPR
Democratic House	0.57	0.14	0.56	0.14	UKCPR
Democratic Senate	0.55	0.16	0.57	0.16	UKCPR
Prison Pop per 100,000	347	161	362	165	BJS
Number of Executions	2	5	2	6	BJS
Right To Carry Law	0.40	0.49	0.42	0.49	Donohue
Police Officers Per Capita	290	62	280	42	Census
Corrections Officers Per Capita	213	69	211	65	Census
Case Shiller Home Price Index/100	1.04	0.42	1.04	0.42	FRED

Data references: BJS (2020); CDC (2020); Donohue et al. (2017); Federal Reserve Board (2020); FRED (2020); SEER (2020); UKCPR University of Kentucky Center for Poverty Research (2016); United States Department of Justice (2005); U.S. Department of Justice (2009).

Table 2
Effect of Damage Caps on Murder Rates (standard errors clustered by state).

	All States			Reform States Only		
Effective Damage Cap	0.20** (0.08)	0.08** (0.03)	0.08** (0.03)	0.20** (0.08)	0.09** (0.03)	0.09** (0.03)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	None	Linear	Quadratic	None	Linear	Quadratic

Dependent variable = ln(murder rate); sample period 1980–2012; estimated with population weights.
*$p < 0.10$ (against a two-sided test of a zero effect).
**$p < 0.05$ (against a two-sided test of a zero effect).
***$p < 0.01$ (against a two-sided test of a zero effect).

Table 3
Effect of Damage Caps on Murder Rates Controlling for Property Crime Rates (standard errors clustered by state).

	All States			Reform States Only		
	Pooled	State-Specific	DDD	Pooled	State-Specific	DDD
Effective Damage Cap	0.09*** (0.02)	0.06** (0.03)	0.09*** (0.03)	0.09*** (0.03)	0.07** (0.03)	0.09*** (0.03)
ln(property crime rate)	0.64*** (0.14)			0.50*** (0.13)		

Dependent variable = ln(murder rate); sample period 1980–2012; estimated with population weights.
*$p < 0.10$ (against a two-sided test of a zero effect).
**$p < 0.05$ (against a two-sided test of a zero effect).
***$p < 0.01$ (against a two-sided test of a zero effect).

To account for other crime relevant changes that may confound our treatment effect estimation, in Table 3, we control for the log of the property crime rate. This variable should be unrelated to damage caps, but will otherwise provide a composite control for a number of crime policy variables, as well as other state characteristics that affect crime. We provide three different approaches, with varying levels of generality of the relationship between property crime and the murder rate. In the pooled specification, we simply include property crime as a covariate, averaging the coefficient across all of the states. In the state-specific specification, we allow for a different coefficient on property crime for each state. Lastly, in the DDD specification, we use state by year property crime to estimate a triple differences model that includes a full set of fixed effects (state X year; crime category X year; state X crime category). Uniformly, we estimate a positive effect of the effective damage cap on the murder rate, and the coefficient shows little variability across the specifications.

Table 4 includes a host of control variables in addition to the property crime rate in the original difference in difference model. These controls include the demographic variables percentage of the population that is a young male and percentage that is black. The model also includes economic variables including the employment to population ratio, the poverty rate, and real per capita personal income. To account directly for crime policy variables, this specification includes the fraction of the population that is in prison,

assumed to be linear. To use the tvdiff program, we must convert our reform variable to a 0−1 indicator which takes the value of 1 if the state had the reform at any point in the year.

Table 4
Effect of Damage Caps on Murder Rates (standard errors clustered by state).

	All States	Reform States Only
Effective Damage Cap	0.07**	0.06*
	(0.03)	(0.03)
ln(property crime rate)	0.61***	0.52***
	(0.12)	(0.09)
Young Male Pop/Total Pop	3.99	3.54
	(2.40)	(2.82)
Black Pop/Total Pop	4.97**	5.28
	(2.46)	(3.14)
Democratic Governor	0.01	0.00
	(0.01)	(0.02)
Democratic House	0.04	−0.07
	(0.17)	(0.10)
Democratic Senate	−0.11	−0.16
	(0.14)	(0.15)
Employed Pop/Total Pop	0.21	0.34
	(0.88)	(1.07)
Pop Below Poverty Line/Total Pop	−0.51	−0.38
	(0.38)	(0.35)
Real Per Capita Personal Income	0.00	0.00
	(0.00)	(0.00)
Prison Pop/Total Pop	−0.00	−0.00
	(0.00)	(0.00)
Number of Executions	0.00	0.00
	(0.00)	(0.00)
Right To Carry Law	−0.06	−0.07
	(0.04)	(0.05)
ln(Police Per Capita)	−0.04	−0.04
	(0.07)	(0.12)
ln(Corrections Officers Per Capita)	−0.13	−0.23
	(0.09)	(0.10)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State-Specific Trends	Linear	Linear

Dependent variable = ln(murder rate); sample period 1980–2012; estimated with population weights.
*p < 0.10 (against a two-sided test of a zero effect).
**p < 0.05 (against a two-sided test of a zero effect).
***p < 0.01 (against a two-sided test of a zero effect).

Table 5
Include Caps That Exclude Intentional Acts (standard errors clustered by state).

	All States	Reform States Only
Effective Damage Cap	0.08**	0.07*
	(0.03)	(0.03)
Damage Cap Excluding Intentional Acts and Medical Malpractice Caps	−0.03	−0.03
	(0.04)	(0.03)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State-Specific Trends	Linear	Linear
Controls	Yes	Yes

Dependent variable = ln(murder rate); sample period 1980–2012; estimated with population weights. Controls = ln(property crime rate), Democratic Governor, Democratic House, Democratic Senate, Young Male Pop/Total Pop, Black Pop/Total Pop, Employed Pop/Total Pop, Pop Below Poverty Line/Total Pop, Real Per Capita Personal Income, Prison Pop/Total Pop, Number of Executions, Right To Carry Law, ln(police officers per capita), ln(corrections officers per capita).
*p < 0.10 (against a two-sided test of a zero effect).
**p < 0.05 (against a two-sided test of a zero effect).
***p < 0.01 (against a two-sided test of a zero effect).

the number of executions carried out by the state, an indicator for whether the state has implemented a right to carry law, and the log of per capita police officers and corrections officers. We also include political variables to allow for the possibility that other policies might affect the murder rate. Once again, we find that effective damage caps are associated with a statistically significant increase in the murder rate, on the order of 6 percent.

To attempt to isolate causality, in Table 5, we leverage the fact that some states adopting non-economic damage caps explicitly exempt intentional acts from the coverage of the caps. States also frequently pass damage caps that are specific to medical malpractice claims. If our results are not due to changes in liability exposure, but rather are due to some other variable that happens to be associated with the passage of damage caps, we should find that the caps exempting intentional acts or that are specific to medical malpractice have some effect on murder rates too. If, instead, our results are picking up behavioral changes related to the changing expected cost associated with tort liability, we should not find an effect from the caps that do not apply to intentional violence.

Consistent with the behavioral explanation, while the effective damage caps are associated with a statistically significant 8 percent increase in the murder rate, the caps exempting intentional acts are associated with a much smaller and statistically insignificant effect. These results improve our confidence in a causal behavioral interpretation.

To continue to improve our ability to make causal claims, Table 6 provides results that allow the effect of the damage caps to depend on proxies for the net worth of the individual facing the cap. In tort law, an award is largely meaningless if the defendant does not have sufficient assets to pay the award. For example, to an individual with a net worth below $250,000, the passage of a $250,000 damage cap does little to change his liability exposure.

We attempt to capture this intuition in two ways. For many individuals, the most important component of net worth is a residence. All other things equal, when housing values are higher, individuals will be less likely to be judgment proof. In the first specification, in addition to the effective cap variable, we also have an interaction between that variable and the national Case-Shiller housing index. Our results suggest that in periods when housing values are higher, the effect of damage caps on the murder rate is larger. This is what we would expect if tortfeasors are changing their behavior based on their liability exposure.

Because the Case-Shiller index is not available on a state by state basis, in the second specification, we interact the cap variable with real state level per capita personal income (which we continue to include as a covariate in its un-interacted form). All other things equal, individuals will be less likely to be judgment proof when their incomes are higher, implying that any effect of the caps should be increasing in income if our results are driven by behavior changes induced by changing liability. This is what we find. Specifically, the baseline effect of the damage caps is slightly negative, but as incomes rise in the states covered by the caps, the effect of the caps becomes increasingly positive.

The effective damage cap and its interaction with the Case Shiller index (in both samples) imply that there is not much of an effect of the caps in state-years when home valuations are low, but there is a substantial effect when housing values are high. For example, for the reform state only sample, at the lowest housing value level, the implied joint effect is a statistically insignificant ($p > 0.50$) +2 percent, while at the median housing value, the implied effect is +7 percent ($p < 0.10$), and at the maximum housing value, the implied effect is +12 percent, though it is not statistically significant ($p < 0.20$)

For the income interaction, there is a similar punchline. For the reform state only sample, at the minimum real per capita income level, the implied joint effect is a statistically insignificant ($p < 0.20$) -9 percent, while at the median real per capita income, the effect is +9 percent ($p < 0.05$), and at the maximum real per capita income, the effect is +29 percent ($p < 0.05$).

These interaction effects are consistent with the hypothesis that tortfeasors change their violent behavior as liability exposure changes but are insensitive to liability changes when wealth and income levels suggest many offenders will be judgment proof.

In a last attempt to ensure our causal interpretation is not warranted, in Table 7, we examine outcomes that are related to murder

Table 6
Effect Grows as Net Worth Proxies Rise (standard errors clustered by state).

	All States		Reform States Only	
Effective Damage Cap	−0.04	−0.23*	−0.24	−0.29*
	(0.27)	(0.13)	(0.30)	(0.16)
Effective Damage Cap * ln(Case Shiller Index)	0.03		0.07	
	(0.07)		(0.07)	
Effective Damage Cap * (State Per Capita Personal Income/10,000)		0.09**		0.10**
		(0.04)		(0.05)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Trends	Linear	Linear	Linear	Linear

Dependent variable = ln(murder rate); sample period 1980–2012; estimated with population weights. Controls = ln(Case Shiller Index), ln(property crime rate), Democratic Governor, Democratic House, Democratic Senate, Young Male Pop/Total Pop, Black Pop/Total Pop, Employed Pop/Total Pop, Pop Below Poverty Line/Total Pop, Real Per Capita Personal Income, Prison Pop/Total Pop, Number of Executions, Right To Carry Law, ln(police officers per capita), ln(corrections officers per capita).
*p < 0.10 (against a two-sided test of a zero effect).
**p < 0.05 (against a two-sided test of a zero effect).
***p < 0.01 (against a two-sided test of a zero effect).

Table 7
Other Outcomes (standard errors clustered by state).

	All States			Reform States Only		
	Assaults	Property Crimes	Suicide Rate	Assaults	Property Crimes	Suicide Rate
Effective Damage Cap	−0.01	0.00	−0.00	−0.02	0.00	−0.00
	(0.05)	(0.02)	(0.02)	(0.05)	(0.02)	(0.02)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Linear	Linear	Linear	Linear	Linear	Linear

Dependent variable = ln(assault rate), ln(property crime rate), and ln(suicide rate) accordingly; sample period 1980–2012; estimated with population weights. Controls = ln(property crime rate)[except in property crime regressions], Democratic Governor, Democratic House, Democratic Senate, Young Male Pop/Total Pop, Black Pop/Total Pop, Employed Pop/Total Pop, Pop Below Poverty Line/Total Pop, Real Per Capita Personal Income, Prison Pop/Total Pop, Number of Executions, Right To Carry Law, ln(police officers per capita), ln (corrections officers per capita).
*p < 0.10 (against a two-sided test of a zero effect).
**p < 0.05 (against a two-sided test of a zero effect).
***p < 0.01 (against a two-sided test of a zero effect).

rates but should be largely unaffected by the changes in liability exposure occasioned by the passage of damage caps. We examine assault rates and property crime rates on the assumption that neither of these activities is likely to generate damages anywhere near the threshold for the damage caps. We also examine suicide rates since suicide is largely unaffected by the tort system.[9] We do not find a statistically significant effect of damage caps on any of these outcomes, and in every case, the point estimate of the damage cap coefficient is substantially smaller than what we find in the murder rate regressions.

4. Conclusion

Civil liability is a previously unexplored channel through which the deterrence of violent crime, specifically murders and non-negligent manslaughters, may occur. We present evidence that reducing liability exposure through the passage of noneconomic damage caps increases the murder rate by more than 5 percent. We believe this estimated effect may be causal. Although this may seem surprising, it is worth considering why intuition suggests liability incentives may affect negligent behavior, as seems to be the case in the medical malpractice literature, but it is assumed not to affect intentional behavior. Arguably, for example, if one believes that insured physicians modify their behavior when liability exposure changes, it would seem inconsistent to find it implausible that violent individuals might be deterred by tort liability. Our confidence is bolstered by the finding that the estimated effect increases as proxies for offender assets rise.

In addition to opening up the consideration of the effects of tort law on crime outcomes, this paper provides evidence of deterrence and rational decision making among the group of individuals committing some subset of murders. This result is notable given the conventional wisdom that for penalties to deter, they must be certain and swift. Tort litigation is neither certain nor swift, and, yet, it appears to affect criminal decision-making on the margin.

[9] Edwards (2013) does note that suicides could be related to tort law through the channel of laws requiring mental health therapists to warn the public if they believe their patients are dangerous. This potential lack of confidentiality could reduce the willingness of patients to seek therapy, perhaps leading to a change in the suicide rate. This effect would be largely unrelated to damage caps.

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