

Law as a Local Amenity

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ABSTRACT. The conventional approach to evaluating a law is to examine its effect on proximate behavior. To evaluate a new criminal law, for example, the conventional approach would look to changes in the crime rate. This paper argues instead that laws should be judged by the extent to which they raise housing prices and lower wages. The logic is that the value of a law, much like the value of a lake or a public school, is capitalized into local housing and labor markets. Desirable laws increase housing prices and decrease wages because more people want to live in the relevant jurisdiction; undesirable laws have the opposite effects. Evaluating laws in the manner has several advantages over the conventional approach. First, it employs a more direct proxy for utility. Second, it accounts for all the effects of a law, including hard-to-measure outcomes, unintended consequences, and enforcement costs. Third, it permits direct comparison of different types of laws, which is important in instances where law-makers have limited resources to invest in law-making. Lastly, it sheds light on the distributional consequences of a law. In particular, it makes clear that a significant portion of every law's benefits are reallocated through housing and labor markets to property owners.

I. Introduction

The value of a law should be judged by the extent to which it raises housing prices and lowers wages.² This may seem an odd way to assess the welfare effect of a law. After all, higher housing prices and lower wages are thought to be bad outcomes, not good ones. But the proper way to understand these changes is as signals of positive outcomes, not positive outcomes themselves. They indicate that something good has happened in the community. Housing prices go up because more people want to live there. Wages go down because more people want to work there. Phrased more formally, higher housing prices and lower wages are how markets ration an attractive local amenity. Indeed, the increase in housing prices combined with the reduction in wages provides a measure how much people are willing to give up to enjoy the amenity. Conventional economic thinking recognizes this when it comes to estimating the social value of a new park or a better school. The same logic, I will argue here, applies when the amenity is anything from a better tort system to smarter rules regarding capital punishment.

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² To be clear, I contend that the value of a law is the sum of the increase in housing prices and the magnitude (or absolute value) of the decrease in wages. So, e.g., if a law increases housing prices by \$1 and reduces wages by \$1, then the total value of the law is \$2.

This is, of course, not the standard practice. Under the conventional approach, the welfare effect of a law would be measured by evaluating the law's effect on specific, related behaviors. For example, a three-strikes law would be evaluated by its effect on homicides;³ a unilateral divorce law by its impact on rates of domestic violence⁴ or divorce;⁵ and a tort reform by its impact on insurance premiums⁶ and accidents.⁷ These are certainly sensible metrics for judging the laws at issue. But none is as effective at measuring the welfare effect of a law as its impact on housing prices and wages.

First, the housing and wages approach employs a more direct proxy for welfare. The conventional approach tells us how much, e.g., the felony-murder rule reduces robbery,⁸ but it does not tell us how much people value that reduction in robbery. Yet that is the very strength of my proposed approach. The increase in housing prices and the loss of wages reveals how much the marginal resident who moves to a community is willing to pay – in terms of lower non-housing consumption – to be subject to a new law in that community.

Second, the conventional approach often provides an incomplete picture of any given law. Frequently, relevant implications are too hard to measure or are unexpected, and are therefore left out of the empirical analysis. For example, a typical study might ignore the expressive benefits of an anti-discrimination law or the placebo effects of corporate governance reforms⁹ because these consequences are so hard to quantify. With respect to unexpected outcomes, until recently scholars studying abortion rights overlooked the important effect of abortion rights on crime rates.¹⁰ The conventional approach also tends to ignore the enforcement costs of laws, whether direct (higher property taxes) or indirect (reduction of other government services). The housing and wages approach does not suffer these omissions. It provides a measure of the net benefits of a law, accounting for intangible benefits, unintended consequences, and enforcement costs.

Third, because the conventional approach uses setting-specific metrics for evaluating different laws, it does not permit a direct comparison of different types of laws. For example, it cannot tell us whether it is better to have gay marriage,¹¹ capital punishment,¹² or exceptions to employment-at-will.¹³ But the housing and wages

³ Thomas Marvell and Carlisle Moody, *The Lethal Effects of Three-Strikes Laws*, 30 *J. Legal Studies* 89 (2001).

⁴ Betsey Stevenson and Justin Wolfers, *Bargaining in the Shadow of the Law: Divorce Laws and Family Distress*, NBER Working Paper 10175 (2003).

⁵ Leora Friedberg, *Did Unilateral Divorce Raise Divorce Rates: Evidence from Panel Data*, 88 *Amer. Econ. Rev.* 608 (1998).

⁶ Cite.

⁷ Paul Rubin and Joanna Shepard, *Tort Reform and Accidental Deaths*, working paper (2005).

⁸ Anup Malani, *Does the Felony-Murder Rule Deter?*, working paper (2000).

⁹ Amitai Aviram, *The Placebo Effect of Law*, working paper 11 (2005).

¹⁰ J.J. Donohue and S.D. Levitt, *The Impact of Legalized Abortion on Crime*, 116 *Quarterly J. Econ.* 379-420 (2001).

¹¹ Thomas Dee, *Forsaking All Others: The Effects of Gay Marriage on Risky Sex*, NBER Working Paper (Oct. 2005).

¹² See, e.g., H. Naci Mocan and R. Kaj Gittings, *Getting of Death Row: Commuted Sentences and the Deterrent Effect of Capital Punishment*, 46 *J. Law & Econ.* 453 (2003); Joanna Shepherd, *Murders of Passion, Execution Delays, and the Deterrent of Capital Punishment*, 33 *J. Legal Stud.* 283 (2004); J.J. Donohue and Justin Wolfers, *Uses and Abuses of Empirical Evidence in the Death Penalty Debate*, 58 *Stanford L. Rev.* 791-846 (2005).

approach can. The reason is that it compares all laws by their effect on a common outcome: the increase in housing prices plus the decrease in wages. The resulting ability to compare different types of laws is quite valuable. For one thing, legislators have limited time and resources. Studies that rank legal reforms will allow legislators to focus on those changes that have the biggest positive impact on residents of their jurisdiction. Such a ranking will also help voters choose among candidates based on policies that actually impact welfare, rather than policies that mainly stroke emotions.

Finally, the housing and wages approach offers a benefit that goes beyond simply being able to better measure the value of law. It provides an important insight into who really benefits from legal reforms. Because local housing is necessary to enjoy a local law, and because people are mobile, but housing is not, a significant part of the welfare gains (or losses) from a local law accrue to the suppliers of housing, i.e., the owners of local property. As a result, a law may not have the precise distributional impact that its authors intend. In other words, labor market forces alter the assignment of gains and losses from a law and unless lawmakers take this into account, they may not achieve an important component of their objectives.

To be clear, this paper does not contend that the housing and wages approach offers a perfect measure of welfare. It has important limitations. From a normative perspective, it gives disproportionate weight to individuals with greater income. It ignores individuals – such as prisoners and military personnel – who do not participate in the housing market. And there is some leakage when evaluating, for example, laws which convey benefits or impose costs on other jurisdictions. But, for the reasons given above, it is a better second-best than the conventional approach to valuing the within-jurisdiction benefits of a law, as well as competing methods for estimating the willingness-to-pay for public goods. Moreover, so long as the limitations inherent in my approach affect all applications equally, it can still be used to conduct relative welfare analysis or rank different legal reforms.

Skeptics will surely wonder whether there is too much noise in housing and wage data to identify the (likely small) effects that any individual law has on those outcomes. But this is an empirical question and the paper offers an empirical answer. It examines the effect of six types of laws (tort reforms, abortion access laws, no-fault automobile liability, unilateral divorce laws, capital punishment, and health insurance mandates) on local housing prices and wages. Data on housing prices and characteristics are drawn from the American Housing Survey. This survey spans odd years from 1974-2003 and includes over 50,000 households per year. Data on wages are from the Current Population Survey. The March portion of the survey provides useful data annually from 1979-2003 on up to 15,000 individuals per year. Data on laws are from recent studies by Jonathan Klick, Thomas Stratmann, Paul Rubin, Joanna Shepherd, Leora Friedberg, and RAND. My preliminary results suggest that tort reform may reduce local welfare and that executions and diabetes coverage mandates may raise local welfare. (I stress, however, that these findings have not been demonstrated robust and should be taken as a proof-of-concept for my methodology rather than as policy recommendations.)

¹³ See, e.g., Thomas Miles, Common Law Exceptions to Employment at Will and U.S. Labor Markets," 16 J. L. Econ. & Org. 74 (2000); David Autor et al., The Costs of Wrongful Discharge Laws, MIT Working Paper (May 2005).

This paper relates to an extensive literature on the so-called hedonic valuation method in the fields of environmental, labor and urban economics. That method attempts to measure the value of a given product characteristic that is bundled with other product characteristics by examining how changes in the characteristic affect product prices. Although the characteristic that environmental and urban economists are interested in valuing are local amenities such as lakes or schools, they have tended to focus on the capitalization of these amenities into the price of housing. Labor economists are not concerned with valuing local amenities so much as using the presence of amenities to explain persistent regional variation in price of labor, i.e., wages. In these literatures, this paper most closely relates to a line of papers beginning with Roback (1982), which offered a simple general equilibrium model to demonstrate how local amenities were capitalized in both the housing prices and wages.¹⁴

This paper also relates to the literature on Tiebout sorting, which focuses on the migration that causes the capitalization of amenities into local prices.¹⁵ It is most closely connected to, though distinct in purpose from, recent work by William Fischel, which also belongs to the field of local government law. Fischel's "homevoter hypothesis" argues that, because local government policies (even those creating public goods) are capitalized into housing prices and homeowners vote based on the value of their homes, local governments follow policies that maximize local welfare.¹⁶ This paper agrees with – in fact provides evidence for – the claim that local laws (as opposed to policies broadly¹⁷) are capitalized into housing prices. However, my goal is not to explain voting but to value laws. Moreover, my analysis is not confined to local laws, but applies to state and even national laws. Fischel specifically rejects the homevoter hypothesis beyond the local government level.¹⁸ Lastly, Fischel ignores capitalization of amenities into rental properties and wages, which he does not think motivates voting.¹⁹ I focus equally on renters and owners and on housing and labor markets.

Against this background, my paper makes four discrete contributions. Although it is not the first paper to examine the effect of a law on housing prices, it is the first paper that neither examines a law closely related to the housing market – such as an

¹⁴ Roback, Jennifer, Wages, Rents, and the Quality of Life, 90 *J. Pol. Econ.* 1257-1278 (1982). See also, Glenn C. Blomquist, Mark C. Berger, and John P. Hoehn, New Estimates of Quality of Life in Urban Areas, 78(1) *Amer. Econ. Rev.* 89-107 (1988). These papers were spurred by two seminal papers by Sherwin Rosen, Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition, *J. Pol. Econ.* (1974) and Wage-based indexes of urban quality of life, In P. Mieszkowski and M. Straszheim, eds., *Current issues in urban economics* (1979). (Interestingly, two important law and economics scholars have written on this topic, though at the time they were working in the field of public finance and did not spell out the implications of their work for the empirical analysis of laws, Mitchell Polinsky and Daniel L. Rubinfeld, Property Values and the Benefits of Environmental Improvements: Theory and Measurement, in Lowdon Wingo and Alan Evans, eds., *Public Economics and the Quality of Life* (1977).)

¹⁵ Charles Tiebout, A Pure Theory of Local Expenditures, 64 *J. Pol. Econ.* 416-424. Tiebout's goal was to respond to Paul Samuelson's claim that market processes could not produce the optimal level of public expenditures on public goods. Tiebout claimed that migration – a market process – could produce optimal expenditure by moving people to good government rather than by improving local government.

¹⁶ William A. Fischel, The Homevoter Hypothesis 1-18 (2001). So Fischel's response to Samuelson is that local government politics can provide the optimal level of goods without actual migration.

¹⁷ To see examples of the difference, consider the examples of capitalization Fischel offers. *Id.* at 45.

¹⁸ *Id.* at 53-54.

¹⁹ *Id.* at 14, 80.

environmental, property or educational law²⁰ – nor views a law as a proxy (or “instrumental variable”) for an underlying neighborhood characteristic that is the true variable of interest.²¹ Second, it is the first paper that examines the effect of a law on both housing price and wages; in other words it is the first to account for the fact that the value of a law is capitalized into multiple markets. Third, although it is not first paper to employ a differences-in-differences estimator to value a local amenity, it is the first to apply this strategy with a large panel data set that spans many jurisdictions and a large number of years. Fourth, and most importantly, this is first paper to make the general case for employing hedonic analysis to evaluate the net welfare and distributional effects of a law, a contribution to the law and economics literature.

The remainder of this paper is organized as follows. Section II explains why the value of a law is capitalized into housing prices and labor wages.²² It also compares the housing and wages approach to the conventional approach to valuing a law. Section III illustrates the housing and wages approach by employing it to evaluate an array of laws.

II. The housing and wages approach

A simple example can illustrate how the value of a local law is capitalized into local housing prices and wages. Consider two contiguous states with identical laws, housing prices and wages. Because the two states are identical, there is no migration between them. Suppose, however, that the first state passes a law that directly improves the welfare of its residents. By this I mean it is a law that people prefer for personal reasons. It might be a felon disenfranchisement law that makes a statement about felons²³ or a parental notification law that comforts parents of teenagers.²⁴

Residents of the second state, who also prefer the law, will begin to move to the first state, in order to enjoy the law. This movement has two effects. First, because migrants need housing, the demand for housing will increase and housing prices will rise. Second, because migrants need jobs, the supply of labor will increase and wages will fall. The migration from the second state to the first state will continue until the increase in housing prices and the reduction in wages is such that remaining residents of the second state are indifferent between living under the new law in the first state and enjoying the lower housing prices and higher wages in the second state. At that point there is no net gain to an individual’s welfare from living under the new law so the second state’s remaining residents stay put. In other words, local housing prices and wages adjust to restore an equilibrium in which there is no further migration between the two states.

A useful byproduct of this equilibrating process is that we now have a measure of the value of the new law: the amount that housing prices rise plus the amount that wages fall. Economists call this the “compensating differential” for enjoying the law. That is

²⁰ Greenstone, Michael, and Justin Gallagher, Does hazardous waste matter? Evidence from the housing market and the superfund program, NBER Working Paper No. 11790 (2005); J.M. Pogodzinski and Tim R. Sass, Zoning and Hedonic Housing Price Models, 1 J. Housing Econ. 271-292 (1991); AER paper on school grades.

²¹ Greenstone and Gallagher 2005.

²² [A future appendix will provide a more formal, general model.]

²³ Thomas Miles, Felon Disenfranchisement and Voter Turnout, 33 J. Legal Stud. 118 (2004).

²⁴ Jonathan Klick and Thomas Stratmann, Abortion Access Laws and Risky Sex Among Teens: Parental Involvement Laws and Sexually Transmitted Diseases, working paper 1 (Oct. 2005).

the most the marginal resident – the resident that is indifferent between living the first or second state – is willing to give up (or pay) to live under the law. In the abstract, if you offered that individual the ability to live under the new law at a price once cent below the compensating differential, she would accept. If you charged her one cent more, she would say no thanks.

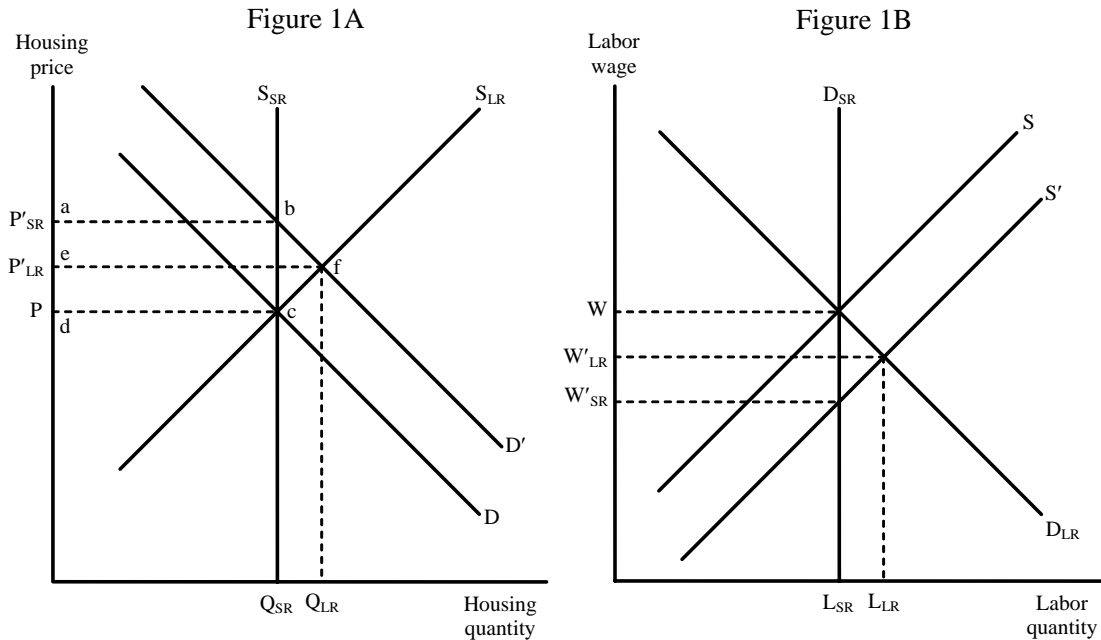
Although this illustration provides the intuition behind the housing and wages approach, it omits some important details. These details fall into three categories. First, how the equilibrating process works. Second, whether the process works with more complex laws. Third, how much information my marginal willingness-to-pay measure provides about the total welfare effects of a law.

A. How the equilibrating process works

The first bit of detail that might be useful is what happens to individuals who were living in the first state before the legal change. Where do they go? In the short-run, it is reasonable to assume that there are a fixed number of houses and jobs in each state. So for each resident from the second state that arrives, a resident from the first state must leave. But who stays and who leaves? The answer lies in the recognition that different people will value the new law differently. Some in the first state will value it more than some in the second state, and vice versa. If you group all the people of the two states together, it is the people who value the law the most that will end up in the first state. If they were in the first state before it passed the new law, they will remain. If they were in the second state, they will purchase houses and take jobs from first state residents who don't value the new law as highly as they do. Ignore jobs for a moment. Because houses are in limited supply migrants will have to bid at least as much as the ultimately marginal resident is willing to pay to live under the new law. If they bid less, there will be another person from state two that will be willing to pay more for each house in the first state. The marginal migrant to the first state, however, will only have to bid her valuation for the law. If she bids more, she will find there is more than one first-staters willing to sell their house. She will be able to lower her price and get at least one the houses. When we re-introduce jobs into the picture, the only change in the dynamic is that migrants will be bidding a combination of a higher housing price and a lower wage for space in the first state.²⁵

A second detail that would be helpful is why the value of a new law is capitalized only in housing and labor markets. Why not in the price of other products or services? Housing and labor markets are different than other product markets because houses and jobs must be locally supplied. A resident of a state needs a house and a job in that state. A house in another state or a job in another state will not do. Because the supply of local

²⁵ A simple numerical example can demonstrate how the equilibrium is restored. Suppose that residents A and B live in state one and C and D in state two before state one passes the new law. Assume A, B, C, and D value the new law at \$4, \$2, \$3, and \$1 respectively. A, whose value is 4, will remain in state one. C, whose value is \$3 will bid the pre-law price of a house plus \$3 for a house in state one. B, whose value is \$2, will accept the bid. (A will not accept because the law is worth more than \$3. If C bid only \$2 above the pre-law price, then B might not have accepted because she was indifferent. C could not simply offer B her house in state two, because that house – without the law – is worth less than C's house in state.) C will take the money from the sale and buy a house in state two at the pre-law price. She will have made \$2 in profits. D will remain put. Note that the new market price for homes in state one is \$2 higher than before.



housing and of local jobs is fixed in the short-run, the resulting increase in demand pushes up the price of local housing and lowers the wage that local jobs must pay. Now the resident also needs a car. But that car may be produced in another state and shipped to her. Because the resident requires a car in whichever state she resides, moving from one state to another does not change the aggregate demand and thus the price for cars. Finally, the resident needs a haircut or medical checkup. As with houses and jobs, traveling to another state is not an option. If a barber or doctor tries to raise her fees, she knows that there is a marginal barber or doctor in the other state that would be willing to buy her house and charge a lower fee. This threat will prevent the increase in service prices. The same cannot be said about houses because one cannot move a house from one state to another.

But what about in the long run? Won't higher prices encourage the construction of new homes? And what about jobs? Won't lower wages attract firms? Let's tackle new housing development first. It is true that in the long run, more houses can and will be built. This means that any given increase in the demand for housing in the first state will produce less of an increase in the price of housing in that state. (This is illustrated in Fig. 1A, which describes the effect of a change in demand when housing supply is fixed and the supply curve is vertical versus when new houses can be built and the supply curve is upward sloping. Note that the housing price increases less in the latter case.) The smaller increase in price does not mean that the change in housing prices does not fully capture how much the marginal resident values the new law. The reason is that the marginal resident has changed. When housing supply is fixed, the marginal resident was the one who took the last pre-existing house. When supply can increase, the marginal resident is the one who takes the last new house. Because more state two people move to state one in the long-run than in the short-run it is necessarily the case that the marginal mover in the long-run values the house less than the marginal mover in the short-run. If that were not the case, the long-run marginal mover would have out-bid the short-run marginal mover and taken her place in the short-run.

If the implication that the estimated marginal valuation of a law declines over time is worrisome, there are two palliatives. First, the long-run supply of housing does not depend on the law one is considering. So long as one compares two laws – say felon disenfranchisement rules with different durations or a disenfranchisement law and a parental notification law – after the same lag, the fall in marginal valuations due to new housing production will not alter the relative valuations of the two laws. Second, the fall in the marginal valuation of a law is greater than the fall in aggregate valuation of a law. The aggregate valuation of a law is the marginal valuation multiplied by the number of people who reside in state one after passage of the new law. (In Figure 1A, the aggregate valuation is “abcd” when supply is fixed and “cdef” when it is increasing.) Aggregate valuation falls at a lower rate because the new housing production that drives down marginal valuations also increases the number of people living in state one.

Let’s now turn to the issue of new jobs. Just as higher housing prices lead to new home construction, lower wages may attract more companies to state one in the long run. This will increase demand for labor and raise wages. This will tend to reduce the marginal valuation of a new law over time. The important thing to understand, however, is that this problem is simply the mirror image of the new housing supply problem. (See Figure 1B.) Therefore, the same analysis applies. The valuation falls because the marginal mover changes. In the long run the marginal mover is a former state two resident who values the law less. This does not affect the relative valuation of laws. And the change is less dramatic for aggregate valuations.

The details about the equilibrating process that remain are more technical. For example, do transactions costs – potentially including several hundred dollars in moving costs, realtor fees equal to five percent or more of a home’s value, and the search costs of finding a new job – limit the extent of capitalization? In my example they would, but in real life they likely would not. In my example, a resident from state two has to pay these costs to enjoy the benefits of moving to state one. If her valuation of the law is less than these transactions costs, she will not move. Since transactions costs can be significant, this means that a law with a smaller valuation will not affect housing prices or wages because it will not trigger migration. In real life, however, there are individuals, such as college graduates, who are already contemplating moving to a different state. If one of the candidate states adopts an attractive law, that state will attract such individuals even if the value of the law is less than the transactions costs of moving. The reason is that these individuals are already committed to moving and would have to pay the transaction costs of moving even if they did not move to the state with the new law.

Another technical detail is how the process works when, e.g., there is more than one working individual per household. In this case, state two residents stop moving to state one when the higher cost of a house plus the loss of wage for multiple members of the household is greater than or equal to the value of the law to all members of the household. The implication for my welfare measure is that individual-level valuation of a law must divide the housing price effect by the number of working members in a household.

Finally, how does the process handle renters as opposed to home-owners? Home owners pay for the right to remain in a home in perpetuity whereas renters pay for the right to remain in a home for a one-month period. The amount that home-owners are willing to pay for a law is the value they expect to draw from the law over the lifetime of

their home. The amount renters are willing to pay is the value they expect to draw over a one-month period. Future value is not captured in the rent because one-month's rent does not give the right to enjoy the law past the end of the month. To do that, the renter has to pay another month's rent. The best way to address this discrepancy when applying the housing and wages approach is to estimate separately the effect of a law on housing prices and on apartment rents. The price effect will provide an estimate of the long run value of the law. The rent will provide an estimate of the one-month long value of the law.²⁶ The rent may seem less useful because it provides only a snippet of a law's value. But the rent may have some useful features, such as avoiding problems with valuing laws where adoption is predictable. Such laws are reflected in housing prices before they are adopted. They are not, however, reflected in rents before they are adopted. The reason is that paying a rent before a law is adopted does not give a resident the right to enjoy the law after it is adopted without further fees.²⁷

B. More complex laws

So far I have focused on the case of a law that simply improves the living conditions of local residents. Does my thesis hold up in the case of more complicated laws? For my purposes, there are three types of "hard" laws:

1. Laws that affect production costs for business firms. Examples of the former include laws that increase penalties for recidivist criminals²⁸ or that require clean-up of hazardous waste. This category also includes laws that directly affect the demand for products, such caps on non-economic and punitive damages or laws that create new organizational forms, such as non-profits.²⁹ It also includes laws that directly affect the labor supply of residents, such as statutes that mandate a minimum level of maternity benefits³⁰ or greater parity between mental health and physical health benefits in health insurance plans.³¹

²⁶ The total welfare effect on a working individual (assuming one worker per household) is the sum of the wage effect plus either the rent effect or the house price effect. The welfare effect on a nonworking individual is simply the rent or house price effect. The investigator should not add both since no individual suffers both a rent effect and a house price effect. This strategy gives four different welfare measures: for workers and non-worker in rental units and in occupant-owned housing units.

Since wages are measured on an hourly basis, the wage effect must be adjusted to map onto the same time interval as rents or housing prices. With rents, the wage effect must be multiplied by the average number of hours worked per month. With housing prices, one must multiply the average number of hours worked over the lifetime of the house. This is obviously a more difficult calculation.

²⁷ An interesting possibility is that one can, by comparing the effect of a law on rents versus on housing prices, back out either the discount rate of residents assuming that a law's value is uniformly distributed over time or resident's prediction about how long a law will last given a discount rate.

²⁸ Joanna Shepherd, *Fear of the First Strike: The Full Deterrent Effect of California's Two and Three-Strikes Legislation*, 31 *J. Legal Stud.* 159 (2002).

²⁹ Cite to Hansmann 1980 Yale L. J. paper. The theory is that the non-profit form signals to consumers that the firm's products are of high quality. This should increase demand for the product.

³⁰ Jonathan Gruber, *The Incidence of Mandated Maternity Benefits*, 84 *Amer. Econ. Rev.* 622 (1994).

³¹ Jonathan Klick and Thomas Stratmann, *Subsidizing Addiction: Do State Health Insurance Mandates Increase Alcohol Consumption?*, working paper (June 2003).

2. Laws that affect the demand for housing. Examples include a higher homestead exemption³² or a more liberal divorce law.
3. Laws that benefit only pre-law, longtime residents of a state. An example is an amnesty for residents with overdue taxes.

I will address these in order, but before I do, let me preview my basic arguments. First, my measure does not aspire to capture the spillover effects of a law, i.e., the effect of a state one law on conditions in state two. Such spillovers do however make it harder for my measure to pick up similar domestic effects of a law. The size of this negative bias are roughly proportional to the size of the state, i.e., the underestimate is larger for larger states. Moreover, this bias is limited by the adjustment of production levels. The more responsive are consumers and producers to price, the larger those adjustments and the less the bias. Second, laws that change demand for housing do so because these laws provide benefits from changing residents' demand that equal the change in demand. In other words, the higher demand reflects value properly attributed to the law. In any case, rental markets (as opposed to home-ownership markets) do not suffer the bias from individuals who try to game laws by modifying their housing demand. Finally, my approach is inappropriate for laws that benefit only pre-existing residents of a state.

1. Laws that affect production costs

Let's start with laws that affect production costs. Without loss of generality, suppose that a law reduces the production costs of a given firm. This will have three possible consequences. First, the price of the firm's product will fall, which will benefit individual consumers. Second, the firm might make greater profits, which will benefit its individual owners. Third, the firm will increase output (or new firms will open in the state) to satisfy greater consumer demand, which will increase the demand for labor and individual workers' wages.³³ (By assumption, firms don't have preferences and therefore do not matter to welfare calculations. How firms affect individual utility, however, does matter to welfare.) How effective my metric is at capturing these welfare gains depends on whether consumption of the firm's products and ownership of the firm are local. A good example of a business with mainly local consumers and local owners is a small restaurant. An example of a non-local business is a car manufacturer which ships products and whose equity owners are scattered around the world. If consumption is local, migrants will want to move to the state in order to enjoy the benefits of the new law. The amount they are willing to sacrifice – in terms of higher housing prices and lower wages – is equal to the amount of lower prices they'll enjoy by residing in the state. The same logic applies to potential business owners if ownership is local. They will bid away the value of the additional profits from residing in the state.³⁴

³² Hynes, Malani and Posner JLE paper.

³³ See, e.g., Jonathan Klick and Thomas Stratmann, Does Medical Malpractice Reform Help States Retain Physicians and Does It Matter?, working paper (Nov. 2005); Daniel Kessler, William Sage, and David Becker, Impact of Malpractice Reforms on the Supply of Physician Services, 293(21) J. Amer. Med. Ass'n 2618-2625 (2005).

³⁴ This argument bears some resemblance to Posner's argument for how firms dissipate the rents from a government monopoly in their attempts to obtain that monopoly. Richard A Posner, The Social Costs of

What if consumption and ownership are not local? In that case the law provides a public good that is not geographically delimited. The product and ownership market-related benefits of the law are spread out across the country, and perhaps the globe. My proposed measure of value does not capture these benefits. But it doesn't seek to. Rather, its goal is to provide a measure of the local, i.e., within-jurisdiction, welfare effects of the law. This narrow scope does not insulate my measure from bias. That bias is proportional to the share of the total product or ownership market occupied by the state that adopts the law. To see this, start with the total non-delimited benefits of the law. The portion of those benefits that fall within the state enacting the law is the fraction of the product and ownership markets occupied by residents of that state. The portion of those benefits that fall outside the state is the fraction of the product and ownership markets occupied by non-residents. My measure cannot capture any of the non-delimited benefits of the law, but is not concerned with any benefits that accrue to non-residents. That means the only non-delimited benefits it cares about but cannot capture are those which accrue to residents. And that is proportional to the size of the state's share of the product and ownership market, which is in turn roughly proportional to size of the state's economy relative to the rest of the country or the world. In other words, the bias is large for California, but small for Georgia.

Importantly, this bias is limited by the extent to which higher productivity increases consumer demand for the product. That demand will increase demand for local workers. From this point on, then, the law can be treated as one which simply increases local wages. Residents of state two will flock to state one to get higher paying jobs. They will stop when their movement has bid up housing prices and partially bid down wages such that the higher housing costs offset the wage gains from residing in state one. In other words, any wage gain will be completely offset by a higher housing price.

Table 1 summarizes this analysis. If consumption and ownership of firms in the affected product market are local, a law's full effects are ultimately manifest in housing and labor markets. If consumption and ownership are not local, then a portion of the law's effect is spread between the consumers (F_1 , due to lower prices) and owners (F_2 , due to higher profits) in the affected product market that reside in (α) and outside ($1-\alpha$) the enacting state. It is the subset of these benefits that land in the enacting state ($\alpha F_1 + \alpha F_2$) that my measure fails to capture. The remaining portion of the law's effect is manifest through local housing and labor markets (F_3 , due to increased demand). These portions are affected by the following variables. The more competitive the product market, the more the law will lower prices (F_1) rather than raise profits (F_2). The larger the size of the enacting state, the larger the in-state effects (α) of the non-delimited law, and the larger the bias. Finally, the more sensitive consumer demand is to price, the larger is the portion of the law's effect that is conveyed via higher labor demand to the housing and labor markets.

Table 1. Distribution of benefits from laws that affect productivity by whether consumption and ownership is local (bias highlighted in grey).

	Local consumption and ownership	Non-local consumption and/or ownership	
		In-state effects (α)	Out-of-state effects ($1-\alpha$)

Monopoly and Regulation, 83 J. Pol. Econ. 807 (1975). My argument is simply that one can track individuals' attempts to get locational rents by examining the housing and labor markets.

Product market		αF_1 – not measured	$(1-\alpha)F_1$ – doesn't count
Market for ownership		αF_2 – not measured	$(1-\alpha)F_2$ – doesn't count
Housing and labor markets	1	$F_3 = 1 - F_1 - F_2$	0

Laws that affect product demand or labor supply can be analyzed just as laws that affect production costs. A law that increases consumer demand, for example, will benefit individuals in the same three ways as a law that lowers production costs. Although prices will rise, they will not rise enough to capture all the additional utility reflected in the increased demand, which will benefit individual consumers. The price rise and increased demand will raise the profits of individual owners. Finally, firms will respond with increased supply, which will increase demand for labor and thus attract non-residents with the prospect of higher local wages.³⁵ As before, the extent to which these effects are captured in housing prices and wages depends primarily on the extent to which consumption and ownership are local, and secondarily on the size of the enacting state and whether supply is more or less sensitive to increases in price.

2. Laws that affect demand for housing

The second type of difficult law is one that directly affects the demand for housing. For example, a more liberal homestead exemption may cause residents to hide more of their worth in homes to protect that worth from creditors. Or a divorce law that divides property according to fault might encourage a cheating husband to hide assets from his wife by, among other things, not investing in their house.³⁶ These are in fact opposite sides of the same coin. In neither case does the housing and wages method fail. Consider the exemption law first. There are two benefits of purchasing a house: a resident protects his assets from creditors and gets utility from having a house. The cost is that the resident is unable to purchase another product that provides greater utility than the house.³⁷ A person will only buy a house in response to an increase in the homestead exemption if the benefits outweigh the costs:

$$\text{Avoid loss to creditors} + \text{Value of house} \geq \text{Value of other product}$$

Now note three things. First, the value of the other product is greater than or equal to the price of that other product. This is the case with all purchases: the anticipated value of the product must be greater than or equal to the price of the product, or else the purchase is irrational. Second, the price of the other product is equal to the amount the resident bids on the house after the law. The reason is that the resident simply took money that was going to be used on the other product and spent it on the house. Third, before the law is passed, the most that the resident was willing to bid for the house is her value of

³⁵ A law that increases labor supply will rive down wages and lower the cost of production. This will benefit individual owners of firms and, as this reduction in costs filters into a lower price, the individual consumers of the firms' products. This demand effect will cause an increase in quantity supplied, which will raise demand for labor and thus the wage that enacting state firms offer.

³⁶ I thank Doug Lichtman for this example.

³⁷ This other expenditure could have been savings or investments, which are just proxies for future consumption.

the house. For the marginal consumer, the value of the house is equal to the pre-law price of the house. These points can be summarized as:

$$\begin{aligned} \text{Value of product} &\geq \text{price of product} = \text{post-law bid for house} \\ \text{Value of house} &= \text{pre-law bid for house} \end{aligned}$$

If we plug these equations into the first equation, we see that a rational home purchase must satisfy:

$$\text{Avoid loss to creditors} + \text{Pre-law bid of house} \geq \text{Post-law bid for house}$$

Or, to put it another way, the asset-protection value of the home purchase must be greater than the excessive amount the resident spent on the house:

$$\text{Avoid loss to creditors} \geq \text{Post-law bid for house} - \text{Pre-law bid of house}$$

But the asset protection value is only available because of the exemption law, and the change in bids is simply the change in price of housing. For the marginal resident these values will be identical, i.e., the protective value of the law is equal to the increase in housing prices. That is exactly my contention!

What about the divorce law case? How does my measure fare when, e.g., a cheating husband hides assets from his wife after the state adopts a law that considers fault when dividing marital property following a divorce? An obvious way to hide assets is for the husband to reduce his investment in the couple's house because that is an asset easily traced by the wife. This will reduce housing prices. My welfare measure counts this as a loss in value, though all that seems to have transpired is that wealth has been transferred from the wife to the husband.

But appearances can be deceiving. The transaction at issue is not merely a transfer from the wife to the husband, but also a loss of utility to both from having better housing. For purposes of illustration, assume that the typical cheating husband stashes \$100,000 that would have been spent on a house in a lock box, and that after he gets divorced – say a year from now – he plans to spend the money on another house. In that case, housing demand will fall to reflect that fact that the typical couple with a cheating husband is getting one less year of a \$100,000's worth of housing. If housing supply is fixed, the price drop will reflect exactly this loss of utility. The fall in price will not be the whole \$100,000 because the husband will reinvest the money in housing after the divorce.

What if the money is invested rather than stored in a lock box? Even assuming the alternative investment could not be traced by the wife, the investment, which would increase the husband's wealth after the divorce, merely exacerbates the wealth transfer. Either the investment gains would have been split between husband and wife under no-fault property settlement or kept by the husband under at-fault settlement. We do not expect that the investment opportunity changes the marginal propensity to consume housing, the reason being that it is available to the couple even if the husband does not hide wealth from the wife.

The possibility that supply of housing is not fixed or that the husband might spend less than \$100,000 on housing post-divorce similarly makes little difference. If supply falls with the decreased demand, price rises. This may reflect a slight increase in marginal valuation, but will not reflect a serious change in aggregate valuation since the higher price would be offset by lower quantity of housing. That the husband does not spend all his hidden cash on housing after the couple separated is only a problem if the wife has a higher marginal propensity to purchase housing with that money than the husband. In that case the wife's consumption would affect housing prices more than the husband's, though there is no reason to suspect that the cash transfer offers greater welfare to the wife than the husband. I suspect, however, that the gap in marginal propensity to consumer housing is a second- or even third-order effect.

If the reader remains skeptical, there is yet one more solution. Instead of looking to housing prices (in addition to wages) to gauge welfare, look to rents. The effect of the gaming by cheating husbands is much smaller among couples that rent because rental expenses are not split upon divorce. Unless one believes that welfare effects of a divorce law are actually different across property owners and renters, this will address any qualms about gaming among owners.

3. Laws that give exclusively benefit longtime, pre-law residents

The last category of hard laws includes those that only benefit individuals who lived in the state before the law was even anticipated. An example is a tax amnesty that absolves filers of penalties on past-due taxes. Clearly one had to be a pre-law resident of the state in order to have owed taxes. (Moreover, if the amnesty were announced before taxes were due, then it would not be an amnesty, but rather a law that lowers penalties on future non-payment of tax.) These laws are difficult for my measure because migrants cannot capture the benefits of the law. Therefore, they have no incentive to move to the enacting state, driving up housing prices and down wages.³⁸

These laws are reflected in a state's housing prices only to the extent that existing residents use the private proceeds from the law to increase their consumption of housing. This will drive up the demand and thus prices for domestic housing.³⁹ Yet this effect is limited by residents' marginal propensity to spend additional income on housing. If they spend, say, only 10% of additional income on better housing, then housing prices will pick up only 10% of the effect of the law. Yet even in this case, the effect will be offset by changes in the labor market. The benefits from the law will reduce residents' need to work to earn any given level of income. They may respond consuming more leisure, i.e., by working less. This will drive up wages, which count as lower value under my approach. One solution is to ignore wage effects when evaluating a category-three law. But even then the estimate will be too low because people don't spend every additional dollar of income on housing. The better response is not to use my approach for laws that only benefit longtime, pre-law residents.

³⁸ This is not the case if the law is anticipated. In that case migrants will move to the state before the law is enacted. All that is required to value the law is to examine housing prices after the law is anticipated but before it is passed.

³⁹ An implicit assumption is that, without some change in state two's laws, a state one resident will remain in state one if she is looking for a bigger house.

C. How informative is housing and wages measure?

My thesis is that the housing and wages approach provides a good approximation to the marginal resident's willingness to pay for a law. In this regard it is the best second-best measure of the local welfare effect of a law. In the previous subsections I explained why the measure was only an approximation; for example, it has difficulties with spillover effects and with laws that exclusively benefit prior residents of a state. In this section I want to explain why the method is not a first-best measure, but is a better second-best than the conventional method of valuing a law.

A first-best measure of welfare would tell us how much a law increased the utility of all residents in a jurisdiction.⁴⁰ The housing and wages approach does not attempt to provide this sort of information. Rather it provides information on how much the marginal migrant to a state values the law. This means that it ignores how much infra-marginal residents value the law. These residents fall into two categories: post-law residents that were also residents pre-law and individuals (other than the marginal migrant) who moved to the enacting state post-law. Pre-law residents who remain in the state all value the law at least as much as the marginal migrant. Otherwise they would have sold their property to the marginal migrant and been better off in another state with the cash proceeds and no law. Even if they did not own a home, they would have been better off leaving because the rents would be sufficiently lower and the wages sufficiently higher in other states to make the law not worth these lost opportunities. All post-law migrants must also value the law at least as much as the marginal migrant. If they valued the law less than the marginal migrant, the additional cost of housing and the lower income offered in the enacting state would outweigh the private benefits they derived from the law. The net implication is that the housing and wages approach offers a lower bound on the first-best measure of welfare.⁴¹

That said, it is a better second-best measure than its competitors. These include not only the conventional approach to valuing a law, which focuses on how the law affects proximate behavior, but also other willingness-to-pay measures, such as the number of post-law migrants to a state, the effect of a law on GDP or stock prices, the tolerance for longer commutes to work among post-law residents, and surveys of willingness-to-pay.⁴² Comparisons of different marginal willingness-to-pay measures can be found elsewhere in the literature.⁴³ I will only summarize my main concerns with these alternative measures and focus on comparing the housing and wages method to the conventional approach for valuing a law.

⁴⁰ There is the question of whether one is interested in the welfare of residents within the state before the law or after the law. Given that homeowners who leave the state capture some of the benefits of the law as proceeds from home sales, and that all post-law migrants to the state value the law more than the residents they displace, the ex-post measure provides some information on the ex-ante measure. This information is confined, however, to the set of outward migrants.

⁴¹ To obtain this bound, simply multiply the marginal migrant's willingness to pay by the number of residents in the state, i.e., multiply the increase in housing prices by the number of houses, the increase in rent by the number of rental units, and the increase in wages by the number of workers. The sum is a lower bound on aggregate welfare effects.

⁴² Cites to relevant papers on method or exemplary papers.

⁴³ Cites to handbook chapters.

The advantage of the housing and wages approach over counting the number of migrants to the enacting state is that the latter only tells you the number of people that prefer a law, not the extent to which they prefer a law. The problem with the GDP/stock price approach is that it does not capture the effect of laws that do not affect productivity. The issue with commuting-time approach is that in the short-run it may find little effect because the number of homes and the number of jobs is fixed. Moreover, the magnitude of the effect depends on where new homes are built or new factories are located. Better urban planning could lower the valuation of a law under this approach. Finally, surveys of willingness-to-pay are wholly subjective and provide respondents with little incentives to provide accurate answers.⁴⁴

The more serious challenge to the housing and wages approach – or to be honest, the more serious target of the proposed approach – is the conventional method of valuing laws. This approach looks at the effect of a law on proximate behavior. For example, the effect of truth-in-sentencing laws on violent and property crime rates⁴⁵ or the effect of no-fault and compulsory auto insurance laws on traffic fatalities.⁴⁶ This approach does have some benefits, but these are generally outweighed by the advantages of my proposed approach.

One benefit of the conventional approach is that it provides a truly objective measure of the effects of a law. In contrast, because migration is driven by individuals' perceptions about the effect of a law, the housing and wages approach only captures an objective manifestation of individuals' subjective valuations of a law. Of course, welfare is driven by subjective valuation; otherwise the expressive effect of a law has no value. Moreover, subjective valuations are based on residents' observations and thus reflect objective realities. One can be as confident in the objectivity of the housing and wages approach as one is confident in rational expectations. But the real tradeoff on this issue – the one that tips the scales in favor of the housing and wages method – concerns the scope of effects that the two approaches capture. The conventional approach only captures objectively those effects that investigators can identify and measure. It does not capture, for example, unpredictable benefits or benefits that are difficult to observe or quantify. The housing and wages approach can.

Unpredictable benefits can be very important. A good example is the connection between abortion and crime. For quite some time, people did not examine the effect of permitting legal abortions on crime rates. (While I do not mean to justify abortions rights on this ground, it would be hard to contend that the fall in crime is not a benefit in some sense.) The reason was that until a few really creative scholars thought about it, the theoretical connection was not made. An advantage of the housing and wages approach is that creativity is not required for abortion rights to be credited for their effects on crime. Lower crime would have driven up housing prices. The fact that this would have occurred in states with more liberal abortion rights means that housing prices would be positively correlated with more liberal abortion rights.

⁴⁴ Cite Philipson/Malani, J. Econometrics paper.

⁴⁵ Joanna M. Shepherd, Police, Prosecutors, Criminals, and Determinate Sentencing: The Truth About Truth-In-Sentencing Laws, 45 J. Law & Econ. 509 (2002).

⁴⁶ J. David Cummins, Richard D. Phillips, and Mary A Weis, The Incentive Effects of No-Fault Automobile Insurance, 44 J. Law & Econ. 427 (2001); Alma Cohen and Rajeev Dehejia, The Effect of Automobile Insurance and Accident Liability Laws on Traffic Fatalities, 47 J. Law & Econ. 357 (2004).

Benefits that are difficult to measure include expressive benefits and enforcement costs. An example is Megan's law, which requires sex offenders to register with a state when they move there.⁴⁷ Such a law might deter sex offenders from moving to a state or allow the state to assign police to monitor offenders, which in turn will reduce sex offenses. These might plausibly be estimated via the conventional approach.⁴⁸ But a registration law might also make other residents less anxious about sex offenders in their community or allow these residents to express their outrage against sex offenders. These effects are very hard to quantify and employ under the conventional method; I know of no variable that captures placebo effects or expressive values.⁴⁹ On the other hand the law might be very costly to administer in obvious and non-obvious ways. Registration may require costly computer systems and public notices. Enforcement of the registration obligations may eat up scarce police resources; so might the fact that knowing one's neighbor is a sex offender might cause one to file more police complaints about suspicious behavior by the neighbor for any given level of sex offenses he commits. Any criminologist will tell you that all these costs are very hard to measure directly. But that is not a problem for the housing and wages approach. Housing prices and wages will capture placebo effects, expressive values and enforcement costs because people take these factors into account when they move.

Another benefit of the conventional approach is that it can identify the pathway through which a law operates. For example,⁵⁰ it has been reported that wrongful-discharge laws have had a small but significant effect on the level of employment.⁵¹ But perhaps more significant is the fact that they have changed the nature or terms of employment, by causing an expansion of employment at temp agencies.⁵² It is the conventional approach that was used to tease out these effects. The housing and wages approach, in its simplest form, would simply lump these different effects together. The offsetting advantage, however, is that my approach provides a better estimate of welfare. While the conventional approach tells us that wrongful-discharge laws may reduce the level and terms of employment, it does not tell us how important those effects are to welfare. What are people willing to pay for a one to two percent decrease in unemployment, given that they are also more likely to be working at a temp agency without health benefits? Who knows? But with the housing and wages approach one can

⁴⁷ Cite future Shepherd work on this.

⁴⁸ See, e.g., Elizabeth Lovell, *Megan's law: does it protect children? A review of evidence on the impact of community notification as legislated for through Megan's law in the United States. Recommendations for policy makers in the United Kingdom* (2001). For a summary of findings, see http://www.nspcc.org.uk/Inform/Research/Findings/MegansLaw_asp_ifega26197.html. See also Thomas J. Miles, *Community Notification and the Sentencing of Sex Offenders*, Working Paper, University of Chicago, September 2003.

⁴⁹ Perhaps one could use levels of happiness from the General Social Survey, but that is very rough measure and the outcomes are hard to interpret. For example, what does mean it for welfare to find that people are more likely to say they are "very happy" as opposed to merely "happy" in states with Megan's law? Cite Lakdawalla/Malani paper on effect of smoking on mental health.

⁵⁰ [Find better example with costs and benefits; e.g., guns laws' effects on accidents v. deterrence, but where outcomes are qualitatively different, not deaths on deaths.]

⁵¹ See survey in David H. Autor, John J. Donohue III and Stewart J. Schwab, *The Employment Consequences of Wrongful-Discharge Laws: Large, Small, or None at All?* 93(2) *Amer. Econ. Rev. Papers & Proc.* (2004).

⁵² David Autor, *Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing*, 21(1) *J. Labor Econ.* (2003).

answer this question. It is possible to identify the amount that the marginal resident is giving up to have the protection of a wrongful discharge law by examining how much more she is willing to pay for housing and how much less she is willing to earn.

What's more, this approach can be combined with the conventional approach to determine both welfare effects and pathways of causation. For example, if you want to discover the welfare implications of the effect of a wrongful-discharge law on temporary employment, simply regress housing prices and wages once on the wrongful-discharge law, and once on the law and the temporary employment level in the state. The coefficient on the law in the first regression would provide an estimate of the welfare impact of the law, including all effects of the law. The coefficient on the law in the second regression would provide an estimate of the welfare impact of the law excluding its effect on temporary employment. The difference in the coefficients on the wrongful-discharge law across the two regressions would provide an estimate of the welfare implications of changes in temporary employment.

A weakness that the housing and wages approach has vis-à-vis the conventional approach is that it implicitly weights an individual's welfare in proportion to her wealth.⁵³ The reason is that it relies on a market measure of value – housing prices – and market prices weight individuals' preferences in proportion to their income. To see this, suppose two individuals with identical income have the same valuation (and thus bid) for a house. If the first individual is magically given a small amount of additional income, she will raise her bid for the house for no other reason than that she has more disposable income with which to bid. Because she will be able to outbid the second individual, her bid will determine the house's ultimate sale price. Therefore, any approach that employs housing prices to estimate the value of a law will weight wealthier residents more.

But this weakness is surely overcome by one of the primary benefits of housing and wages approach: the ability to compare different types of laws. Whereas the conventional approach would have trouble comparing, e.g., a concealed weapon law and a school-choice law (how would one compare a law that affects mortality rates with a law that affects test scores?), my approach would have no difficulty doing so. The reason is that my approach examines the effect of all laws on the same two outcomes. This permits a direct comparison of laws that have entirely different objectives, let alone pathways. A concealed weapon law would be better for welfare than a school-choice law if on net it raised housing prices and lowered wages more.

A difficulty might arise if the distributional effects of the two laws differed, i.e., if the first law helped the poor more than the second. Even though the net effect of the first law might be greater than the second given equal weighting of each person's utility, a naive application of the housing and wages approach might find that the second law was better than the first. Where one suspects important distributional consequences, however, there is an adjustment that can be made to account for these effects. Specifically, the investigator should divide the sample of homes and jobs into bins representing higher and lower wealth populations. For example, single family homes or apartments with more than five rooms have higher income residents and jobs in management or professional services tend to have higher wealth workers. Then the investigator should estimate the

⁵³ A more technical way to put this is that the housing and wages approach implicitly assumes each person's weight in the social welfare function is proportional to her lifetime wealth given complete credit markets.

effect of the laws separately on each bin. If there is a distributional effect that favors the wealthy, the law should increase the housing prices or rents and lower the wage of individuals in high-wealth bins more than those of individuals in low-wealth bins.

Finally, there are two shortcomings of the housing and wages approach for which there are no related offsetting benefits. All that can be argued is that these shortcomings are second-order in terms of magnitude. The first flaw is that the method does not capture the effect of laws on individuals who are not in the housing or labor market. This includes, e.g., prisoners, members of the armed forces, and children. If these individuals are residing in state two when state one passes a law they like, they cannot move to show their preference for it. Of course a child's parents may consider her welfare and move, and we can try to rationalize that prisoners don't deserve to be in the social welfare function. But at the end of the day, these folks are under-counted by my method.

The second flaw is that the measure fails to control for what economists call income effects. Suppose state one passes a law that makes individuals happier. After the law, housing prices will rise and wages will fall. Although migrants to state one will have higher utility, they will suffer a loss of disposable income. That loss will have a second-order effect on consumption of housing and leisure. Because consumption of housing generally rises with income, i.e., housing is a normal good, the feedback effect will reduce demand for housing and thus the price of housing. The effect on wages is unclear. On the one hand, a decrease in wages will cause a substitution towards more productive uses of time, namely leisure. This will tend to increase wages because it lowers labor supply. On the other hand, the initial decrease in wages will reduce consumption of leisure, which is also a normal good. That will increase labor supply and thus lower wages. The problem is that the housing and wages approach captures these feedback effects, even though they do not reflect value from the law, but rather residents' adjustments to drawing value from the law. The consolation is that the adjustment effects on housing prices and wages will be minor relative to direct-value effects because they are mediated by residents' marginal propensity to consume housing and leisure. These propensities are significantly less than one, i.e., a dollar increase in income will produce much less than a dollar change in expenditure on housing and leisure. This is not just because the budget constraint (can't buy a \$2 toy with just \$1 dollar), but because studies by economists have shown this to be the case.⁵⁴ To summarize, while the income effect will make the housing and wages approach a less accurate approximation of the marginal resident's willingness to pay for a law, the additional error is not very large.

III. Empirical example.

I imagine that a significant source of skepticism about this project is concern that housing price and wage data are too noisy to permit identification of the effect of a legal change. The purpose of this section is to ease that concern. What follows is a preliminary evaluation of a series of laws according to the housing and wages method. Each of the laws has previously been evaluated employing conventional methods and I will reference prior studies in order to highlight the potential, practical contributions of my method. Readers should, however, not view my findings as definitive. Because this paper is an early work-in-progress, my empirical analysis has not undergone the sort of

⁵⁴ Cites.

double-checking that is required by the time of submission for publication. In other words, do not take the signs of coefficients too seriously.

A. Data

Housing prices. Data on housing prices were drawn from the national version of the American Housing Survey (AHS). This survey is conducted by the Department of Housing and Urban Development (HUD). It includes roughly 50,000 housing units per year. The survey was conducted annually from 1973 – 1981, and bi-annually after that.⁵⁵ My sample excludes the year 1973 because the data on numerous covariates are missing for that year.

The AHS-National survey provides three measures of housing price. For housing units that are owned by the occupant, one measure is the occupant's subjective estimate of a housing unit's value.⁵⁶ Another is the price at which (and the date on which) the occupant acquired the housing unit. For housing units that are rented by the occupant, the AHS reports the monthly rental price. Presently, my analysis only employs the subjective owner's assessment of value as a proxy for housing prices. I do not use purchase price because the survey does not report housing characteristics for the year a property was acquired, but rather for the year that the occupant was surveyed.

The housing characteristics I extract from the AHS are those typically employed in environmental or urban economic studies that attempt to value environmental amenities such as clear air or urban amenities such as a professional sports stadium.⁵⁷ Occasionally I omit important variables, such as lot size or square footage, when the cost they impose in terms of reduced sample size outweigh what I subjectively assess to be their explanatory power. The latter assessment is significantly influenced by the existence of alternative variables, such as number of rooms and height of building, that have fewer missing observations and are workable proxies for the initial variables.

Wages. Data on wages were drawn from the March version of the Current Population Survey (CPS). The CPS gathers data on roughly 200,000 workers per year. These workers are interviewed for once a month for four consecutive months, then left alone for eight months, then interviewed again once a month for four consecutive months. Only twice, in the fourth and eighth interviews are workers asked about their hourly or weekly wages. If the fourth or eighth interview happens to occur in March, it will be included in my current sample.⁵⁸ For some reason, some workers are asked their hourly wage and others are asked their weekly wage. The worker characteristics I extract from the CPS

⁵⁵ In interceding years, the HUD conducts the so-called MSA (for metropolitan statistical area) version of its survey. Although both versions confine their sample to MSAs, the MSA version focuses on a more narrow set of the most populous MSAs so as to enable more precise inferences about the nature of housing in those MSAs.

⁵⁶ Prior to 1984, this assessment is recoded into \$5,000 bins. I assign to each house a value equal to the midpoint of the bin to which the owner's subjective valuation is assigned.

⁵⁷ Gerald Carlino and N. Edward Coulson, Compensating differentials and the social benefits of the NFL, 56 J. Urb. Econ. 25-50 (2004).

⁵⁸ That means that my sample size, ignoring missing observations on relevant covariates, should be $2 \times 200,000 \times 1/12$. In a future iteration of the paper, I will employ the results of all fourth and eighth interviews, regardless of the month in which they fall. This will increase the sample size by roughly 12 times.

are those typically employed in labor economics studies examining disparities in wages. Because the CPS only asked workers about their weekly or hourly wage starting in 1979, my wage sample starts that year. Because certain crucial worker characteristic variables have not been released for 2004 data, my wage sample ends in 2003.

Laws. In order to facilitate a comparison of the housing and wages approach with the conventional approach, I gather data on laws from prior studies that employ the conventional method. These include data on:

- Medical malpractice-related tort reforms from Jonathan Klick and Thomas Stratmann, “Does Medical Malpractice Reform Help States Retain Physicians and Does It Matter?,” working paper (Nov. 2005)
- Tort reforms from Paul Rubin and Joanna Shepherd, “Tort Reform and Accidental Deaths,” working paper (2005)
- Abortion rights laws from Jonathan Klick, “Mandatory Waiting Periods for Abortions and Female Mental Health,” *Health Matrix* (forthcoming, 2006); and Jonathan Klick and Thomas Stratmann, “Abortion Access Laws and Risky Sex Among Teens: Parental Involvement Laws and Sexually Transmitted Diseases,” working paper (Oct. 2005)
- No-fault automobile insurance laws from RAND
- Divorce laws from Leora Friedberg, “Did Unilateral Divorce Raise Divorce Rates: Evidence from Panel Data, 88 *Amer. Econ. Rev.* 608 (1998)
- Capital sentences and executions from Joanna Shepherd, “Murders of Passion, Execution Delays, and the Deterrent of Capital Punishment,” 33 *J. Legal Stud.* 283 (2004)
- Health insurance mandates from Jonathan Klick and Sara Markowitz, “Are Mental Health Insurance Mandates Effective?: Evidence from Suicides,” *Health Econ.* (forthcoming, 2006); Jonathan Klick and Thomas Stratmann, “Subsidizing Addiction: Do State Health Insurance Mandates Increase Alcohol Consumption?,” *J. Legal Stud.* (forthcoming 2006); and Jonathan Klick and Thomas Stratmann, “Diabetes Treatments and Moral Hazard,” Florida State University Law and Economics Working Paper No. 05-21 (Aug. 2005)

The law data are merged with housing and wages data by state. Since the housing data are sorted by metropolitan statistical area (MSA) and an MSA may cover more than one state, I matched MSAs to states based the which state has the largest population within the MSA.⁵⁹

Table 2 provides summary statistics for the housing and wage data sets. Statistics are computed separated for each dependent variable because there may be different numbers of homes and workers with non-missing observations on the dependent variable in the AHS and the CPS data sets. Figures 2 – 7 graph the number of states that have each type of law by year.

B. Empirical model

⁵⁹ In a future iteration of the paper I will regress housing prices and rents against the laws in primary and secondary states by population.

The empirical model I employ resembles the standard model in empirical law and economics studies:

$$y_{ijt} = \beta \mathbf{X}_{ijt} + \mu_j + \lambda_t + \gamma \mathbf{t}_j + \alpha \mathbf{L}_{jt} + \varepsilon_{ijt}$$

where i , j and t index individuals, jurisdictions, and time, respectively; y is housing price, rent, hourly wage or weekly wage; \mathbf{X} is a vector of housing or wage characteristics as appropriate; μ_j is an jurisdiction-fixed effect; λ_t is a time-fixed effect; \mathbf{t}_j is a vector of jurisdiction-specific time trends; and \mathbf{L} is a vector of state law variables. The jurisdiction for house price and rent data is the MSA; it is the state for the CPS data. This model employs a differences-in-difference estimator to identify the effect of state laws on housing prices or wages.

C. Interpretation of preliminary results

Tables 3 – 10 presents the results of my preliminary regression analyses. Before I discuss my findings, let me comment on the problem of endogeneity, i.e., the problem that correlation might pick up the effect of welfare on laws rather than laws on welfare. Although it may appear that I have a plausible argument for why my analysis does not suffer endogeneity (surely the laws I examine were not adopted because of shifts in housing prices or wages), that impression is incorrect. Whatever causes conventional analysis to suffer endogeneity bias, also causes my approach to suffer endogeneity bias. The extent of bias is proportional to how sensitive housing prices and wages are to the outcomes studied in conventional analyses.

For example, a concern with studies of the effect of the death penalty on the murder rate is that a higher murder rate may cause a state to ramp up executions. This would lead to a positive correlation between murders and executions, which could mask the predicted negative effect of executions on murders due to incapacitation and deterrence. My analysis of executions is not immune to this problem. A higher murder rate will both depress housing prices and encourage the state to execute more people; the result is a negative correlation between housing prices and executions. But that could mask the deterrence effect, which should increase housing prices. The more sensitive housing prices are to executions, the greater the possible offset.

Fortunately, there is a partial solution. If the investigator were to add the conventional outcome that raises concerns about the endogeneity of a law as an explanatory variable to the housing and wage regression analysis, she would be able to estimate the effect of the law on welfare exclusive of endogenous channels. This is valuable so long as one does not suspect that welfare effects through non-endogenous channels are negatively related to welfare effects through endogenous channels. So, for example, if housing prices were regressed upon executions and the murder rate, then the effect one would find would capture the effect of executions on welfare due the effect of executions on outcomes other than murder.

Table 3 presents the results of my analysis employing tort reform variables employed by Jonathan Klick and Thomas Stratmann in a paper that examines the effect of such reforms on the supply of physicians in a state. Focusing on statistically

significant results, Klick and Stratmann found that caps on non-economic damages and the elimination of joint and several liability tended to increase physician supply.⁶⁰ Given that greater physician supply is thought to be good for welfare, one would expect that these reforms raise housing prices and lower wages. I find, however, that these reforms do not have statistically significant effects on housing or wages. This could be because, once one considers the implications of these reforms on outcomes other than physician supply, the effects are not clearly positive for welfare. More interestingly, I find that caps specifically on damages in medical malpractice cases tend significantly to lower rents, housing prices, and wages. Looking just at rents and weekly wages, and accounting for the fact that on average there are 1.05 employed occupants per rental unit in my sample and that the average month has 4.16 work weeks,⁶¹ it appears that the wage effect offsets the rental effect such that the net effect on welfare is a gain of \$27.30 per worker. (Non-workers, however, tend to lose roughly \$47 per person under the law.)

The skeptical reader might wonder whether the rental effect is too large to be credible. Forty-seven dollars is nearly half the value I estimate for a full bathroom. I avoid focusing on the housing price effects of medical malpractice caps for a similar reason: a \$12,000 effect appears too large given that the average home in my sample is worth \$99,700.⁶² I suspect a part of the problem is that my measure of housing values is not very precise. It is the owner's own assessment of the value of his or her property. (For this reason, I do not take too seriously my finding that eliminating the collateral source rule, which bars defendants from introducing evidence to show that the plaintiff has already been compensated for part of her injuries by, e.g., a health insurer, increased housing prices.) But my rental data is much more objective; while based on self-reports, those reports are of a recurring monthly payment almost surely known with precision by the occupant. Another explanation of the large effects might be that my tort reform variables are picking up the effects of other laws for which I do not control. I will explore this possibility in Table 10.

Table 4 presents results that employ tort reform data from Paul Rubin and Joanna Shepherd's recent working paper on the effects of these reforms on non-motor vehicle accident rates. Their conclusion – which they stress to me is preliminary – is that, with the exception of reforms to the collateral source rule, tort reforms tend to lower death rates.⁶³ I would expect, then, that such reforms should raise rents and lower wages. I find the opposite. Caps on punitive damages tend to lower rents and caps in product liability cases tend to raise hourly wages. The latter effect might be explained by the fact that businesses prefer to locate in states with a less aggressive tort environment; this raises the demand for and thus price of labor. Because firms do not count in the social welfare function (only individuals do), this explanation does not imply that caps in product liability cases improve welfare. However, the possibility that tort reform could have positive spillover effects in the form of improving profits of business owners who

⁶⁰ They also find that reforms requiring doctors pay damages in the form of periodic payments rather than a lump sum actually reduced physician supply. They, as I, were puzzled by this finding.

⁶¹ $4.16 \text{ weeks/month} = 50 \text{ weeks} / 12 \text{ months}$.

⁶² Since the housing sample starts in 1974 and the tort reform subsample starts in 1980, it is likely that the average price tort reform sample is higher. That would make the medical malpractice effect more credible.

⁶³ The theory they suggest is that tort liability makes new products, which include greater tort insurance but are also safer, more expensive. Therefore, such liability discourage consumers from buying newer, safer versions of goods.

need not reside in the reform state, suggests that my estimates should only be interpreted as a local measure of welfare.

Table 5 examines the effect of laws that regulate access to abortion. The data on these laws was taken from papers by Klick and Stratmann. In one, Klick finds that mandatory waiting periods tend to lower female suicides, while restrictions on Medicaid funding of abortions tends to increase female suicides. Perhaps the theory is that waiting periods give a woman the opportunity for contemplation before taking an action that may have lingering, harmful psychological effects; but once a woman makes a considered decision to have an abortion, blocking that abortion with restrictions on funding may have its own lingering, harmful psychological consequences. Whatever the rationale, my findings are roughly consistent with the Klick study. I find that housing values rise with mandatory waiting periods and fall with restrictions on funding. I do not put very much weight on these results: the magnitudes are implausible and my housing values are subjective. Moreover, it appears that when a waiting period law is enjoined, both rents and housing values rise.

In a separate paper with Stratmann, Klick finds that parental notification laws reduced gonorrhea infections among teenage females. While some of my findings are consistent with that paper (notification laws raise rents by \$35), others cast doubt upon it (rents rise \$14 when notification laws are enjoined). Perhaps my inconsistent findings can be reconciled either by adding them, which would suggest a net positive effect of roughly \$21, or by hypothesizing that people prefer these laws until courts provide new information that the laws impede the right to privacy under the state or federal constitution.⁶⁴

Table 6 reports the effects of no-fault automobile liability laws. The data were acquired from RAND. Studies by David Cummins, Richard Phillips, and Mary Weis and by Alma Cohen and Rajeev Dehejia have found that no-fault liability laws tend to increase traffic fatalities (theoretically, for the same reasons that tort liability might reduce accidents). When I formulate no-fault laws as a 0/1 indicator variable (one if there is a no-fault law), I find no significant effects on rents or wages. However, in an unreported analysis, I do find significant negative effects on rents when I formulate the law as the threshold below which accident costs are allocated without regard to fault. (This is consistent with results reported in Cummins et al.)

Table 7 examines the effect of different types of divorce law. The data are drawn from Leora Friedberg's previous work, which found that unilateral divorce, with or without property division based on fault, led to greater numbers of divorces.⁶⁵ Betsey Stevenson and Justin Wolfers used these same data to determine the effect of divorce laws on domestic violence. They found that unilateral divorce laws significantly reduced husband-on-wife violence and wife-on-husband severe violence. Stevenson and Wolfers' results, if not Friedberg's result, suggests that unilateral divorce should raise rents and lower wages. I find the welfare effects are mixed. Wages fall (by \$4-18 per week), but so do rents (by roughly \$18, regardless of whether property division remains subject to

⁶⁴ I also find that notification laws increase wages by \$13 per week. I have difficulty explaining this result given that rents also rise. The obvious way to fill the gap is to suppose that firms enter states with notification laws. But I have no plausible explanation for why this would be.

⁶⁵ Wolfers subsequently found that much of Friedberg's findings were short-term effects. In the long-run, unilateral divorce does not appear to increase divorce rates. [Cite 2003 working paper.]

fault). The overall welfare effect is positive and significant for no-fault property division, but virtually zero for at-fault property division. What's more, falling rents suggest people are leaving states with unilateral divorce, but the wage effect suggest that firms are staying. That is hard to explain (why should firms care about divorce laws?). Perhaps there is a shift in the composition of workers – low wage workers leave, high skilled remain. It remains for me to test this hypothesis by computing the aggregate wage effect of the unilateral divorce law. That effect is the change in total wage bill – wages times workers – before and after the law.

Table 8 takes on a politically more-controversial law: the death penalty. It employs death sentence and execution data from a recent study by Shepherd. That study found that increases in the probability of a capital sentence and of execution lowered the murder rate by 2 and 4 deaths, respectively, per million population (per month). That study, and others, were criticized in a recent paper by John Donohue and Wolfers.⁶⁶ A central theme of that paper is that there are too few executions and murders and so much variation in the latter that studies of the death penalty are likely finding spurious correlations. An important advantage of my approach is that it is less susceptible to this criticism. The reason is that I offer many more observations (homes and workers) that are given treatment (death sentence and execution rates) and I have many more explanatory variables to explain away this variation.⁶⁷ The implication is that I place many more hurdles – other explanatory variables – in the way of a correlation between capital punishment and my welfare measure. If I do find a correlation, it is much less likely to be spurious.

The results of my analysis suggest that executions tend to slightly increase rents and weekly wages. (Note that, due to time constraints, my coding of capital punishment differs from Shepherd's. She normalizes capital sentences and executions by the number of murders. I have not yet accumulated the relevant murder data so my coding is simply the number of capital sentences and executions per state.) My calculation of the net effect suggests a \$0.25 increase in welfare per execution. This is not a lot, but not surprising given the small decrease in the probability of death from murder implied by Shepherd's numbers. Another caveat that is warranted is that, although I include fixed effects and jurisdiction-specific trends to control for omitted variable bias, those controls are likely incomplete. Future analysis will include controls such as the local unemployment rate and the demographic composition of the neighborhood.⁶⁸

Table 9 considers the effect of health insurance mandates on local welfare. The two mandates studied are requirements that health insurance companies provide coverage for diabetes therapies and that such companies provide coverage for mental health problems on par with their coverage of physical health problems. The latter are called mental health parity laws. In two separate papers, Klick and Stratmann find that diabetes

⁶⁶ Cite Stanford L. Rev. paper.

⁶⁷ The latter, in particular, is important. Death penalty investigators could simply add observations by making each county or individual in a state be a unit of observation. But they run up against the challenge of findings explanatory variables at the unit level. Without this, they have artificially increased their power by adding sample size without further information.

⁶⁸ Another important improvement is to interact the criminal sanction with the location of a home or worker. If one finds that criminal sanctions have bigger effects on welfare in neighborhoods with greater crime, then one can be more confident that the housing and wages measure is actually picking up the welfare effect of the criminal sanction.

mandates increased the body-mass index (BMI) of diabetics and that mental health parity laws increased alcohol consumption. They interpret these as examples of moral hazard induced by mandatory insurance. My analysis finds that diabetes mandates increased rents but lowered housing values. (Mental health parity laws had no significant effects.) One interpretation is that the rent regression is more trustworthy and the mandates appear to have increased welfare. The reason is that moral hazard is only clearly a cost to the principal; it is a short-run and perhaps even a long-run gain to the agent. Another interpretation is that there are distributional consequences of mandates. Occupants in rental units benefited while homeowners suffered. The reason could be that the former are poorer and sicker and were likely to have benefited from medical care whose cost was borne predominantly by the latter, who are richer and healthier. The explanation that is most appealing will depend on how skeptical the reader is of my measure of home values.

Table 10 is a first stab at the problem of spurious correlation in studies that examine only one type of law. The risk is that, because laws are enacted in groups, one law might appear to have an effect on housing prices that is really due to another, unaccounted-for law. One suspects this possibility when a law has unbelievably large effects on a given outcome. An example is medical malpractice caps in Table 3. In order to check whether this is a serious concern, I estimated a regression model that includes Rubin and Shepherd's tort reform data, no-fault auto liability laws, death sentences and executions, and health insurance mandates as treatment variables all at once. To facilitate comparison with estimates from regression models that examine each set of laws separately, Table 10 reproduces the appropriate regression results from prior tables in its first four columns. The results of the combined regressions are in the last four columns. The most optimistic finding (from a methodological perspective) is that caps on punitive damages reduce rents and executions and diabetes mandates increase rents in both separate and combined regressions. Moreover, caps in product liability cases tend to increase hourly wages across the models. This should increase one's confidence that the specified tort reforms reduce local welfare and that executions and diabetes mandates increase local welfare.

IV. Conclusion (to be completed)

Implications of normative analysis:

- Diminishing returns to law. Effect of Tiebout sorting.
- Distributional effects – to owners of housing.
- Distributional effects – to non-residents.

Future research:

- Controls for adjacent state laws
- Better controls for endogeneity
- Demonstration of distribution effects, enforcement costs
- Better housing data (odd year AHS-MSA data)
- Appendix with advanced version of Roback model

Figure 2: Tort reforms (Klick)

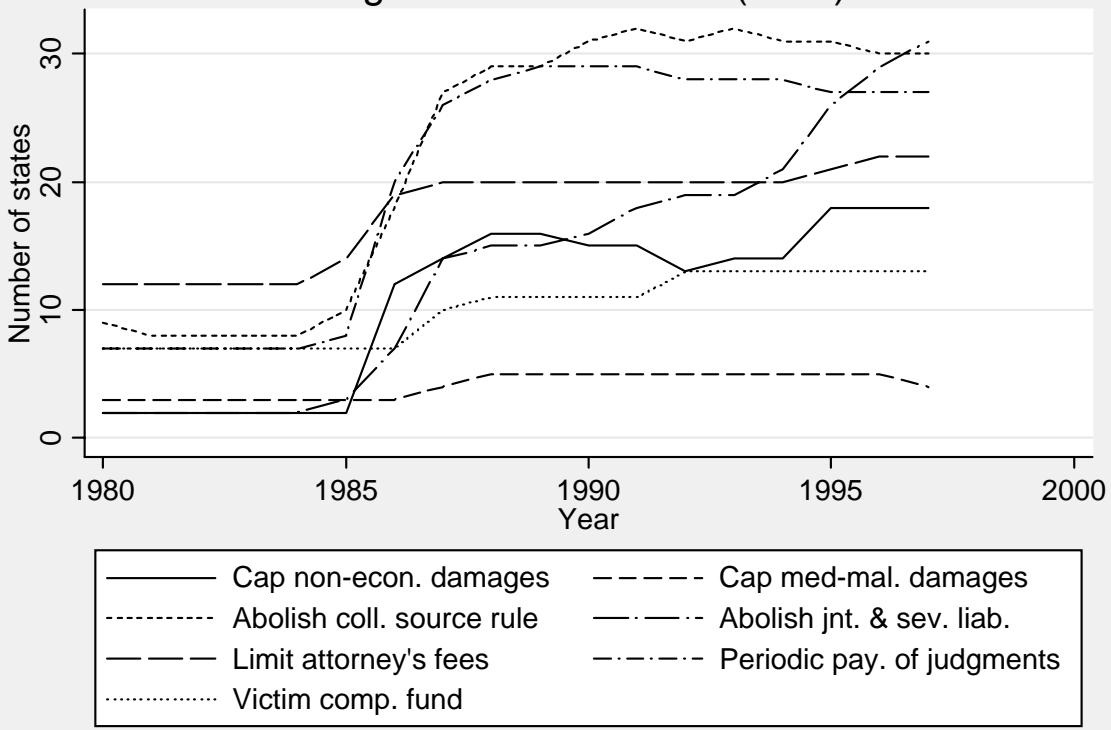


Figure 3: Tort reforms (Rubin/Shepherd)

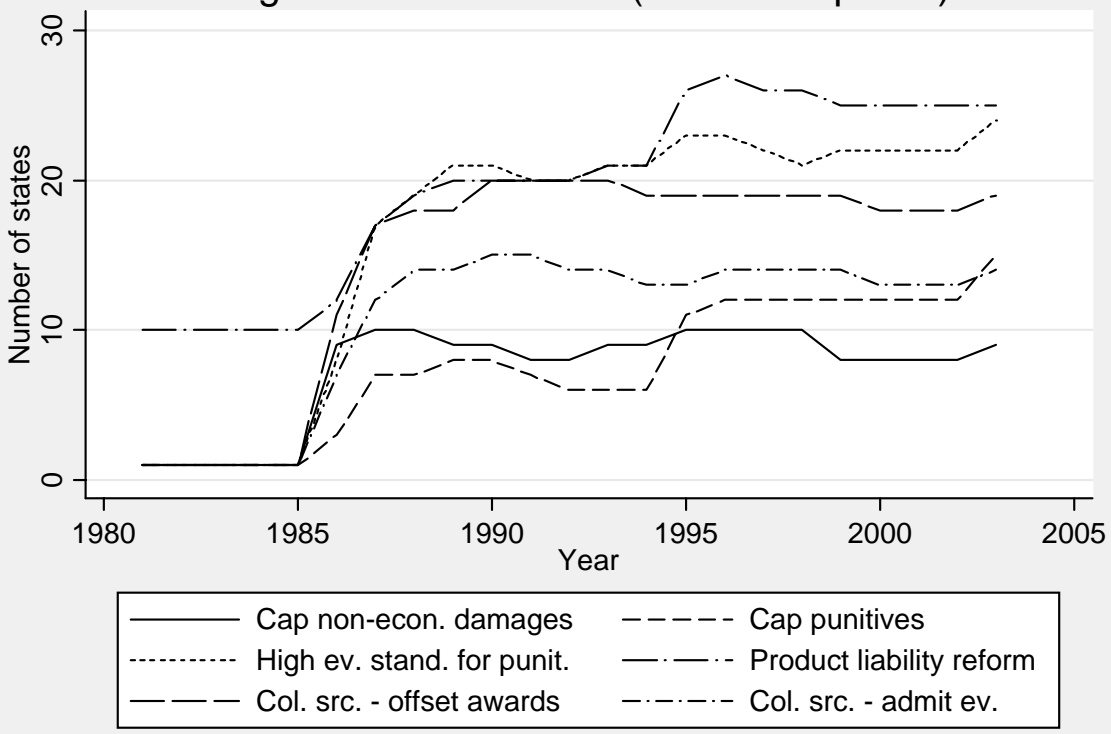


Figure 4: Abortion laws (Klick/Stratmann)

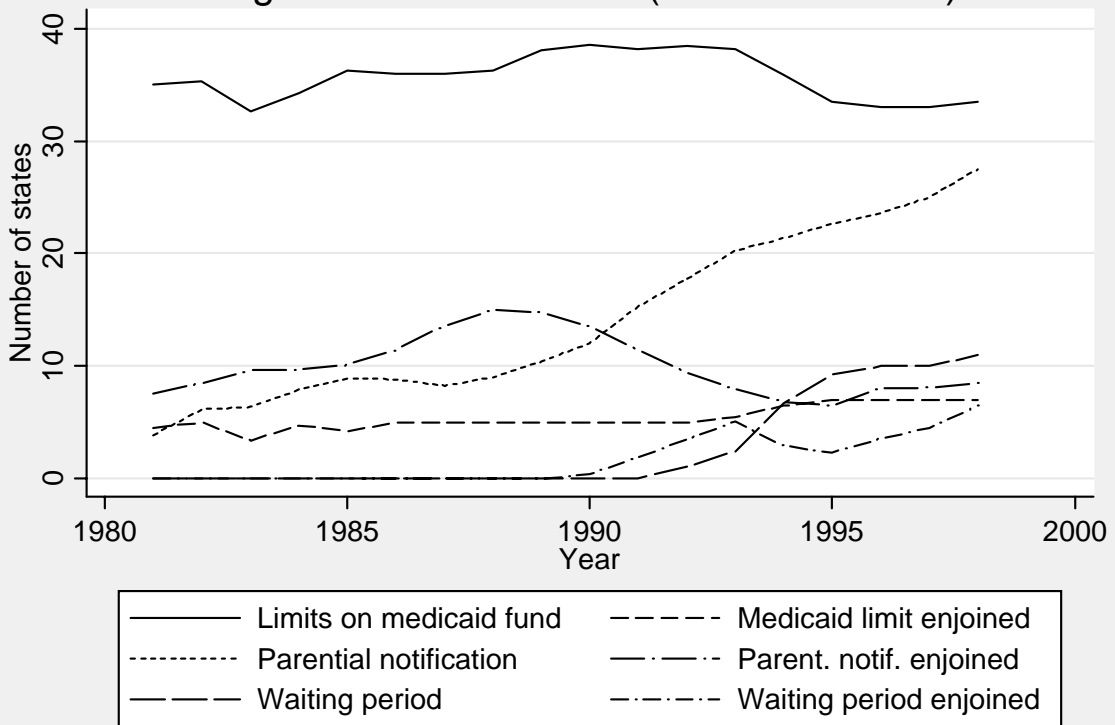


Figure 5: Auto liability (RAND)

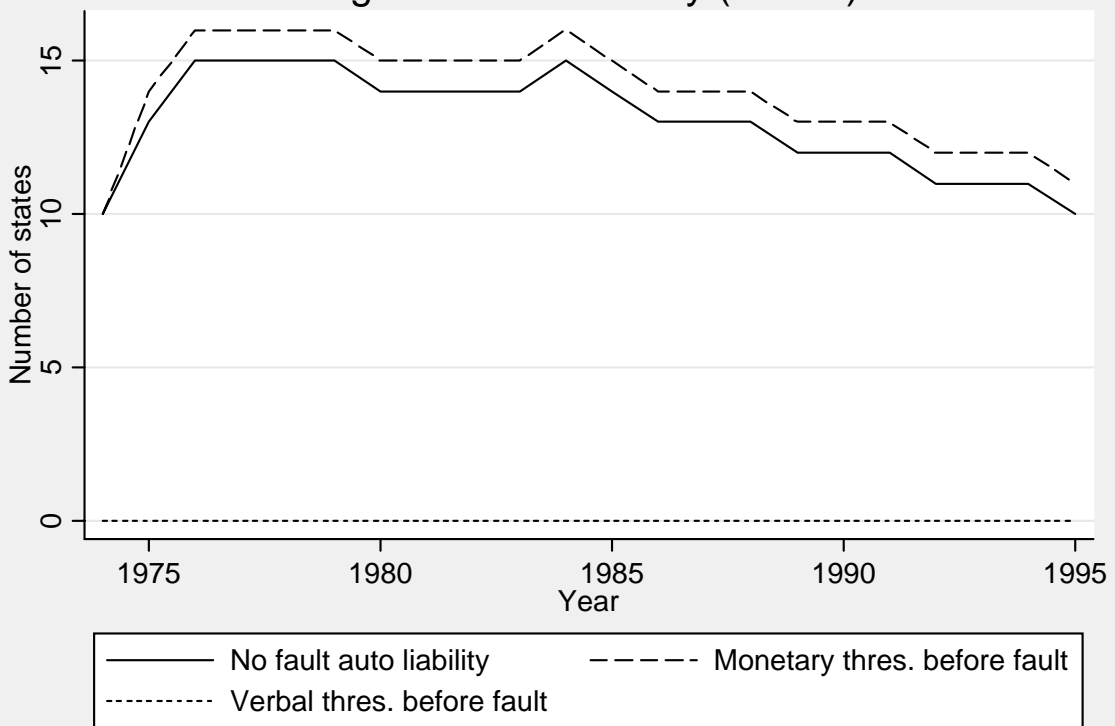


Figure 6: Divorce laws (Friedberg)

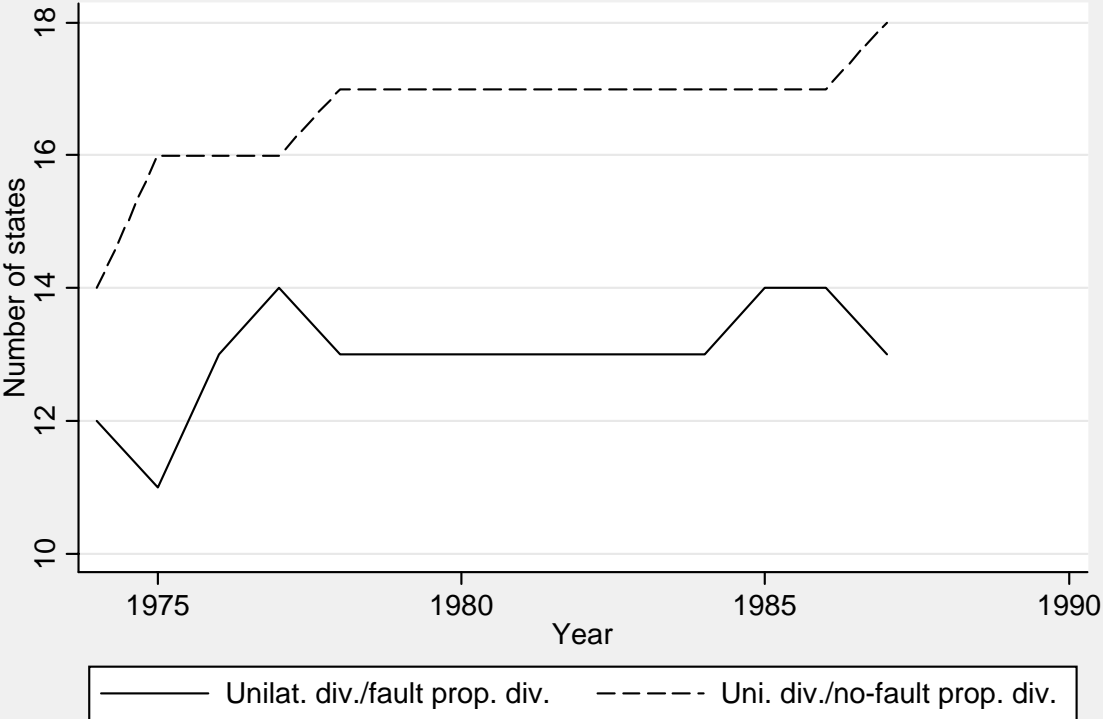


Figure 7: Capital punishment (Shepherd)



Figure 8: Health laws (Klick/Stratmann)

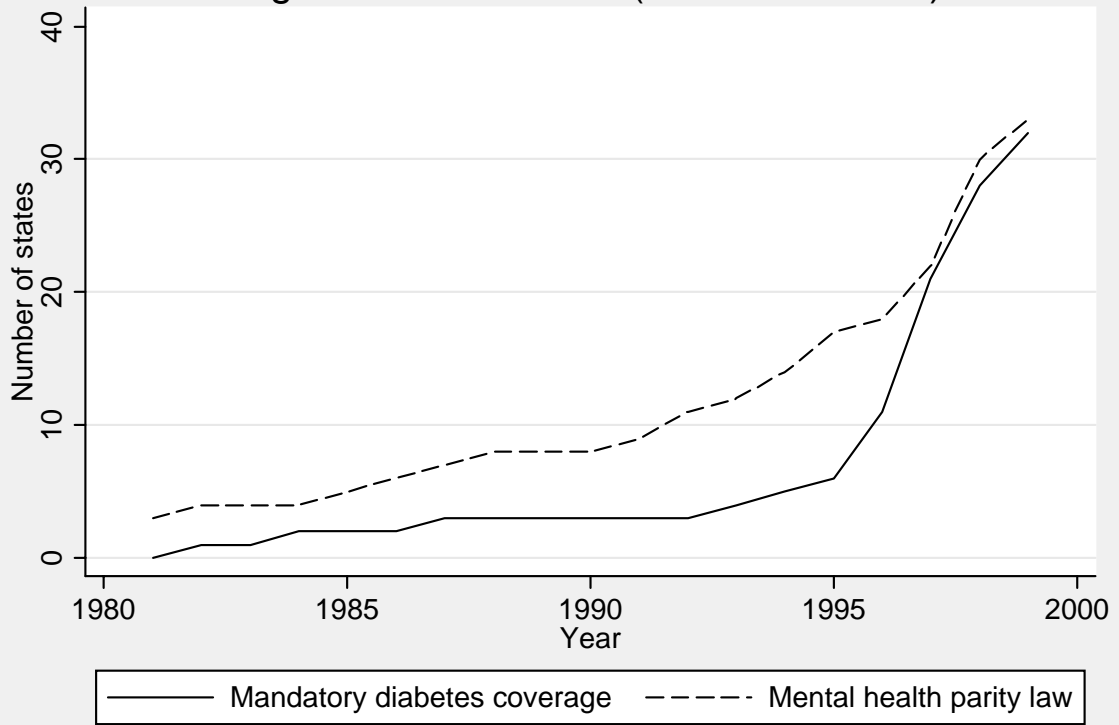


Table 2. Summary statistics.

Variable	Rent regression			Housing regression		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Rent/Value (\$)	210590	370.52	289.02	259656	99702.57	103861.40
Age (yr)	210590	31.07	20.25	259656	29.51	19.18
Full baths (#)	208161	1.11	0.34	248019	1.44	0.59
Bedrooms (\$)	210589	1.76	0.91	259656	2.97	0.86
Garage/parking (0/1)	191927	0.33	0.47	245189	0.62	0.48
Low rise (0/1)	208036	0.83	0.38	239336	0.94	0.24
High rise (0/1)	208036	0.17	0.38	239336	0.06	0.24
Holes in floor (0/1)	200874	0.03	0.16	256441	0.01	0.08
Hous. quality rating (1-10)	196168	7.22	2.26	250592	8.41	1.68
Kitchen (0/1)	210590	0.96	0.19	259656	0.99	0.12
Rooms (0/1)	210590	4.03	1.39	259656	6.21	1.62
Rent controlled (0/1)	210590	0.03	0.17			
Married (0/1)	210590	0.24	0.42	259656	0.50	0.50
Children (#)	210590	0.61	1.11	259656	0.79	1.17
Black head of hhd (0/1)	210590	0.66	0.47	259656	0.84	0.37

Variable	Hourly wage regressions			Weekly wage regressions		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Wage (\$)	206307	8.75	5.66	298809	447.05	379.76
Age (yr)	206307	35.98	13.50	298809	37.44	13.10
Union member (0/1)	206307	0.14	0.35	298809	0.14	0.35
Job has pension (0/1)	198045	0.40	0.49	290556	0.46	0.50
Employer contributes to health insur. (0/1)	198045	1039.65	1412.24	290556	1316.57	1619.08
High school grad (0/1)	206307	0.70	0.46	298809	0.63	0.48
College grad (0/1)	206307	0.11	0.31	298809	0.22	0.41
Married (0/1)	206307	0.55	0.50	298809	0.59	0.49
White (0/1)	206307	0.96	0.20	298809	0.96	0.20
Hispanic (0/1)	206307	0.09	0.28	298809	0.08	0.27
Veteran (0/1)	206307	0.13	0.33	298809	0.13	0.33

Notes. Summary statistics are unweighted. Rental and housing data span 1974-2003 (only biannually after 1981). Wage data span 1979-2003. A significant fraction of observations are dropped in subsequent regressions because law data are only available for subsets of these dates.

Table 3. Tort reform from Klick and Stratmann.

Specification	K&S Tab. 3	Rental	Housing	Wage	Wage
Dependent variable	Docs/100k pop	Monthly rent	Value	Hourly wage	Weekly wage
Non-econ	1.652 **	-3.77	2051.72	0.049	-2.361
damages cap	(0.74)	(8.38)	(4662.99)	(0.08)	(3.37)
Med mal cap	1.917	-47.77 **	-12216.32 ***	-0.288*	-17.506***
(1.50)	(19.82)	(4524.56)	(9491.70 **)	(0.15)	(4.58)
No collateral source rule	-0.619	-0.20	9491.70 **	-0.016	-2.462
(0.51)	(10.05)	(4585.36)	(0.10)	(4.55)	
No joint & several liability	1.612 **	10.61	5475.74	0.158*	5.216
(0.64)	(9.14)	(3911.24)	(0.09)	(4.52)	
Cap on contingency fees	0.432	8.08	-957.45	0.085	8.892
(0.73)	(10.77)	(5319.17)	(0.10)	(5.91)	
Madated periodic payments	-1.299 **	-6.59	-1152.45	-0.015	3.488
(0.56)	(8.34)	(3601.40)	(0.09)	(4.62)	
Victim's fund	-1.385	-18.47	-5542.04	0.091	6.826
(1.20)	(13.89)	(3421.01)	(0.13)	(8.81)	
Date range		1980-1997	1980-1997	1980-1997	1980-1997
Observations		87648	90309	156201	220410
R-Squared		0.55	0.54	0.45	0.54

Notes for other studies. In general the coefficient that is reported was selected based on similarity between empirical model behind that coefficient and the empirical model in this study. This implies a preference for OLS estimates (without correction for endogeneity) and empirical models with year- and state-fixed effects and state-specific linear time trends. Where multiple coefficient estimates satisfy these criteria, the one stressed by a studies' authors as most accurate is reported.

Notes for last four columns. The dependent variable in the housing regression is the owner's subjective assessment of the value of his/her home. These values are top coded at the 97% each year; for most of the sample this is \$300,000. The empirical model includes jurisdiction- and year-fixed effects, as well as jurisdiction-specific linear time trends. Robust (White) standard errors are reported in parentheses below each coefficient. Standard errors are permitted to cluster at the jurisdiction level. Each regressions includes the relevant covariates listed in summary statistics table. Moreover, the rental and housing regressions include building age squared and bedrooms squared variables. The wage regressions include age of worker squared as well as major industry and major occupation dummies. A **/** indicates significance at the 10/5/1% level.

Table 4. Tort reform data from Rubin and Shepherd.

Specification	R&S Tab. V†	Rental	Housing	Wage	Wage
Dependent variable	Non-auto death rate	Monthly rent	Value	Hourly wage	Weekly wage
Non-econ damages cap	-0.039 *** (0.01)	-0.24 (8.10)	2811.69 (5857.69)	0.044 (0.07)	-2 (4.32)
Punitive damages cap	-0.008 (0.01)	-15.89 * (8.83)	12034.84 ** (5987.48)	0.001 (0.07)	-4.928 (4.97)
Higher ev standard for punitive damages	-0.026 *** (0.01)	-10.67 (6.51)	-18451.84 *** (5363.91)	-0.118 (0.09)	-2.841 (4.67)
Caps in product liability cases	-0.032 *** (0.01)	10.83 (9.76)	2500.07 (5814.12)	0.199*** (0.07)	4.708 (4.24)
Prejudgment interest reform	-0.048 *** (0.01)				
Collat. src. rfm.: offset awards	0.055 *** (0.01)	9.61 (9.90)	12917.24 * (6944.75)	0.059 (0.16)	2.058 (8.16)
Collat. src. rfm.: admit ev	0.025 ** (0.01)	3.02 (8.61)	6822.61 (6497.72)	-0.119 (0.17)	-4.532 (8.21)
Date range		1981-2003	1981-2003	1981-2003	1981-2003
Observations		103419	117270	188365	280881
R-Squared		0.49	0.56	0.45	0.52

Notes. See notes for Table 3.

† Rubin and Shepherd stress that these findings are preliminary.

Table 5. Abortion access law data from Klick and Klick & Stratmann.

Specification	Klick Tab. 3	K&S Tab. 2	Rental	Housing	Wage	Wage
Dependent variable	Female suicides/ 100k women	White, female gonorrhea cases/100k pop < 20	Monthly rent	Value	Hourly wage	Weekly wage
Restriction on medicaid funding	0.091 (0.04)	**	-2.27 (9.44)	-7829.16 * (3994.47)	-0.109 (0.16)	-5.143 (6.34)
Restriction enjoined			11.94 (12.18)	-11530.00 (7951.72)	0.151 (0.27)	6.515 (11.17)
Mandatory waiting period	-0.106 (0.04)	**	-1.53 (5.68)	8140.65 *** (2520.32)	-0.09 (0.09)	-1.01 (4.63)
Waiting period enjoined			18.16 *** (6.63)	4013.55 * (2112.40)	0.024 (0.06)	6.514 (5.43)
Parental notification law		-9.541 *** (3.45)	35.39 ** (16.38)	9135.61 (7808.21)	0.121 (0.12)	13.094** (5.97)
Notification enjoined			13.69 * (8.10)	9986.88 *** (3296.00)	0.101 (0.08)	3.168 (3.86)
Date range			1981-1998	1981-1998	1981-1998	1981-1998
Observations			76766	76524	145749	209963
R-Squared			0.52	0.54	0.44	0.53

Notes. See notes for Table 3.

Table 6. No-fault auto liability data from RAND; comparison with Cummins, Phillips and Weiss and with Cohen & Dehejia.

Specification	CPW Tab. 3 OLS state effects	C&D Tab. 7 (6)	Rental	Housing	Wage	Wage
Dependent variable	Fatal auto accident rate	Fatalities per 10k pop	Monthly rent	Value	Hourly wage	Weekly wage
No-fault	1.688 n/a	*** 0.258 (0.07)	*** -6.11 (5.55)	-5270.20 (3335.57)	-0.148 (0.15)	-8.044 (7.77)
Date range			1974-1995	1974-1995	1979-1995	1979-1995
Observations			132685	154274	132929	182386
R-Squared			0.65	0.56	0.45	0.55

Notes. See notes for Table 3.

Table 7. Divorce law data from Friedberg; also comparison with Stevenson & Wolfers.

Specification	Fried-berg Tab. 4 (4.2)	S&W Tab. 4 (c)	S&W Tab. 4 (c)	Rental	Housing	Wage	Wage
Dependent variable	Divorces/ 1k pop.	Log husb. on wife violence	Log wife on husb. severe violence	Monthly rent	Value	Hourly wage	Weekly wage
Unilateral divorce		-0.038 ** (0.02)	-0.03 *** (0.01)				
Unilateral divorce & no-fault prop. division	0.545 *** (0.07)			-18.35 * (9.56)	-21999.94 *** (5774.26)	-0.007 (0.06)	-18.388*** (3.41)
Unilateral divorce & fault prop. division	0.392 *** (0.06)			-18.34 ** (7.77)	-16595.57 *** (3101.98)	-0.268*** (0.04)	-4.090* (2.24)
Date range				1974-1987	1974-1987	1979-1987	1979-1987
Observations				99587	132367	70157	76234
R-Squared				0.54	0.55	0.49	0.56

Notes. See notes for Table 3.

Table 8. Death penalty data from Shepherd (JLS 2004).

Specification	Shepherd Tab. 2		Rental	Housing	Wage	Wage
Dependent variable	All murders		Monthly rent	Value	Hourly wage	Weekly wage
Executions per murder	-0.04 (0.01)	***				
Death sentences per murder	-0.021 (0.00)	***				
Executions (#)			2.02 (0.50)	*** -256.79 (171.19)	0.004 (0.00)	0.400*** (0.15)
Death sentences (#)			0.13 (0.16)	442.99 (106.74)	*** 0.002 (0.01)	-0.018 (0.17)
Date range			1977-1997	1977-1997	1979-1997	1979-1997
Observations			115660	125800	148251	207411
R-Squared			0.61	0.53	0.45	0.54

Notes. See notes for Table 3.

Table 9. Health insurance mandates data from Klick & Stratmann.

Specification	K&S (2005) Tab. 5	K&S (2003) Tab. 4 (iii)	Rental	Housing	Wage	Wage
Dependent variable	BMI	Beer consump. (gallons/person)	Monthly rent	Value	Hourly wage	Weekly wage
Diabetic * Diabetes coverage mandate	0.401 (0.13)	***				
Diabetes coverage mandate			17.21 (5.55)	*** -12929.36 (3315.22)	*** 0.024 (0.08)	-1.513 (4.56)
Mental health parity law with alcohol coverage		0.025 (0.01)	***			
Mental health parity law			5.86 (6.23)	-3190.06 (3158.48)	-0.034 (0.07)	-3.438 (4.02)
Date range			1981-1999	1981-1999	1981-1999	1981-1999
Observations			86810	91362	161307	235307
R-Squared			0.5	0.54	0.44	0.53

Notes. See notes for Table 3.

Table 10. Multiple laws data.

Specification	Separate regressions				Joint regression			
	Rental	Housing	Wage	Wage	Rental	Housing	Wage	Wage
Dependent variable	Monthly rent	Value	Hourly wage	Weekly wage	Monthly rent	Value	Hourly wage	Weekly wage
Non-econ damagescap	-0.24 (8.10)	2811.69 (5857.69)	0.044 (0.07)	-2 (4.32)	-10.37 (9.60)	-4156.57 (4518.01)	0.046 (0.10)	-3.546 (5.49)
Punitive damages cap	-15.89 * (8.83)	12034.84 ** (5987.48)	0.001 (0.07)	-4.928 (4.97)	-18.76 ** (8.72)	-74.40 (2480.10)	0.05 (0.10)	7.225 (5.87)
Higher ev standard for punitive damages	-10.67 (6.51)	-18451.84 *** (5363.91)	-0.118 (0.09)	-2.841 (4.67)	0.24 (7.90)	-8368.39 ** (3347.36)	-0.064 (0.10)	-7.926 (5.59)
Caps in product liability cases	10.83 (9.76)	2500.07 (5814.12)	0.199*** (0.07)	4.708 (4.24)	19.79 ** (7.92)	7624.60 *** (2524.70)	0.242*** (0.08)	6.103 (5.32)
Collat. src. rfm.: offset awards	9.61 (9.90)	12917.24 * (6944.75)	0.059 (0.16)	2.058 (8.16)	-6.47 (7.19)	9196.62 *** (2689.07)	0.057 (0.09)	11.770** (4.92)
Collat. src. rfm.: admit evid.	3.02 (8.61)	6822.61 (6497.72)	-0.119 (0.17)	-4.532 (8.21)	2.61 (7.26)	-7915.49 ** (3533.70)	0.016 (0.13)	-10.786 (6.43)
No-fault	-6.11 (5.55)	-5270.20 (3335.57)	-0.148 (0.15)	-8.044 (7.77)	1.09 (7.75)	139.47 (6973.65)	-0.068 (0.11)	-5.337 (8.91)
Executions (#)	2.02 *** (0.50)	-256.79 (171.19)	0.004 (0.00)	0.400*** (0.15)	1.94 ** (0.85)	589.96 ** (279.60)	0.005 (0.01)	0.664 (0.45)
Death sentences (#)	0.13 (0.16)	442.99 *** (106.74)	0.002 (0.01)	-0.018 (0.17)	-0.16 (0.27)	-54.11 (108.23)	0.002 (0.01)	0.003 (0.13)
Diabetes coverage mandate	17.21 *** (5.55)	-12929.36 *** (3315.22)	0.024 (0.08)	-1.513 (4.56)	14.51 ** (6.28)	-9024.67 ** (4028.87)	-0.141* (0.08)	-4.003 (8.10)
Mental health parity law	5.86 (6.23)	-3190.06 (3158.48)	-0.034 (0.07)	-3.438 (4.02)	0.23 (5.70)	634.58 (3525.46)	0.118** (0.06)	0.911 (3.41)
Date range		Varies			1981-1995	1981-1995	1981-1995	1981-1995
Observations		Varies			69750	66339	122329	171193
R-Squared		Varies			0.53	0.52	0.44	0.54

Notes. See notes for Table 3.