POLICY ANALYSIS AND POLICY ADOPTION: A STUDY OF THREE NATIONAL DRUNK DRIVING INITIATIVES*

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Abstract: All published studies analyzing the effect of the minimum legal drinking age, the .08 per se blood alcohol threshold for drunk driving, and zero tolerance laws on drunk driving or crashes were collected and analyzed. The evolution of study design and study findings is remarkably similar in all three cases. Estimated effects become steadily smaller and less variable over time, in a decades-long process, while quasi-experimental methods yield to regression-based methods, particularly panel regression. The evolution in findings is related to these changes in study design and to an "early-adopter effect" that yields larger law-fatality associations in early-adopting states.

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*** This paper has several pages of figures that are best viewed in color. ***

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This paper is the second of a four part series on the economics, political economy, and statistical analysis of drunk driving legislation. The first, Grant (2010b), examines the statistical properties of fatalities and the incidence of drunk driving, argues that analyzing the latter yields improved assessments of drunk driving legislation, and finds that drunk driving laws generally have small effects on drunk driving, casting their efficacy into doubt. The third, Grant (2010c) explores how the political system at the federal level assesses the efficacy of drunk driving laws through an extended narrative of the process surrounding the passage of the National Minimum Drinking Age Act. This indicates that the system amplifies, rather than moderates, the overoptimism of the early evidence. The last paper in the series, Grant (2010d), argues that drunk driving behavior is fundamentally non-economic, that this helps explain the relative inefficacy of laws, and that improved legislation can be designed using structural methods.

The states, "laboratories of democracy," experiment with various approaches to solving social problems; those that are successful are adopted by other states or by the federal government. Perhaps nothing exemplifies this ideal better than traffic safety legislation, which repeatedly follows a familiar pattern: a few "precocious" states adopt a new initiative; independent academic and government analysts certify the initiative's success in those states; testimony to this effect by policy advocates and key government officials persuades other states to voluntarily adopt this legislation, or Congress to encourage its adoption nationwide.

Ultimately, however, this process is successful only if the legislation that is adopted is effective. In some cases, such as seat belt laws, this is unquestioned; in other cases is it not so clear. Thus the vitality of this process hinges neither on the willingness of states to experiment nor on the readiness of the federal government to act, both of which have been demonstrated repeatedly over the decades, traffic safety legislation being much less partisan than many other areas of American political life. Instead, it hinges on the ability of policy analysts to evaluate the effect of the policy under consideration in early-adopting states, forecast its likely effect should it be adopted more widely, and communicate this information (and its exactitude) to lawmakers. While the public might view this as an opaque and relatively routine exercise, researchers will understand that this is not so.

Fortunately, most major traffic safety initiatives are studied not just when they first appear, but also long after they have been well established. In fact, these retrospective evaluations, just like hindsight in general, will probably be both more accurate and more precise. Studying the evolution of the estimated effects of a particular policy over time is therefore a useful way to understand the role of the policy analyst in the political process outlined above.

In this paper we do this for three signature pieces of traffic safety legislation targeted at drunk driving: the minimum legal drinking age (MLDA) of twenty-one; zero tolerance (ZT) laws for youth under twenty-one years of age, which set the per se illegal blood alcohol (BAC) threshold at .01 or .02 (g/dl); and laws setting the per se standard for adults at .08. Each has been adopted nationwide within the past generation, primarily because of Congressional mandate (the threatened withdrawal of substantial highway funds), and each has been studied repeatedly, both before and after this mandate.

Assessments of each of these laws' effects follow a similar pattern: initially large and diffuse, they decline markedly in magnitude and variability over time, in a convergent process vaguely resembling exponential decay. Three forces drive this evolution: improvements in study design, increases in the amount of data analyzed, and differences in the reception, implementation, or enforcement of these laws between early-adopting and late-adopting states. In consequence, early analysts' predictions of the likely effects of adopting these policies nationwide have proven overly optimistic to a substantial degree.

In consequence, policy analysts must seek out improved methods of forecasting the likely effects of traffic safety legislation in late-adopting states, particularly when these adoptions are encouraged by Congress. A companion paper, Grant (2010b), introduces a practical method of doing so. Even more importantly, lawmakers must institutionalize processes that ensure that assessments of the effects of large-scale traffic safety legislation are made judiciously. Another companion paper (Grant, 2010d) demonstrates that political processes currently amplify optimism about the likely effects of these policies, rather than dampening it, and outlines institutional reforms that could reverse this tendency.

I. Drunk Driving Legislation: The Life Cycle of a Literature.

To understand why the estimated effects of a given law vary over time and to formulate proscriptive suggestions for improving policy analysis, one must understand how evaluations of drunk driving laws evolve to begin with. (Some of the material in this section is based on the extensive discussion in a companion paper, Grant, 2010d.)

To begin with, a few states adopt (ZT, .08) or change (MLDA) the law in question.[1] Between 1983 and 1992, five states adopted .08 per se BAC limits. During the 1980s six states adopted lower BAC limits for youth. Between 1976 and the end of 1980, one state raised its drinking age three years, four raised it two years, and eight raised it by one year. These early law changes are most common in coastal states, several of which appear more than once in the states identified above (including California, Maine, and Georgia), while many others appear not at all (such as Louisiana, South Dakota, and Ohio). The sparse number of early-adopting states has a profound influence on the literature, as it complicates inference substantially.

Nevertheless, interest naturally arises as to the effect of these law changes. This interest is substantial enough that studies begin to appear within a couple of years of implementation, sometimes earlier. These studies use a variety of quasi-experimental techniques applied (generally) to time series data, which typically compare the before-law/after-law change in fatalities in the target group with those in a control group, though one can find variations on this theme. Those studies that are conducted by academics, policy advocates (such as researchers at the Insurance Institute for Highway

[1] A dozen states have had drinking ages of twenty-one essentially since the repeal of Prohibition. These cannot be used to identify the effect of the MLDA except in discredited cross-section studies.

Safety), or contractors to the National Highway Traffic Safety Administration (NHTSA) are typically intended for publication, usually in health or safety journals such as the *American Journal of Public Health*, the *Journal of Safety Research*, or *Accident Analysis and Prevention*. Those produced by federal or state agencies, on the other hand, are rarely intended for publication, and often would not meet the required standards of evidence (Grant, 2010d). For these agencies, this is not a problem: their intended audience is policymakers, not academics, and with this audience these agencies have institutional credibility that does not depend on certification of their work by the academic community. Thus the early published work, while perhaps weaker by retrospective, academic standards, includes the *better* studies contemporaneously available when these laws begin to diffuse more widely throughout the country.

To illustrate this point, consider the report produced by the General Accounting Office (GAO) concerning the effect of the MLDA in March 1987, three years after the National Minimum Drinking Age Act was passed by Congress in July 1984 (GAO, 1987). Following an extensive literature search, studies of *raised* drinking ages that met minimum criteria for soundness were identified and reviewed. Of those studies identified that analyzed crashes, injuries, or fatalities, roughly half did not meet these minimum standards. Of the remaining fourteen studies, three had been successfully refereed and were published or forthcoming; another five were ultimately published; and the remaining six were never published (that is, in a book or academic journal). Half of these six were produced by NHTSA and the other half by state government agencies.

Similarly, in the GAO's 1999 review of the effects of .08 laws, seven studies were identified: one produced by a state agency, two produced partly or wholly by NHTSA, and four conducted by academics or contractors that were funded by NHTSA or other federal agencies. Three of the funded studies were ultimately published; none of the others were.

In the case of the three laws studied here, Congress acted to strongly–and successfully–encourage their adoption nationwide by otherwise threatening to withhold states' highway funds. This typically occurs as the early evidence on these laws' effects begins to trickle in and the rate of voluntary adoption of these laws begins to rise. The Congressional "mandate" for the MLDA was passed in 1984, and the last states acquiesced in 1988; it was passed in 1995 for ZT laws, uniformly adopted by 1998; and in 2000 for .08 laws, in place nationwide by 2005. As a result, about two-thirds of the states adopt these laws rapidly and involuntarily, based on a relatively small number of studies that generally employ data from just a few early-adopting states.

From the point of the Congressional mandate, the literature evolves as follows. The topical interest engendered by Congressional action (and the precursors to that action, such as media attention and meetings of Congressional committees) inspires a flurry of academic research, so that (ironically) the greatest frequency of publications typically occurs a few years after the mandate has been passed. At the same time, as the number of states changing the law in question increases, it becomes feasible to use large scale empirical methods that analyze outcomes within all states over a decade or more, so that study designs begin to evolve as well.

These regression-based methods take two forms: pooled time-series cross-section (TSCS) studies, which omit state and year fixed effects in favor of direct controls for confounding factors (such as residents' religious affiliations), and panel methods which also include state and year fixed effects. The latter method requires more data than the former to be practical–to stand a reasonable chance of yielding significant coefficient estimates–so that, among regression-based methods, pooled TSCS dominates initially, eventually yielding in frequency to panel methods. These regression-based methods, especially panel regression, tend to be the province of social scientists, so the research outlets shift along with the methods, away from health and safety journals toward social science and policy analysis journals such as the *Journal of Policy Analysis and Management*, the *Journal of Health Economics*, and *Social Science Quarterly*. Studies continue to be published for more than a decade after the initial Congressional mandate, so that the "life cycle" of the literature extends at least twenty years in total.

Figure 1 maps out this evolution, dividing this life cycle into three periods, loosely distinguished by the passage of the Congressional mandate and by complete adherence of the states to that mandate. Early studies typically utilize data on just a few, early-adopting states, employ quasi-experimental methods that rely on control groups instead of control variables, and belong to the health and safety literature; later studies utilize data on most or all states, employ regression-based methods, and belong to the social science literature. In between, during the period of greatest academic interest, is a transition zone, so a hybrid of methods and outlets are to be found.

Some evidence on this point is presented in Table 1, based on the data described below, and more will be presented in figures discussed shortly. This bifurcates the studies in each of these three literatures by publication date, with the line of separation placed three years after the Congressional mandate. Publications are almost evenly divided in frequency between these two periods; panel studies and social science publications are rare in the earlier period but dominate the latter period.

In summary, the evolution of research on drunk driving legislation can be described through three sets of actors, each in distinct and somewhat segmented professional spheres: state and federal agencies, health and safety researchers, and social scientists. Each set of actors faces differing constraints and objectives, leading each to choose methodologies that represent different tradeoffs between the immediacy of findings and their accuracy. We now determine whether these different choices lead to different outcomes–estimates of the laws' effects. For this we focus on (academically) published studies, and thus the latter two sets of actors.

II. Data.

For this study we compiled all studies of three drunk driving laws–the minimum legal drinking age, zero tolerance laws, and .08 laws–that met the following criteria:

- They are based on U.S. data;
- They are published in a book or a refereed journal;
- They study drinking and driving, crashes, crashes with injuries, or crash fatalities (collectively referred to as "outcomes") but not drinking without driving;
- For the MLDA and ZT laws, the group studied is youth (under 21 years of age), to whom the laws apply.

Studies of raised or lowered drinking ages are included, as are those of zero tolerance laws that do not fully meet the federal mandate (called "partial laws" in Grant, 2010a). To find studies, we used a variety of techniques: prior literature reviews, standard literature searches in various databases, and citation tracking. As a result, our universe of studies is not only more current than in other literature reviews (Shults et al., 2001; Wagenaar and Toomey, 2002, 2005), but also more comprehensive, including a number of earlier studies other reviews left out.

For each study we recorded the spatial and temporal features of the data; the empirical methodology; data sources; definitions of the dependent variable, key independent variable, and controls; funding sources; academic citations in Google Scholar as of the summer of 2009; and both our and the authors' conclusion of the effect size–the estimated influence of the law on the outcome studied, in percent. For the most part our determinations of significance and effect size closely matched those of the authors and of literature reviews (Shults et al., 2001; Wagenaar and Toomey, 2002); where there were reasonable differences on the interpretation of the evidence we deferred to the authors. The Appendix contains a complete listing of all included studies and their key features. The unit of analysis here is the study; whenever a study reports multiple results, we use a simple average of these results unless one result clearly trumps the others in generality. (In a few cases, two publications featured the same authors, methods, and findings; these are treated as one.)

Of particular importance is the study design. Except for a small number of cross section studies and one regression discontinuity analysis, study designs fall into the three types itemized above. Cross section studies estimate effect sizes solely from geographic variation in laws and outcomes across states. Because this variation in outcomes tends to be strong and is probably associated with a multiplicity of factors, many of which cannot be easily controlled for, this study design is generally recognized as being weak. It has, accordingly, become obsolete, the last such study having been published in 1993, and thus receives little attention here.

Effect sizes are measured as the percentage change in outcomes attributed to the law. If there are several results presented, a simple average of the findings is taken unless the authors indicate one result trumps the others. Insignificant findings (which generally indicated very small effects) were coded as zero; those estimating effects exceeding forty percent were top-coded at that point. These choices, along with using the study as the unit of observation and being perhaps overly deferential to the authors' conclusions (see below) all are an attempt to reflect the perspective of the political system rather than a strict meta-analytic perspective.

III. Study Findings–An Overview.

For each law, we can represent many of these study features on a single "bubble plot"; these are presented in Figures 2, 3, and 4. Each plot has the following features. The horizontal axis represents the year of publication, which typically follows the last year of the data by a couple of years. The vertical axis represents the effect size, as described above. The *volume* of each bubble is proportional to the number of citations; later studies are cited less frequently, of course. (There is a minimum bubble size so that even uncited studies are represented on the graph.) Bubbles ringed in black circles are supported by external funding, generally from the National Institute on Alcohol Abuse and Alcoholism (NIAAA) or the National Highway Traffic Safety Administration (NHTSA), both federal institutions. Finally, the color (in the electronic version of this manuscript) or shading (in the paper version) indicates a key feature of the study design: the type of variation used to identify the effect of the law. Cross section, quasi-experimental, and pooled TSCS studies have various types of dark shading in Figures 2-4, while panel studies have light shading. The vertical line in each figure corresponds to the Congressional "mandate" to the states to adopt the law in question.

Studies of the MLDA are presented in Figure 2. The MLDA is unique among the three laws studied in having a significant body of research at the time of Congressional action, because many states had lowered their drinking ages in the early 1970s, raising awareness of the issue and making statistical analyses possible. Thus there are in practice two literatures, each clearly identified in the figure: one about lowered drinking ages, early in the chronology and having positive effect sizes (increased fatalities), and another about raised drinking ages, coming later and having negative effect sizes. (This bifurcation also reflects our political orientation, as virtually no evidence on the effects of lowered drinking ages was used when contemplating the MLDA of 21 mandate in Congress; Grant, 2010d.) While both literatures follow the patterns we are about to describe, our attention is naturally focused on the more policy relevant raised MLDA.

This literature is also far more numerous. The greatest study density, in fact, occurs in the mid-1980s, shortly after the Congressional mandate, when topical interest was at its peak, but studies have been published steadily thereafter, up to the present–a total of seventy studies. The evolution in study design described above is also clearly evident, with quasi-experimental studies dominating early and panel studies dominating late, with a mix in between, a period of transition.

While a significant number of studies are funded externally, funding does not have a strong association with study type or study findings. A careful look at study design indicates that funded studies' methods evolve over time, just as unfunded studies' methods do, and that funded studies are not early or disproportionate adopters of improved methods. Funding increases the number of studies, particularly in the mid-1980s period of greatest topical interest, but does not promote improvements in study quality, following rather than leading the way in this regard.

Citations, too, are not strongly associated with study quality. They also vary widely, even among studies of similar antiquity, as in most academic literatures. Among studies published in the 1980s, for example, eleven have been cited fewer than ten times, while three have been cited more than one hundred times. The two most-cited studies have in common an esteemed author (health economist Michael Grossman), but their study quality is not distinctive, nor the journal pre-eminent, nor the findings unusual. The studies that were most influential in the political process leading up to the adoption of the Congressional mandate are not particularly well cited (Grant, 2010d). The only obvious correlate to citations, besides study age, is its findings: studies are less frequently cited if their findings are particularly large or particularly small.

The final and most striking feature represented in Figure 2 is the findings. Effect sizes decrease markedly over time while also becoming less variable–an important feature, as the adversarial nature of the politics of traffic safety legislation can encourage cherry picking of the most-favorable and least-favorable studies by the opposing political sides. The trend line indicates a reduction in the mean effect size of about five percentage points.

Figure 3, for ZT laws, and Figure 4, for .08 laws, exhibit most of the same features as Figure 2, though studies are fewer in number and later in chronological time. Funding increases the number of studies, particularly around the period of greatest topical interest, but does not promote advances in study quality. The evolution in study frequency and study design matches that in Figure 2, with quasi-experimental and pooled TSCS more frequent early, eventually giving way to panel analyses. Finally, and most vitally, study findings both decline and become less variable, in both cases converging almost to zero. Effect sizes drop by at least five percentage points in the .08 literature and by much more in the ZT literature.

In Section I we offered an institutional explanation for the evolution in study design.[2] We must now explain the patterns in findings, which, like those in study design, are systemic.

IV. The Evolution of Study Findings: The Role of Early-Adopters.

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[2] The difference in chronological time rules out a technological explanation for the evolution in study quality. The same features that appear in the MLDA literature, initiated in the 1970s when data, methods, and computing power were relatively crude, also appear in the ZT and .08 literatures, initiated two decades later when these limitations no longer pertained.

What explains this evolution in study findings? While variation in study quality is an obvious candidate, we should first consider that early-adopting states may differ from late-adopting states, particularly as these latter states are not adopting these laws voluntarily. Thus, even if study design were constant, estimates of effect size may vary over time, as the data includes an increasing number of late-adopting states. One possibility is that states that voluntarily adopt laws are more likely to enforce them, and the citizens of that state are more likely to obey them. An alternative possibility, frequently noted in the traffic safety literature, is that laws, media campaigns, and (possibly) enforcement efforts tend to come in "packages," so statistical analyses–which do not and cannot control for everything in the "package"–will tend to overestimate these laws effects. These packages may be stronger, again, for voluntary adopters. While the causal chains differ, the ultimate result is the same: larger estimated effects in early studies that are not indicative of the effect in states forced to adopt the law.

Miron and Tetelbaum (2009) argue for, and find evidence of, just such an early-adopter effect in a careful study of MLDA laws. The "stock plot" at the top of Figure 5 illustrates their key finding (their Table 5). This table presents the results of 38 separate regressions, each conducted on an individual state that passed an MLDA of 21 between 1978 and 1988, and each using the same sample period (1976-2005) and log-linear regression specification. The horizontal axis represents the date the MLDA of 21 was implemented in that state, and the vertical axis is demarcated in log points. The red dot contains the estimate of the MLDA on log fatalities in that state, negative indicating lower fatalities and positive (presumably) higher fatalities, though of course there is sampling error. This is illustrated with the bars extending up and down from each dot, which each have a length of two standard errors.

Clearly the MLDA effect size is much larger in early-adopting states, and the difference is substantial. How, then, do estimates using early-adopting states compare with those using all states? To find out, we re-execute the stock plot in the bottom part of Figure 5 as a cumulative, or running, average. The red dot now reflects the average of all effect sizes up to that date, weighted by the inverse of the variance of each estimate, and the vertical lines represent two standard errors of that average. The cumulative estimate at the end of the period is seven or eight percentage points lower than that at the beginning–a sizeable effect.

Does this same trend occur with ZT and .08 laws? To find out, we turned to two recent, comprehensive studies of these laws, Grant (2010a) and Freeman (2007), respectively. Both studies are panel data analyses that span virtually the entire period during which these laws were passed; both carefully relate their findings to earlier work; both are attentive to econometric issues that could confound their estimates; and both authors have made available their data to us. To retain maximum comparability with the original studies, instead of running multiple state regressions with this data, we simply re-estimated their main regression specifications, interacting the law variable with state dummies, thus again retrieving state-specific estimates of the effect of each law, as follows:

$$F_{s,t} = \beta X_{s,t} + \gamma_s L_{s,t} + \sigma_s + \tau_t + \epsilon_{s,t} \tag{1}$$

where σ represents state fixed effects; τ represents year fixed effects; F is the log of the per mile or per person fatality rate; L is a dummy variable for the law in question; X represents directly observed statewide, time-varying controls such as the unemployment rate; and ϵ is an error term. The effect of the law, γ, is estimated separately for each state. From these we can create stock plots in the format of Figure 5.

Figure 6 presents the results for the ZT law. (Grant's primary specification covers nighttime accidents involving under 21 drivers for the years 1988-2000.) The variation in individual state effects is even larger than for the MLDA, but in the bottom of the figure the cumulative estimate at the end of the period is again seven or eight percentage points lower than that at the beginning. Figure 7 presents results for .08 laws. (Freeman's specification includes all drivers in all continental states for the years 1980-2004.) The variation in individual state effects is smaller than observed for the other two laws, yet, still, in the bottom of the figure the cumulative estimate is smaller: five percentage points lower at the end of the period than that at the beginning.

In summary, there appears to be an "early-adopter" effect that makes early estimates of these laws overly optimistic indicators of their effect if passed nationwide, by about seven percentage points on average. This effect could be genuine, because the law is actually more effective in states that adopt it voluntarily, or artificial, because voluntarily-adopted laws are associated with changes in drinking sentiment, enforcement, or other legislative activity that also affects drunk driving.

V. The Evolution of Study Findings: The Role of Study Design.

To think about the role of study design, modify equation 1 above so that the effect of law L on fatalities, γ, is common across all states, and explicitly distinguish between two types of error: specification error, that is, the effect of unmeasured variables or unmeasurable factors that influence fatalities, indicated by ξ, and sampling error, by which realized fatalities vary around their expected value because of the randomness inherent in any Poisson process, denoted by ν.

$$F_{s,t} = \sigma_s + \tau_t + \beta X_{s,t} + \gamma L_{s,t} + \xi_{s,t} + \nu_{s,t} \tag{2}$$

The variance of this last term depends on state population.

Dissonance over study design has always been a part of the traffic safety literature. Simply within the MLDA literature one can find criticisms of each empirical approach. Wagenaar (1981) eschews regression-based methods, arguing that the assumption of "independent observations" is invalid, that is, ξ exhibits serial correlation. Conducting regressions, Grossman and Saffer (1987) and Voas et al. (2003) argue against using state dummy variables because they "overfit" the model and induce "multicollinearity," while Cook and Tauchen (1984) argue that *only* these dummy variables should be included, lest the γ estimates become to "sensitive to specification errors." Garber (1988) has effectively refuted many of these criticisms. Fatalities evolve as equation 2 specifies, and there is good reason to believe that they do, then the empirical method should reflect that process.

<u>Study Design: Two Empirical Issues</u>. The differences between the three primary estimation approaches in these literatures can be laid out in terms of equation 2, and devolve to the methods by which they control for extraneous factors that could coincide with the adoption of law L. There are two nominal distinctions between these three methods. The first is whether extraneous factors are controlled for explicitly, with control *variables*, or implicitly, with control *groups*. This issue is best explored by comparing quasi-experimental methods with panel regression. The second is whether regression methods should use "indirect controls," that is, the state and year fixed effects employed by panel methods, or "direct controls," as in pooled TSCS analyses.

Quasi-experimental Methods vs. Regression Based Methods. Panel methods adopt equation 2 as their specification, generally conducting estimates on all fifty states over a period spanning at least a decade, including explicit state time-varying controls in X, and weighting by state size to (crudely) account for the varying variance of ν. The large geographical and temporal span of the data will tend to reduce variability in the γ estimate, but these estimates can still be biased if X does not contain vital controls, and will vary across studies as the sample and the contents of X vary.

Some components of X are easy to measure and consistently significant–particularly economic factors, unemployment and/or personal income, which are almost always present in the specification and significant. Religion variables (percent Catholic, Baptist, etc.) are also sometimes included, and sometimes significant. But many other potentially relevant factors, including safety attitudes and drinking sentiment, are difficult to measure.

Quasi-experimental methods, in contrast, almost always leave out control variables, but this is more by necessity than design–the before/after style of these methods precludes the use of regression to estimate control variables' effects. Furthermore, the general basis of comparison, the change in fatalities in a law-changing state relative to a non-law-changing state, is the functional equivalent of adopting state and year fixed effects.

To see this, consider two states, 1 and 2, and two periods, 1 and 2. The first state adopts law L just before period 2, while the second state never adopts law L. One common type of quasi-experimental method, estimates γ as $\log((f_2^1/f_1^1) / (f_2^2/f_1^2))$, where f is unlogged fatalities or fatality rates, and the superscripts refer to states and the subscripts refer to periods. If $X_t^1 = X_t^2$, the maintained assumption of these methods, then this term identifies γ:

$$\begin{aligned} \log(f_2^1/f_1^1) &= [\sigma_1 + \tau_2 + \beta X_2 + \gamma] - [\sigma_1 + \tau_1 + \beta X_1] = \tau_2 - \tau_1 + \beta(X_2 - X_1) + \gamma \\ \log(f_2^2/f_1^2) &= [\sigma_2 + \tau_2 + \beta X_2] - [\sigma_2 + \tau_1 + \beta X_1] = \tau_2 - \tau_1 + \beta(X_2 - X_1) \end{aligned} \quad \textbf{(3)}$$

Of course, if $X_t^1 \neq X_t^2$, there may be bias. Thus the validity of quasi-experimental methods, like that of panel methods, therefore, relies on a kind of smoothness: that the extraneous factors that influence fatalities are sufficiently smooth across space (quasi-experimental) or time (panel regression) that they are adequately captured by control groups (quasi-experimental) or state and year fixed effects and a nominal number of controls (panel regression). This assumption, while reasonable, may not be well-justified, or equally well-justified, in the two approaches. But this is an empirical distinction, not a structural distinction. (In fact, Grant, 2010b finds that $\rho(\xi^1,\xi^2) \approx 0.4$ after extracting year fixed effects.)

In fact, the similarity between the two approaches does not end there–one can use control variables and control groups together, and an increasing number of studies do just that. That is, panel regressions are estimated both on the intended population and on a control group, and the resulting γ estimates compared, just as analogous "falsification tests" are used for robustness checks in a wide variety of empirical studies in various fields of microeconomics. Doing so is particularly simple for studies of laws that affect only youth. Thus, for example, many panel studies of the MLDA (including Eisenberg, 2003, and Polnicki, Gruenwald, and LaScala, 2007), have re-estimated their specifications on an age group slightly above the drinking thresholds, often finding little effect in that group. Grant (2010a), in contrast, employs control groups based both on age and time-of-day to demonstrate bias in panel estimates of the effect of ZT laws fully sufficient to account for the entire coefficient. For .08 laws, age-related control groups are not feasible, but time-of-day groups are, under the supposition that nighttime accidents are much more likely to be affected than daytime accidents.

The primary differences in these two approaches, therefore, have less to do with the analytical structure that is used than with details of their implementation. Of these, three are worthy of note.

Most important is the data used in each. The early use of quasi-experimental techniques is primarily by necessity: one wishes to identify the effect of a law when it is been in effect for just a few states for just a few years. Thus, even if the estimator is unbiased, the effect it identifies is the short-run effect in early-adopting states–that is, the "early early-adopter effect." Panel methods identify the long run effect in all states. Above we demonstrated the difference in the laws' putative effects in early-adopting and late-adopting states. But also, as Ross (1982) has demonstrated and Howland (1988) has stressed, the "effects of deterrence laws are evanescent." Thus, even if the *method* is unbiased, the effect that it estimates need not accurately forecast the long term effect of the law, even within those early adopting states. Supplementary regressions conducted by this author indicate that such an effect is present for the MLDA and ZT laws.

A second difference is the level of standardization. Panel techniques are relatively systematic, almost always studying long time periods across most or all states, identifying the coefficient via the specification above, and controlling for economic factors. Quasi-experimental techniques, on the other hand, admit a wider variety of methods and interpretations, which allows more opportunities for selectivity or subjectivity. A nice illustration occurs with O'Malley and Wagenaar (1991), which estimates the effect of the raised MLDA in thirteen law-changing states by comparing the before/after change in fatalities among the affected group with those in a control group. Their "aggregate estimate" (Table 5) for the "rate of crashes" is as follows: a 15% reduction in night crashes involving youth, a 14% percent reduction in daytime crashes involving youth, an insignificant 5% reduction in night crashes involving adults, and a 13% reduction in daytime crashes involving adults. Two issues arise, each of which vitally influences the final conclusion. First, are daytime crashes are a control group or part of the affected group? If it is the former, the net effect of the law is nil; if not, it is about 15%. Second, how should one compare youth night crashes to adult night crashes, since the 5% reduction in the latter group is insignificant? Using the point estimate, the estimate of effect is 15%-5% = 10%; assigning the insignificant estimate to zero, the estimate is 15%. Ultimately, the authors conclude (p. 488) that "aggregated across the several states...there was a decline of 15.4% in fatal crash rates involving drivers under 21 compared to a decline of only 5.4% involving drivers 21 and older," treating daytime crashes as a robustness check, not a control group, and use the point estimate for adult nighttime crashes as a point of comparison. It is very easy to find studies in the literature that make the opposite choices on both counts. Wagenaar and Maybee (1986), for example, do not report point estimates in control groups that are insignificant, while Williams et al. (1983) use daytime fatalities as a control group.

The final difference concerns inference. While the variance of ν is identified in equation 2, the variance of ξ is not identified in a before/after, quasi-experimental comparison. In practice, hypothesis tests are conducted assuming that $\mathrm{var}(\xi) = 0$. This is far from not accurate; Grant (2010b) demonstrates that the variances of ξ and ν are comparable. Panel regression methods do not suffer from this flaw; those that employ negative binomial regression (Dee and Evans, 2001, Grant 2010a) explicitly account for both sources of variation in the estimation technique. The upshot is that quasi-experimental estimates will be taken to be more precise than they really are.

In summary, quasi-experimental studies' findings should differ from those of panel studies in two ways. They should be more variable, because of the greater opportunity for researcher discretion, the smaller number of states analyzed, and the shorter time span of the data. Effect sizes may also be larger, because of researcher discretion, the "early early-adopter effect," or overstated significance.

Explicit vs. Implicit Controls. The other primary distinction in this literature is between the two regression-based approaches, pooled TSCS and panel. The key restriction here is, that if the vector X is complete, that the state and year effects can be replaced with a constant. (Some pooled TSCS analyses employ year fixed effects, but not state fixed effects, in which case only these can be replaced with a constant.) In particular, then, these studies claim one can adequately control for cross-section variation directly, without resorting to indirect state fixed effects.

The difficulties of adequately controlling for cross-sectional variation in fatalities are well-recognized in the literature, however.[3] Both physical geography (weather patterns, geological land features) and cultural geography (driving behavior, drinking sentiment) vary dramatically across states, affect accident rates significantly, and can be difficult to quantify directly. In consequence, pure cross-section regressions are quite rare–the last was published in the 1993. For some reason, however, this concern has not discouraged the publication (and frequent citation) of many pooled cross-section time-series regressions, though they suffer from this same flaw. To the extent that more safety-oriented states tended to be earlier adopters of traffic safety legislation, this omission will tend to bias coefficients in a favorable direction. To the extent variation in these controls influences estimates of γ, effect sizes will also be more variable.

Furthermore, as in the comparison above, there is no longer any dissonance between regression methods regarding the inclusion of directly measured, time-varying covariates–these are always to be found in panel regressions. In fact, the largest number of control variables utilized in a regression in this literature come from a panel study (Benson, Mast, and Rasmussen, 2000, and

[3] For example, cross-section studies are identified as low-quality in the influential review of Wagenaar and Toomey (2002).

associated publications by the same authors). Modern panel studies, therefore, utilize the strengths of all three study designs, which avoiding some of their competitors' weaknesses. In consequence, they are likely to be the most accurate estimators of laws' effects. They cannot be certified as unbiased, but are likely to yield less favorable estimates than will quasi-experimental methods and pooled TSCS analyses. The difference in effect sizes between pooled TSCS and quasi-experimental methods, however, cannot be predicted.

Study Design: Findings. The practical effect of these modeling choices is illustrated in Table 2, which identifies the average effect size estimated by each type of study design, the average publication date, and basic features about the data utilized. There is a steady progression in the publication dates of the different types of studies, time series coming about four years before pooled time series cross section, which in turn precedes panel studies by another four years or so, as noted previously. Along with this progression is a steady and sizeable increase in the geographical and temporal span of the data, and a steady decrease in the average effect size and the within-type standard deviation of the estimates. This suggests that the movement toward identification based on within-state variation in laws and outcomes, coupled with a large increase in the amount of data analyzed, leads to smaller estimated effect sizes. These differences are also substantial, ranging from five to fifteen percentage points, more than can be explained by the "early-adopter" effect alone.

VI. Conclusions and Prescriptions.

The basic pattern documented by this paper is consistent across all three laws studied: the minimum legal drinking age, zero tolerance, and the .08 per se BAC limit. In each case, early estimates of the effects of the law in the states that first adopt it are very favorable. But later estimates, which employ more data, use improved methods, include states that adopt the law "involuntarily," and estimate long run rather than short run effects, are much less favorable. The bottom line is that the results of early studies are poor predictors of the ultimate effect of the law, once adopted nationwide.

This pattern results, in part, from a tradeoff between the quality of the analysis that is conducted and the rapidity and ease with which it can be executed. Early studies must draw conclusions from the experiences of just a few states. This necessarily reduces the precision of the estimate and, in time series analyses using little data, makes it difficult to directly control for extraneous factors. With the diffusion of the law throughout the country and the collection of substantial data on pre-law and post-law outcomes, more demanding analyses can be conducted, but this process takes two or three decades to consummate. This scenario unfolds, and is institutionalized to a certain extent, through two literatures, one in health and safety journals, which dominate the early studies, and the other in economics and policy sciences, which dominate the later studies. The (modest) segmentation of these literatures has helped sustain the status quo and retard the reconciliation of their conflicting results.

In fact, this tradeoff–and segmentation–extends further than just the academic literature. While the panel and pooled TSCS regressions in this literature are almost always produced by academics and refereed, quasi-experimental studies are also produced by state agencies, by NHTSA, or by their contractors. These studies, which may consist of little more than a before-after comparison of fatalities relative to a control group, are often not refereed, but they are available to legislators at the time of decision making and possess credibility within the political arena that is not greatly dependent on their methodological rigor (Grant, 2010d). This independence, along with the segmentation of these agencies and the academic community, again helps sustain the status quo and prevent reconciliation of conflicting estimates in the long term.

As a result, the opportunity cost of not conducting a weaker refereed study is not waiting for a better study, but relying on an even weaker study that is not refereed. The only practical solution is to determine an empirical method that is less likely to yield biased estimates for early adopting states, that does not yield highly variable estimates, and that can be executed reasonably rapidly. Economists' methodological prowess has not been applied to this sticky problem–virtually every analysis conducted by economists involves fairly straightforward applications of basic panel regression methods. (The most technically sophisticated work in this literature, by DuMouchel, Williams, and Zador, 1987, while published in an economics journal, was not conducted by economists.)

There are two lines of attack. Regarding the dependent variable, researchers have not compared the relative merits of two options: the *frequency* of drunk driving accidents versus the *relative frequency* of those accidents, that is, the fraction of accidents involving drinking drivers. Currently almost all studies analyze the number of accidents, while Grant (2010b) argues that relative frequency is preferred. Second, regarding the independent variables, the optimal practical method of controlling for extraneous factors can be determined. At present, the literature contains a variety of methods, including matched comparison groups, time series analysis, and direct measurement of control variables, whose relative merits have not been carefully assessed. These developments could help us better predict drunk driving laws' ultimate effects based on the early experiences of early-adopting states, those laboratories of democracy.

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Figure 1. Visual Model of the Life Cycle of a Typical Literature on the Effects of Drunk Driving Legislation.

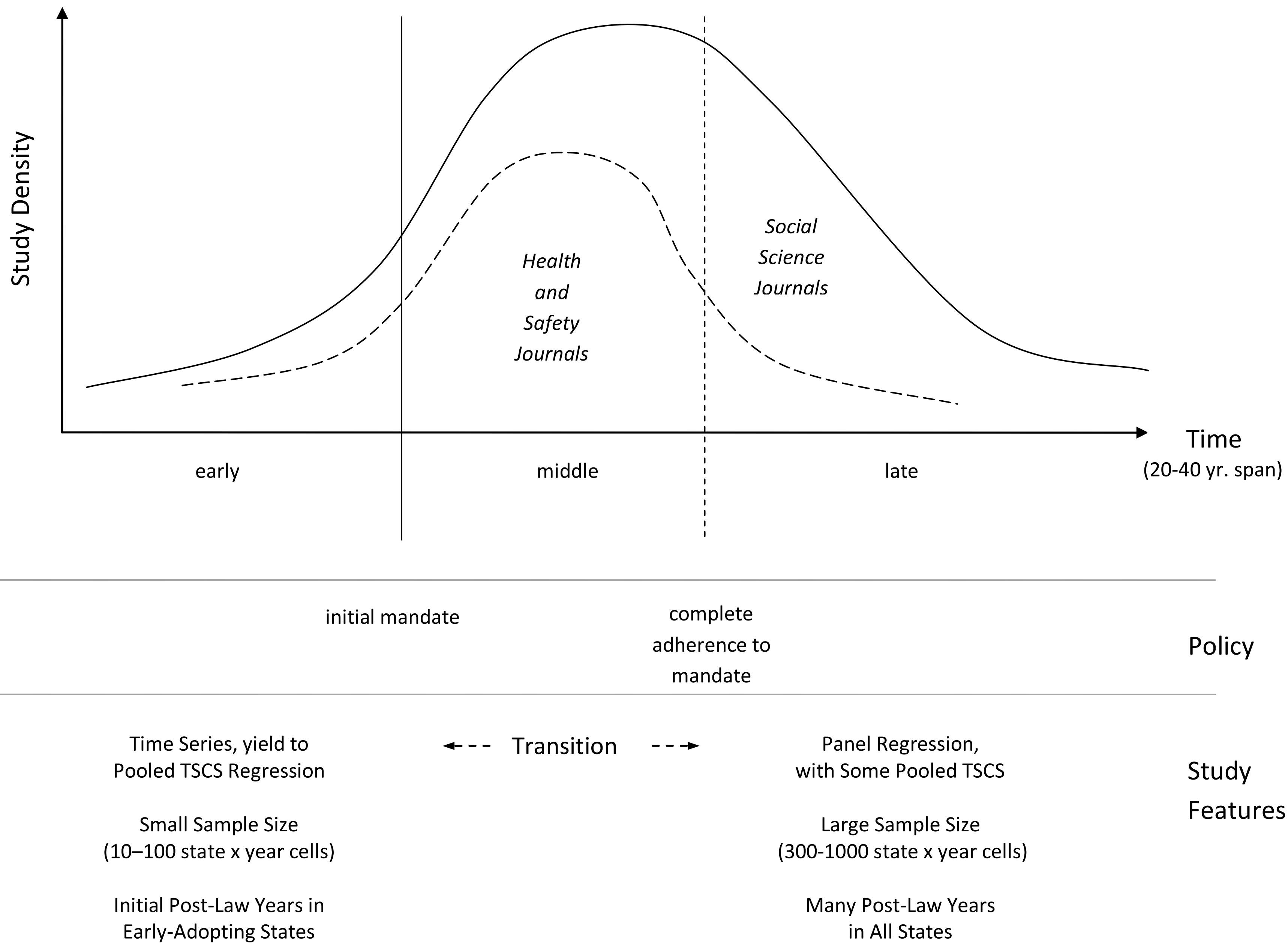


Figure 2. Academic Studies of the Minimum Legal Drinking Age Bubble Plot. (For description, see the text.)

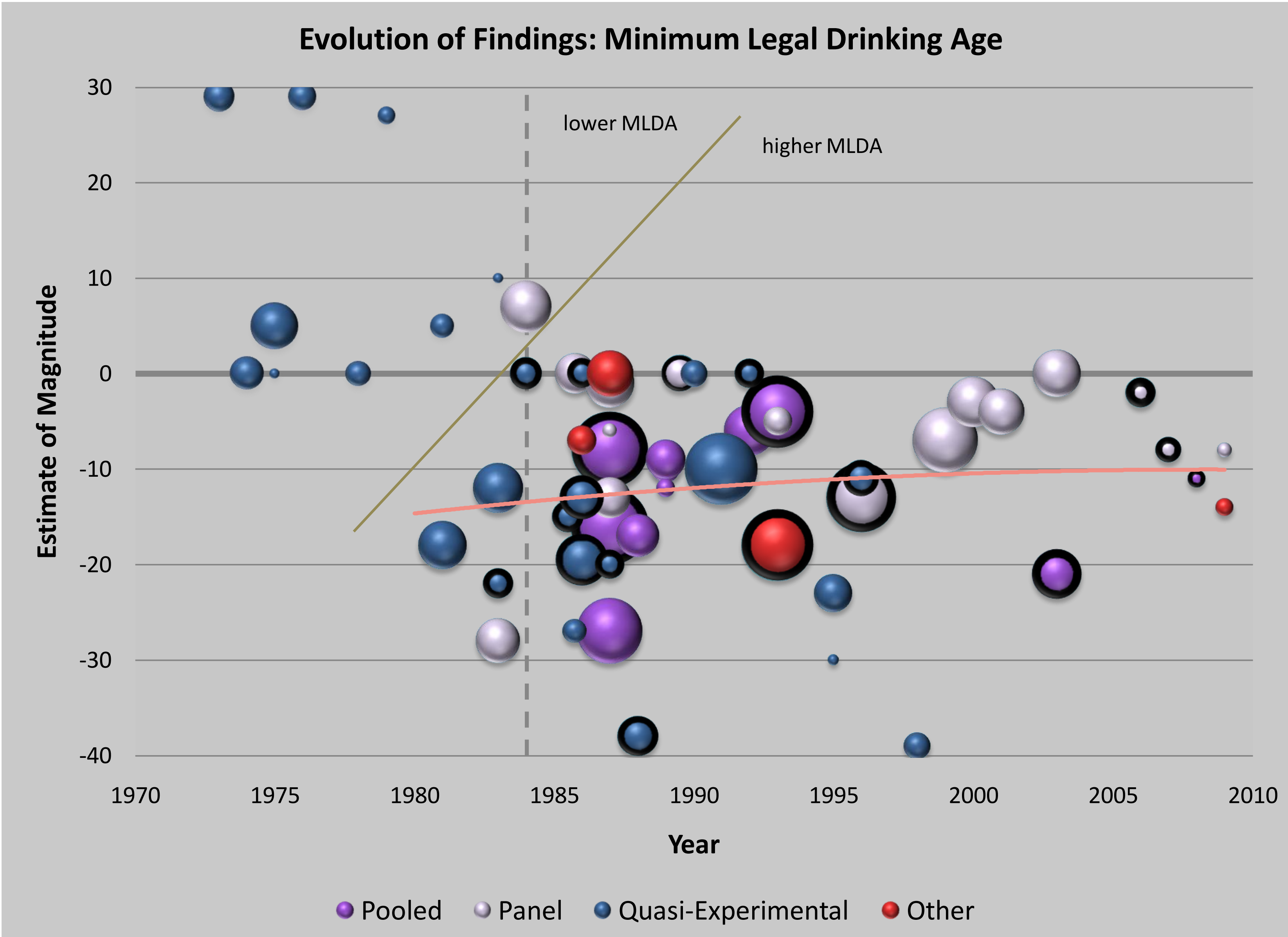


Figure 3. Academic Studies of Zero Tolerance Laws Bubble Plot. (For description, see the text.)

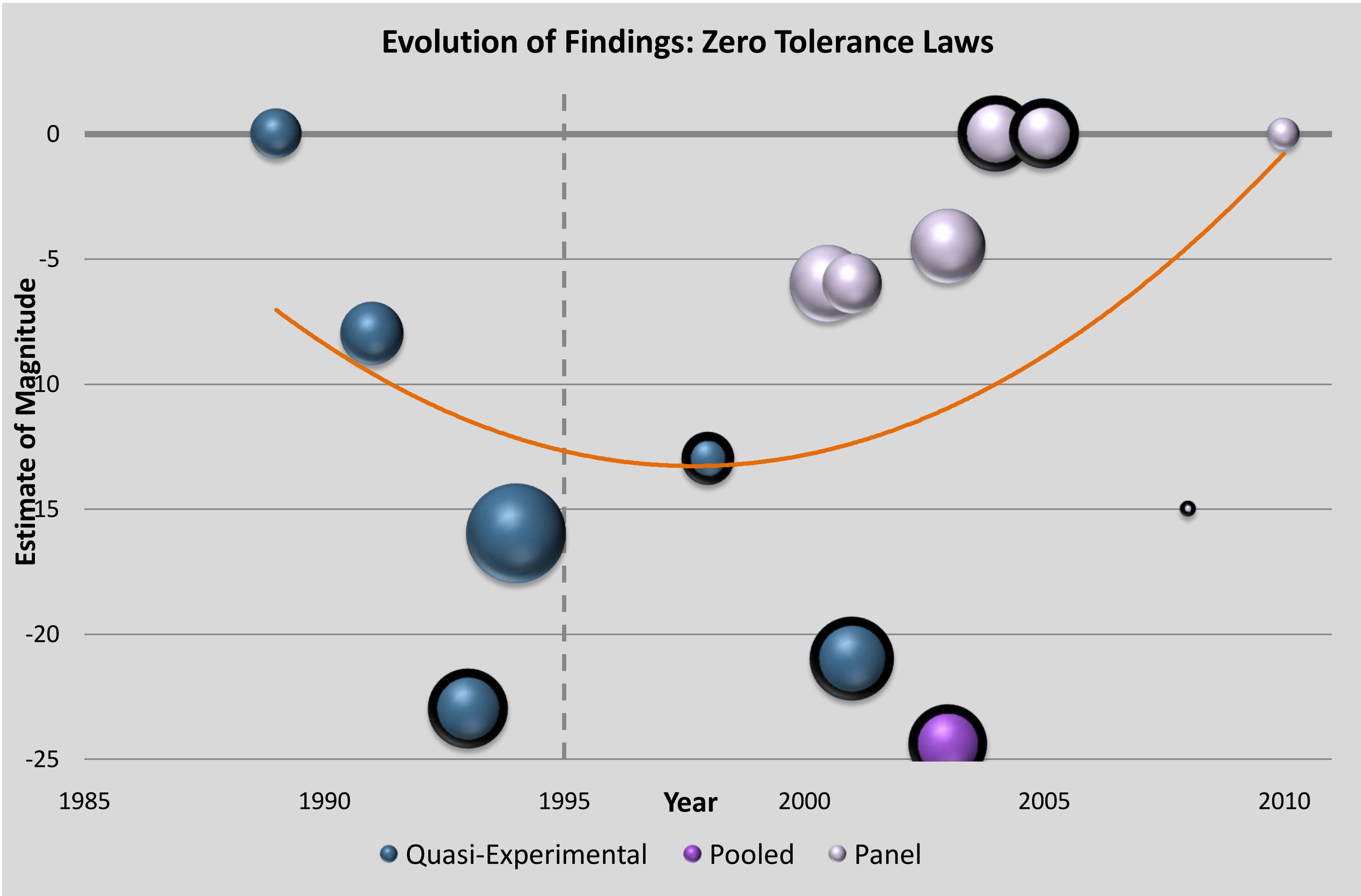


Figure 4. Academic Studies of .08 Laws Bubble Plot. (For description, see the text.)

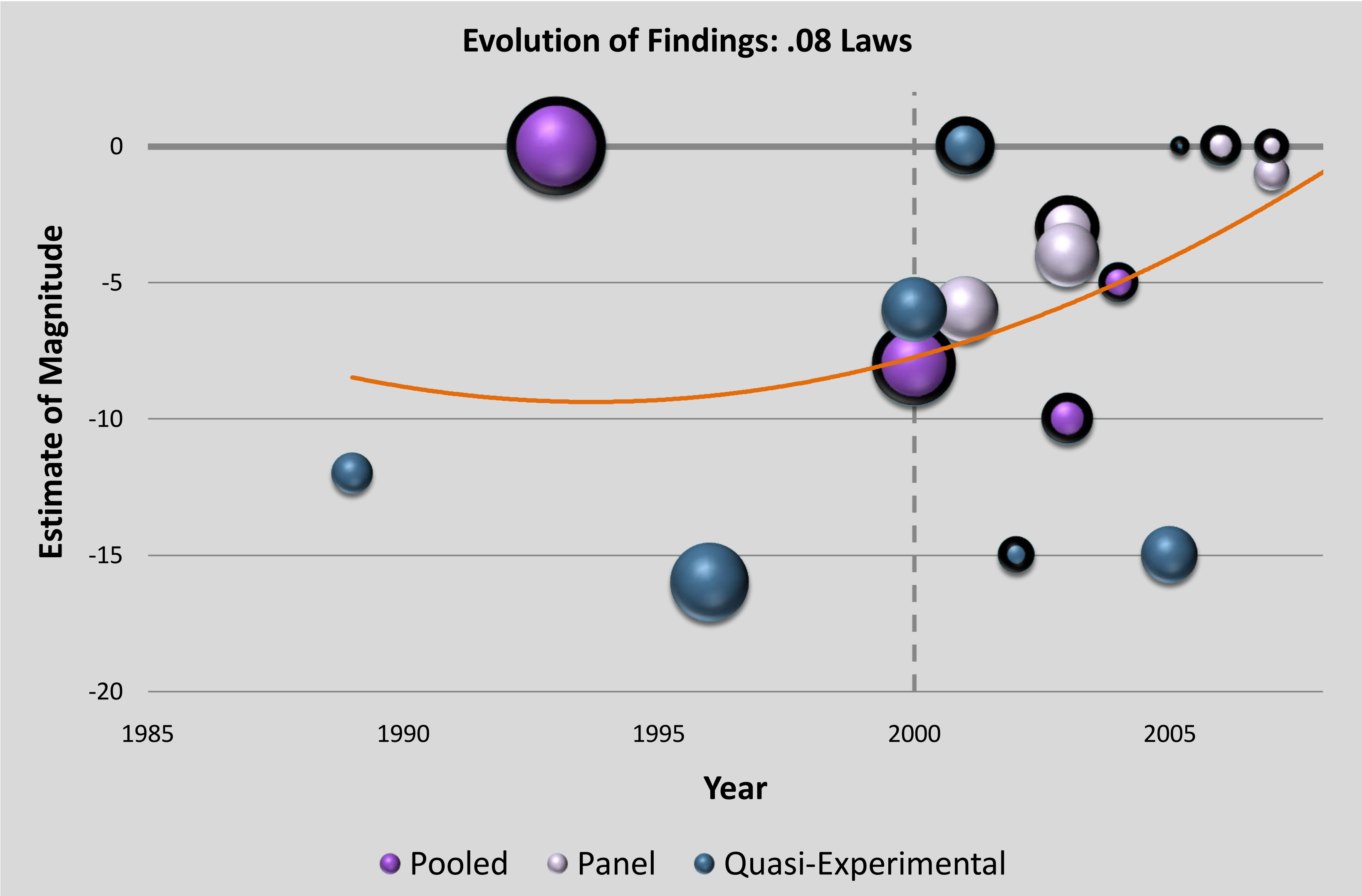


Figure 5. Timing of MLDA Adoption and Estimated Effect Size. (For description, see the text.)

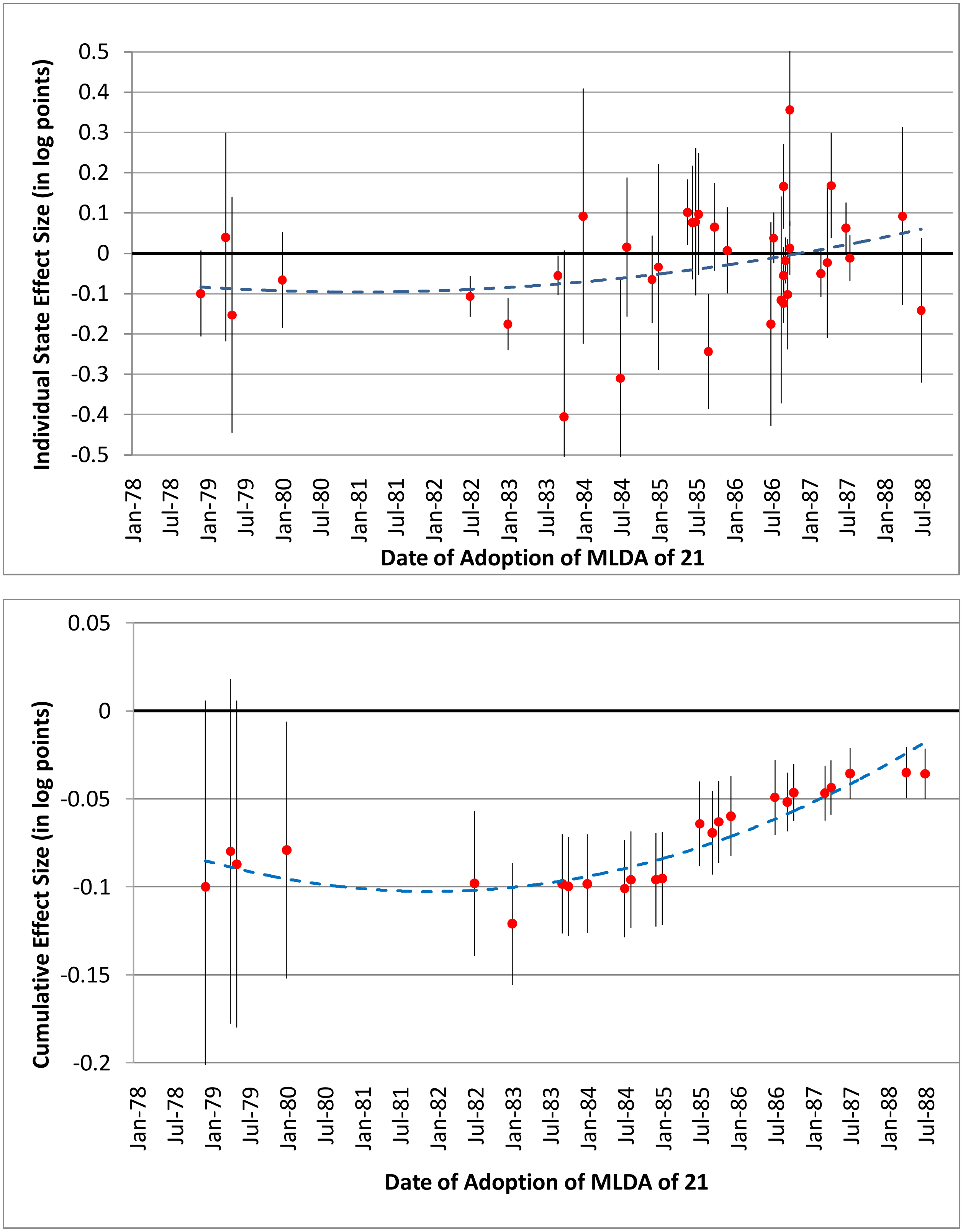
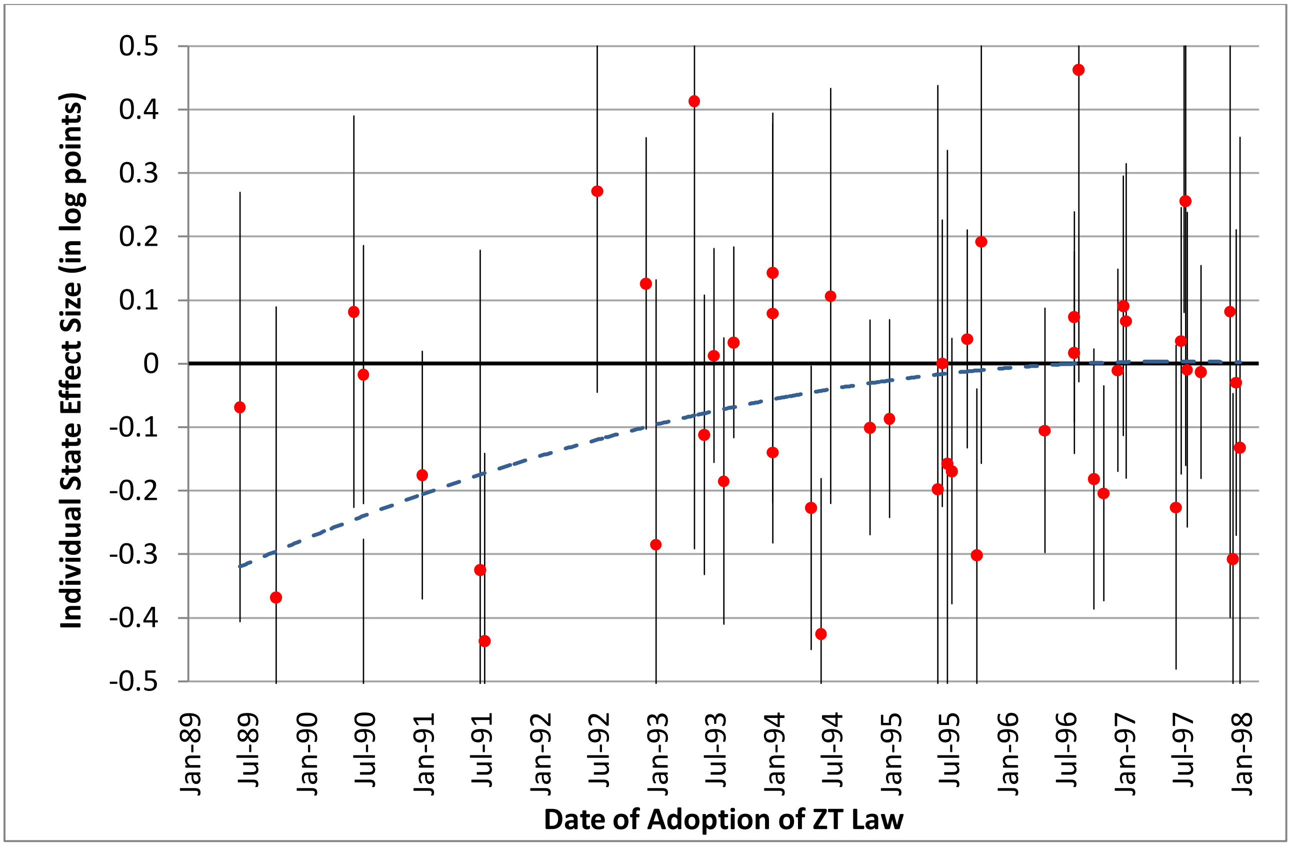


Figure 6. Timing of ZT Adoption and Estimated Effect Size. (For description, see the text.)



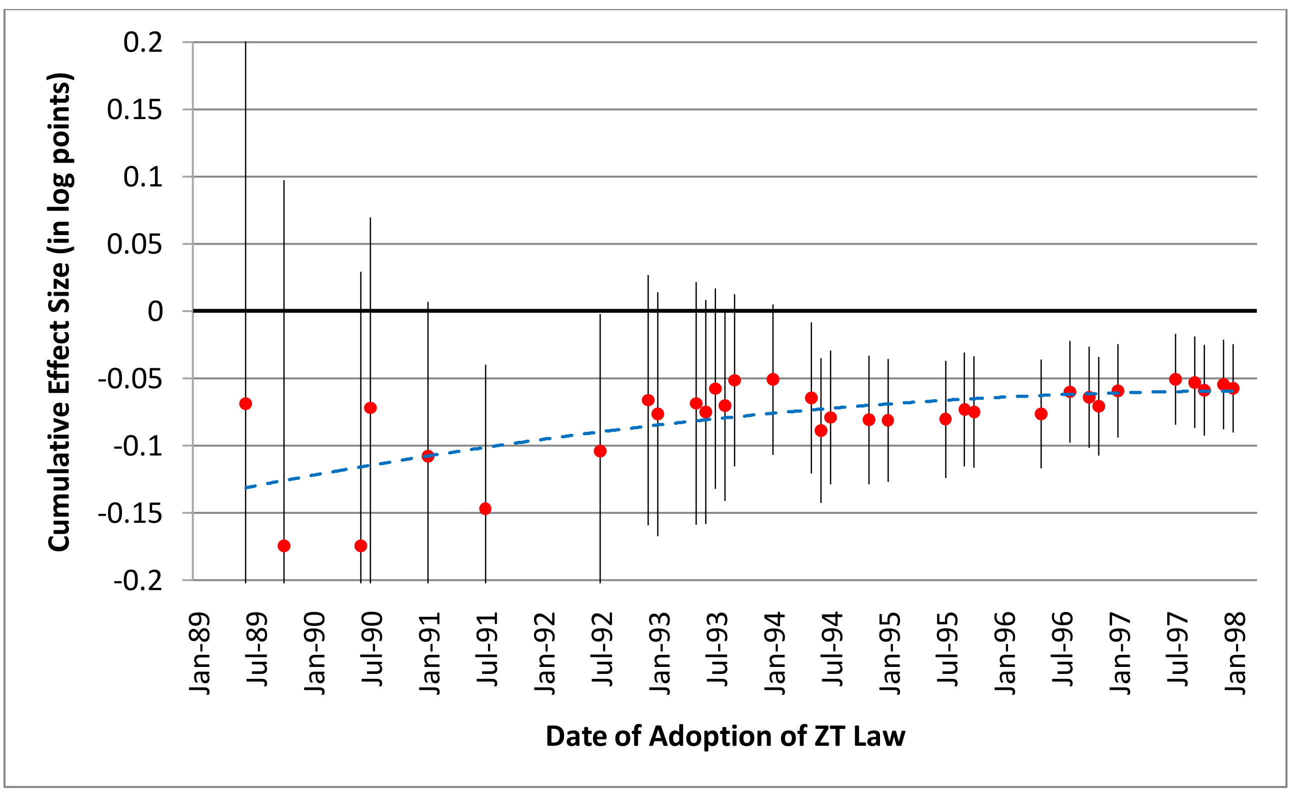
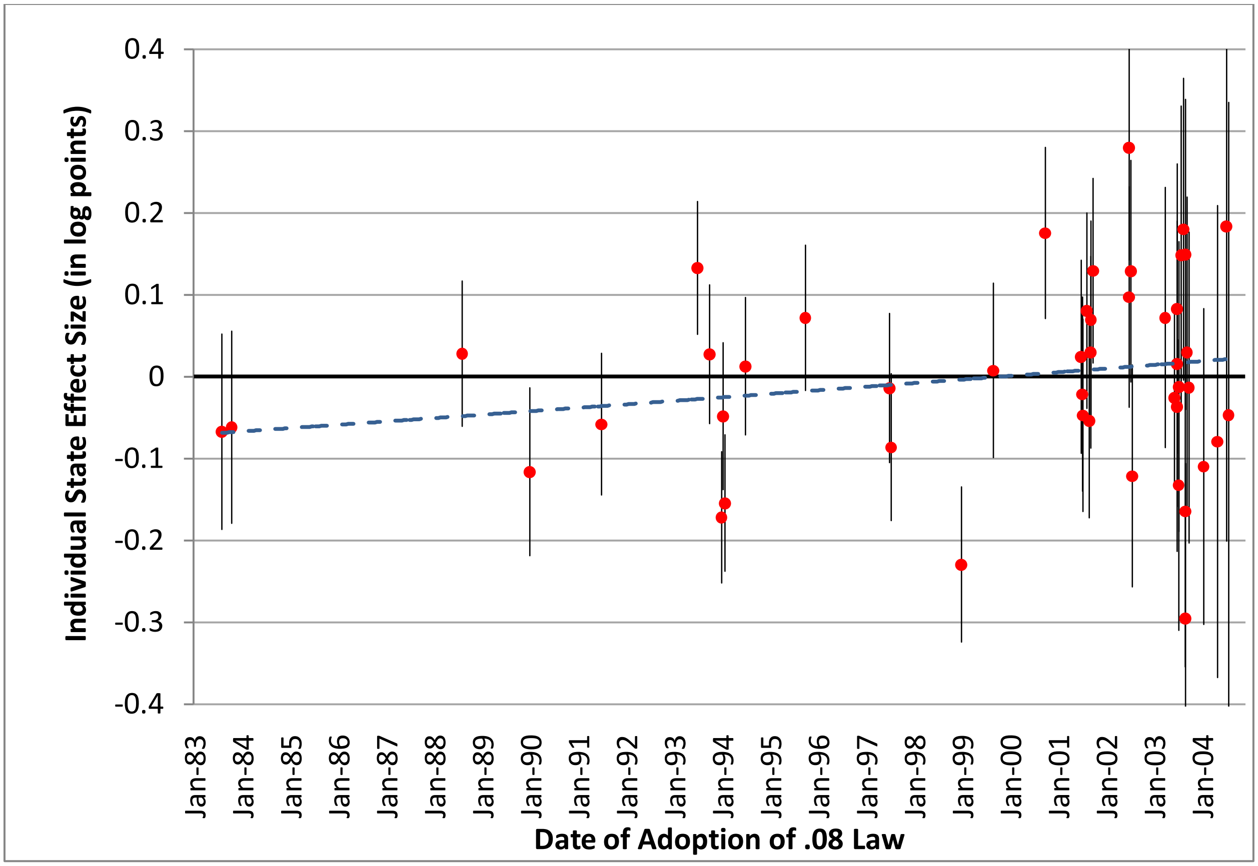


Figure 7. Timing of .08 Law Adoption and Estimated Effect Size. (For description, see the text.)



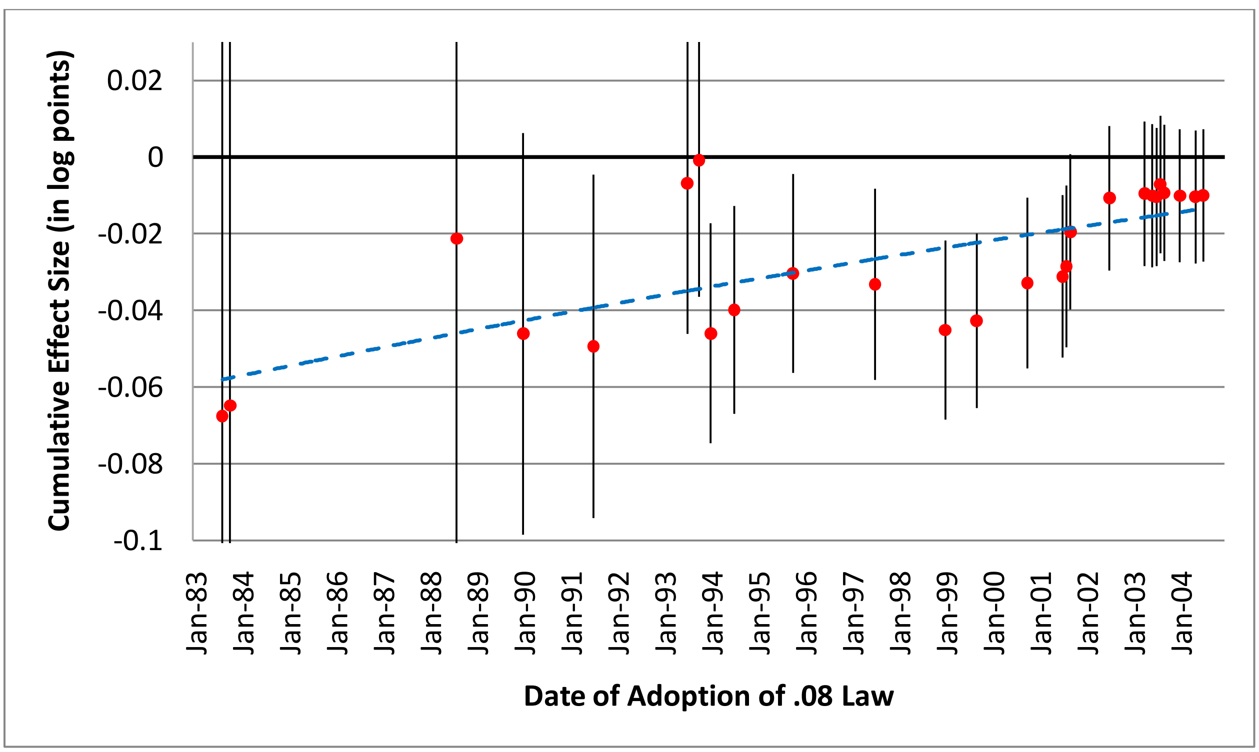


Table 1. Early / Late Study Characteristics.

	MLDA		ZT		.08	
	Early	Late	Early	Late	Early	Late
Number of Studies	34	36	5	9	9	6
Percent in Social Science Journals	32%	64%	0%	78%	33%	33%
Study Design (Number of Studies)						
Quasi-Exp.	17	10	4	1	3	2
Pooled TSCS	5	11	1	1	4	1
Panel	6	13	0	7	2	3
Other	6	2	0	0	0	0

Note: Early studies are published within three years after the financial incentive for states to pass these laws passed Congress. The remainder are classified as late.

Table 2. Elementary Study Features.

	Quasi-Experimental	Pooled Time Series Cross Section	Panel
Average Effect Size (Stand. Dev):			
MLDA	15.1	11.7	7.8
	(15.7)	(10.5)	(6.8)
ZT	13.1	20.0	4.5
	(9.6)	(15.7)	(5.4)
.08	7.2	6.2	2.5
	(7.6)	(6.9)	(2.3)
Average Publication Year:			
MLDA	1985.0	1989.4	1996.1
ZT	1994.4	1998.5	2004.6
.08	2002.8	1999.2	2004.5
Average Number of States Included:			
MLDA	1.2	35.3	42.6
ZT	9	26.5	47.1
.08	7.6	35.6	49
Average Number of Aggregate Pre-Law / Post-Law Years:			
MLDA	7 / 4	107 / 65	136 / 219
ZT	65 / 38	365 / 110	373 / 220
.08	50 / 32	118 / 67	375 / 175

Appendix Table 1. Studies of Changes in the Minimum Legal Drinking Age.

Study	Jurisdiction	Data Source	Sample Period	Design	Control Group	Effect
Asch and Levy, 1987	50 States	FARS	1978	Cross Section	No	Not Sig.
Asch and Levy, 1990	47 States	FARS	1975-1984	Panel	Yes	Not Sig.
Brown and Maghsoodloo, 1981	AL	FARS	1972-74& 1976-1979	Quasi – Exp.	Yes	-5%
Chaloupka, Saffer, and Grossman, 1993	48 States	FARS	1982-1988	Pooled TSCS	Yes	-4%
Coate and Grossman, 1987	50 States	State data	1975-1981	Pooled TSCS	Yes	-8%
Colon, 1984	51 States	FARS	1976	Cross Section	No	Not Sig.
Colon and Cutter, 1983	51 States	FARS	1976	Cross Section	No	-15%
Cook and Tauchen, 1984	48 States	State data	1970-1977	Panel	Yes	-7%
Davis and Reynolds, 1990	NY	Survey	1985-1986	Quasi – Exp.	No	+3%
Decker, Graitcer, and Schaffner, 1988	TN	FARS	1980-1986	Quasi – Exp.	Yes	-38%
Dee and Evans, 2001	48 States	FARS	1977-1992	Panel	Yes	-3% - 5%
Dee, 1999	48 States	Mon. the Future	1977-1992	Panel	Yes	-7%
Dobkin and Carpenter, 2009	50 States	Vital Statistics	1997-2005	Panel	No	-14%
Douglas and Millar, 1979	MI	Police crash data	1968-1975	Quasi – Exp.	Yes	-27%
DuMouchel, Williams, and Zador, 1987	26 States	FARS	1975-1984	Panel	Yes	-8% - 18%
Durrant and Legge, 1993	MI	FARS	1975-1987	Quasi – Exp.	No	-19%
Eisenberg, 2003	50 States	FARS	1982-2000	Panel	Yes	Not Sig.
Engs and Hanson, 1986	72 colleges	Survey	1984-1985	Cross Section	Yes	-7%
Engs and Hanson, 1988	56 colleges	Survey	1982-83, 1984-85,	Pooled TSCS	No	-17%

			1987			
Fell et al., 2000	37 States	FARS	1982-1990	Pooled TSCS	No	-11%
Ferreira and Sicherman, 1976	MA	Motor vehicles registry	1969-1973	Quasi – Exp.	Yes	-40%
Figlio, 1995	WI	State data	1976-1993	Quasi – Exp.	Yes	-19% - 26%
Fowles and Loeb, 1995	Pooled States	National Safety Council	1952-1991	Quasi – Exp.	No	+30%
Hammond, 1973	MI	State police data	1971-1972	Quasi – Exp.	No	-114%
Hingson et al., 1983	MA	Survey	1979-1981	Quasi – Exp.	Yes	-12%
Hoskin, Yalung-Mathews, and Carraro, 1986	10 States	FARS	1975-1978	Pooled TSCS	Yes	-27%
Houston, Richardson, and Neeley, 1995	50 States	FARS	1967-1991	Pooled TSCS	No	-2%
Houston, Richardson and Neeley, 1996	50 States	FARS	1975-1991	Pooled TSCS	No	Not Sig.
Hughes and Dodder, 1992	OK	Survey	1981-1984	Quasi – Exp.	No	Not Sig.
Hughes and Dodder, 1986	OK	Survey	1983-1984	Quasi – Exp.	No	Not Sig.
Joksch and Jones, 1993	18 States	FARS	1980-1987	Panel	Yes	-5%
Jones, Pieper, and Robertson, 1992	50 States	National Center for Health Statistics	1979-1984	Pooled TSCS	No	-6%
Kenkel, 1993	50 States	Survey	1985	Cross Section	No	-14% - 21%
Klepp, Schmid, and Murray, 1996	MN	Survey	1987-1988	Quasi – Exp.	Yes	-11%
Legge and Park, 1994	50 States	FARS	1980, 84, 87	Pooled TSCS	No	Not Sig.
Legge, 1990	NY	FARS	1975-1987	Quasi – Exp.	No	Not Sig.

Lillis, Williams, and Williford, 1987	NY	Survey	1982-1983	Quasi – Exp.	Yes	-20% - 25%
Loeb, 1987	51 States	Highway Statistics	1979	Cross Section	Yes	Not Sig.
MacKinnon and Woodward, 1986	IL, MA, MI	FARS	1975-1981	Quasi – Exp.	Yes	-15%
Maisto and Rachal,1980	28 States	Survey	1978	Cross Section	No	Not Sig.
Males, 1994	14 States	FARS	1975-1983	Panel	Yes	Not Sig.
Mast, Benson, and Rasmussen, 1999	48 States	FARS	1984-1992	Panel	No	-5% - 8%
Miron and Tetelbaum, 2009	48 States	FARS	1976-2005	Panel	No	-8% - 11%
Naor and Nashold, 1975	WI	Blood alcohol program	1968-1973	Quasi – Exp.	Yes	Not Sig.
O'Malley and Wagenaar, 1991	13 States	Survey	+/- 3 years of MLDA change	Panel	Yes	-10%
Orsak, 1983	TX counties	TX DOT	1970-1977	Quasi – Exp.	No	-10%
Polnicki, Gruenwald, and LaScala, 2007	48 States	FARS	1975-2001	Panel	Yes	-8%
Robertson, 1989	31 States	FARS	1982 & 1984-1986	Pooled TSCS	No	-22% - 58%
Ruhm, 1996	48 States	FARS	1982-1988	Panel	No	-5%
Saffer and Chaloupka, 1989	48 States	FARS	1980-1985	Pooled TSCS	No	-2% - 4%
Saffer and Grossman, 1987a (beer)	48 States	FARS	1975-1981	Pooled TSCS	Yes	-4% - 7%
Saffer and Grossman, 1987b	48 States	FARS	1975-1981	Pooled TSCS	Yes	-6% - 12%
Smith et al., 1984	MA	Survey	1979-1981	Quasi – Exp.	Yes	Not Sig.
Voas, Tippetts, and Fell, 2003	51 States	FARS	1982-1997	Pooled TSCS	No	-21%
Wagenaar, 1981	MI	State data	1972-1979	Quasi – Exp.	Yes	-18%
Wagenaar, 1986	MI	Police crash data	1976-1984	Quasi – Exp.	Yes	-16%
Wagenaar, 1983	ME	State data	1972-1979	Quasi –	Yes	-22%

				Exp.		
Wagenaar and Maybee, 1986	TX	State data	1978-1984	Quasi – Exp.	Yes	-11% - 14%
Weinstein, 1987	48 States	State data	1970-1977	Panel	No	-6%
Wilkinson, 1987	51 States	FARS	1976-1980	Panel	No	-1%
Williams, Rich, Zador, and Robertson, 1975	MI, WI	State police reports	1967-1973	Pooled TSCS	Yes	-5%
Williams, Zador, Harris, and Karpf, 1983	9 States	FARS	1975-1980	Panel	Yes	-28%
Womble, 1989	13 States	Study by Arnold	1975-1986	Pooled TSCS	Yes	-12%
Young and Beilinska-Kwapisz, 2006	48 States	FARS	1982-2000	Panel	No	-1% - 3%
Young and Likens, 2000	48 States	FARS	1982-1990	Panel	No	-3%
Yu, 1995	NY	State data	1978-1988	Quasi – Exp.	No	-30%
Yu and Shacket, 1998	NY	Survey	1982, 83, 85, 86, 96	Quasi – Exp.	Yes	-46% - 84%
Zylman, 1974	MI	State police data	1971-1973	Quasi – Exp.	No	Not Sig.
Zylman, 1978	ME, MA	State data	1963-1974	Quasi – Exp.	No	Not Sig.

Appendix Table 2. Studies of Zero Tolerance Laws.

Study	Jurisdiction	Data Source	Sample Period	Design	Control Group	Effect
Blomberg, 1993	MD	Survey	1985-1990	Quasi – Exp.	No	-24%
Carpenter, 2004	50 States	Survey	1984-2001	Panel	Yes	Not Sig.
Dee, 2001	48 States	FARS	1982-1998	Panel	No	-6%
Dee and Grabowski, 2005	51 States	FARS	1992-2002	Panel	No	Not Sig.
Dee and Evans, 2001	48 States	FARS	1977-1992	Panel	No	-6%
Eisenberg, 2003	50 States	FARS	1982-2000	Panel	Yes	-5%
Grant, 2010	51 States	FARS	1988-2000	Panel	Yes	Not Sig.
Hingson, and Morelock, 1989	ME	Survey	1983-1986	Quasi – Exp.	Yes	Not Sig.
Hingson, Heeren, and Winter, 1994	12 States	FARS	1975-1992	Panel	No	-16%
Hingson, Heeren, and Winter, 1991	ME, NC, NM, WI	FARS	1977-1988	Quasi – Exp.	Yes	-8%
Liang and Huang, 2008	41 States	Survey	1993, 1997, 1999	Pooled TSCS	Yes	-15%
Voas, Lange, and Tippetts, 1998	CA, TX, CO, NV, WY	FARS and Survey	1988-1996	Quasi – Exp.	Yes	-13%
Voas, Tippetts, and Fell, 2003	51 States	FARS	1982-1997	Pooled TSCS	No	-24%
Wagenaar, O'Malley, and LaFond, 2001	30 States	Survey	1984-1998	Quasi – Exp.	No	-21%

Appendix Table 3. Studies of .08 Laws.

Study	Jurisdiction	Data Source	Sample Period	Design	Control Group	Effect
Bernat, Dunsmuir, and Wagenaar, 2004	18 States	FARS	1983-2000	Pooled TSCS	No	-5%
Chaloupka, Saffer, and Grossman, 1993	48 States	FARS	1982-1988	Pooled TSCS	No	Not Sig.
Dee, 2001	48 States	FARS and state data	1982-1998	Panel	No	-6%
Eisenberg, 2003	51 States	FARS	1982-2000	Panel	No	-3%
Foss, Stewart, and Reinfurt, 2001	NC	NC DMV files	1991-1995 (monthly)	Quasi – Exp.	No	Not Sig.
Freeman, 2007	48 States	FARS	1980-2004	Panel	No	0-2%
Gorman, Huber, and Carozza, 2006	TX	FARS and TXDOT	1995-2000	Quasi – Exp.	No	Not Sig.
Hingson, Heeren, and Winter, 2000	KS, NC, FL, NM, NH, VG	FARS	1988-1998	Quasi – Exp.	Yes	-6%
Hingson, Heeren, and Winter, 1996	UT, OR, MA, CA, VT	FARS	1976-1993	Pooled TSCS	Yes	-16%
Polnicki, Gruenwald, and LaScala, 2007	48 States	FARS	1975-2001	Panel	No	Not Sig.
Tippetts, Voas, Fell, and Nichols, 2005	18 States	FARS	1982-2000 (monthly)	Quasi – Exp.	No	-15%
Villaveces et al., 2003	51 States	FARS	1980-1997	Pooled TSCS	No	-10%
Voas, Tippetts, and Fell, 2000	51 States	FARS	1982-1997	Pooled TSCS	No	-8%
Voas, Tippetts, and Taylor, 2002	IL	FARS	1988-1998	Quasi – Exp.	Yes	-15%
Young and Beilinska-Kwapisz, 2006	48 States	FARS	1982-200	Panel	No	Not Sig.

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