AN EXAMINATION OF THE UNINTENDED CONSEQUENCES IN MOTORCYCLE HELMET LAWS AND ORGAN DONATION POLICY

By

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ABSTRACT

AN EXAMINATION OF THE UNINTENDED CONSEQUENCES
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Chapter 1: "The Effect of Economic Conditions on Organ Donation"

More than 100,000 individuals in the United States are currently on the organ transplant waiting list, yet surprisingly little is known about the factors that affect organ donation rates. In order to increase our knowledge about the supply of transplantable organs, this paper assesses the effects of economic conditions on deceased organ donations. Using data from the Scientific Registry of Transplant Recipients, I estimate that a 1 percentage-point increase in the unemployment rate is associated with a 3.5 percent decrease in the supply of organ donors killed in motor vehicle accidents. Despite evidence of strong cyclical patterns in deaths involving cerebrovascular disease, suicides, and homicides, no correlation was found between the unemployment rate and organ donation rates for these causes of death. Understanding cyclical fluctuations in donations will help policy makers and transplant specialists design policies aimed at increasing the supply of organs by enhancing their knowledge of expected donors.

Chapter 2: "Not on My Back Roads: Are there Spillover Effects in Motorcycle Helmet Legislation?"

Previous research on the effectiveness of motorcycle helmet laws has found a roughly 30 percent increase in fatalities following the repeal of a universal helmet law. This study examines how motorcycle helmet laws in neighboring states affect fatality rates. Enactment of a universal helmet law creates incentives for motorcyclists in the state to avoid the helmet mandate by riding

in other states with the more lenient laws. If accidents involving residents of other states lead to increased expenditure, governments may be justified in eliminating crossover incentives by imposing helmet laws within their borders. I find that including controls for the helmet laws of neighboring states has no effect on fatality rates, nor does it change the estimated effectiveness of a statewide motorcycle helmet mandate. In addition, I find that neither a state's own helmet law, nor those of its neighbors affect the fraction of motorcyclist fatalities involving non-residents. Lastly, I find own-state helmet laws are associated with significant reductions in non-resident fatality counts, although the helmet laws of adjacent states remain insignificant. While cross-state spillover effects could be used to support strategic interaction among state governments, my examination of fatality patterns shows no evidence of a shift in motorcycle riding location based on the helmet laws of neighboring states.

Chapter 3: "Donorcycles: Motorcycle Helmet Laws and the Supply of Organ Donors"
(with Stacy Dickert-Conlin and Todd Elder)

Traffic safety mandates are typically designed to reduce the harmful externalities of risky behaviors. We consider whether motorcycle helmet laws also reduce a beneficial externality by decreasing the supply of viable organ donors. Our central estimates show that organ donations resulting from fatal motor vehicle accidents increase by 10 percent when states repeal helmet laws. Two features of this association suggest that it is causal: first, nearly all of it is concentrated among men, who account for over 90 percent of all motorcyclist deaths, and second, helmet mandates are unrelated to the supply of donors who die in circumstances other than motor vehicle accidents. The estimates imply that every death of a helmetless motorcyclist prevents or delays as many as 0.33 deaths among individuals on organ transplant waiting lists.

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Dedicated to the memory of my mother, San Juana Moore

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CHAPTER 1

THE EFFECT OF ECONOMIC CONDITIONS ON ORGAN DONATION

1.1 Introduction

As of November 2010, more than 109,000 individuals were on the organ transplant waiting list in the United States. Over the last ten years, an average of 6,730 people died while waiting for an organ annually and another 2,025 became too sick for transplantation.[1] The shortage of transplantable organs in the United States has attracted considerable attention among health professionals and policy makers. Understanding the determinants of organ donation rates will help in designing policies intended to increase the supply of transplantable organs. Because more than three-quarters of all transplanted organs come from deceased donors, events and policies that impact mortality rates are also likely to affect organ donation rates.

Using data from the Scientific Registry of Transplant Recipients (SRTR), I estimate the effect of the state-level unemployment rate on monthly organ donation rates by cause of death of the donor. I show that an increase in the unemployment rate is associated with decreases in organ donors due to motor vehicle accidents and cerebrovascular disease (stroke) and increases in donors from suicide and homicide. Specifically, I find that a 1 percentage-point increase in the unemployment rate is associated with a 3.5 percent decrease in organ donors from motor vehicle accidents. For those under the age of 18, the results are even stronger, as a 1 percentage-point increase in the unemployment rate is associated with a 9.1 percent decrease in organ donors from motor vehicle accidents.

These results are an extension of existing research finding that mortality rates exhibit a strong cyclical pattern (Ruhm 2000, Ruhm 2003, Ruhm 2005, and Miller et al. 2009), with differential effects by cause of death. Furthermore, the causes of death that are most responsive

[1] These numbers are taken from the Organ Procurement Transplantation Network (OPTN) national data available for public use on the OPTN's website: http://optn.transplant.hrsa.gov/latestData/step2.asp.

to changes in the unemployment rate are the same causes of death most likely to result in organ donation: those resulting in brain death. Using monthly data, rather than annual data used by previous authors, I find results consistent with the previous estimates: a 1 percentage-point increase in the unemployment rate is associated with decreases in motor vehicle accident and cerebrovascular disease fatalities of 2.5 percent and 1.6 percent, respectively. I also find that a 1 percentage-point increase in the unemployment rate is associated with increases in suicides and homicides of 1.2 percent and 4.2 percent, respectively. Together with the donation estimates, I estimate that each additional motor vehicle death caused by economic expansion results in 0.04 additional organ donors and 0.14 additional transplants.

In the following section, I elaborate on the effects of macroeconomic conditions on mortality and provide a brief overview of organ donation in the United States. Section 3 describes the organ donation data from the SRTR as well as the publicly available mortality data. Section 4 presents the empirical specifications and results, and Section 5 concludes.

1.2 Institutional Details

Macroeconomic conditions and mortality

Previous work on the effects of the business cycle on mortality rates (Ruhm 2000, Ruhm 2005, Miller et al. 2009) showed a strong procyclical trend in mortality rates. The results were substantively and statistically significant, as well as robust to changes in specification, estimation method, and decompositions by age and cause of death. When decomposed by cause of death, eight out of ten categories examined by Ruhm (2000) were found to be procyclical, with motor vehicle accident (MVA) deaths exhibiting the strongest correlation with the unemployment rate. The mechanism behind the procyclicality of mortality rate is not fully understood, especially the strong patterns found among the elderly, who should not be personally affected by changes in the unemployment rate. The procyclicality in mortality for working-age adults is driven by motor vehicle accidents, and the estimates are consistent across all age groups. Based on this, Miller et al. (2009) suggest business-cycle externalities, such as increases in road traffic, rather than changes in individual behavior, are the major factor for working-age adults.

Patterns seen in the age- and cause-of-death-specific regressions set the foundation for the connection between unemployment rates and organ donation. Over 90 percent of deceased organ donors are brain dead at the time of recovery, meaning they have irreversible cessation of all brain function.[2] Ruhm (2000) and Miller et al. (2009) show mortality rates for the causes of death most likely to result in brain death, such as accidents and suicides, are very prone to changes in the unemployment rate. Fatal motor vehicle accidents, in particular, represent over 21 percent of all deceased organ donors since 1994. Suicides, which Ruhm (2000) found to be significantly countercyclical, represent nearly 8 percent of deceased organ donors. Moreover, donations due to suicide are of particular interest, as Figueiredo et al. (2007) showed that families are more likely to agree to donation consent in instances of suicide.

Organ donation in the United States

While roughly 2.4 million people die in the United States each year, less than 1 percent will be due to brain death. Brain death occurs when injuries lead to as the irreversible cessation of all brain function. Medical personnel are then able to keep the heart beating with mechanical ventilation and medication, allowing the internal organs to receive oxygenated blood and remain viable for transplantation. In contrast, organs deteriorate rapidly following cardiac deaths,

[2] Source: Author's calculation from SRTR data. The remaining cases are donations after cardiac death. See http://www.organtransplants.org/understanding/death/ for more details.

making them less suitable for transplantation. In addition to suffering brain death, potential donors must be free of contraindications for organ donation. Contraindications are conditions that prevent the organs from safely being used in transplantation, such as active cancer, HIV, and other infectious diseases.[3] In the absence of these contraindications, Organ Procurement Organization (OPO) staff will seek consent for organ donation.[4]

Organ donation in the United States is an informed-consent system, meaning organs are recovered from a potential donor only if explicit permission has been granted to do so.[5] Consent for donation must be obtained either from the individual prior to death or from surviving family members. Although federal law has always maintained that health care professionals only need the donor's consent to begin recovery, actual OPO practices have been very different. Until recently, health care professionals requested consent from the potential donor's next-of-kin before beginning organ recovery, regardless of documentation of the donor's own wishes.[6] Starting in 1998, individual OPOs began enforcing the wishes of the deceased regardless of family opinion, a practice known as first-person consent, in an effort to respect the donor's wishes and increase the supply of organs. A survey of all OPOs conducted by Wendler and Dickert (2001) found "widely divergent consent practices" among OPOs, noting that the

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[3] A complete list of contraindications for organ donation can be found in Table 2 of Guadagnoli et al. (2003).

[4] OPOs are regional members of the OPTN responsible for organ services within a designated area. There are 58 current OPOs throughout the United States and its territories.

[5] This is in contrast to a presumed-consent system, used in many European countries, where all individuals are presumed to have consented to organ donation unless there is specific evidence that the individual has chosen to "opt-out" of donation. See Abadie and Gay (2006) for details about how organ donation rates are impacted by this decision.

[6] There are several mechanisms available to individuals wishing to designate oneself as a donor, including on a driver's license, signed donor card, state donor registry, living will, or through durable power of attorney. States differ, to a small degree, on which documents are accepted as legal consent for donation.

differences stemmed from ethical disagreements regarding whose wishes (the deceased or next-of-kin) should determine whether or not organs are recovered. Sheehy et al. (2003) noted that low consent rates are the major limiting factor to increasing the number of organ donors and found that the overall family consent rate from 1997-1999 was just 54 percent. Howard et al. (2007) report that by 2004, some OPOs had reached 60 percent conversion of potential donors. Once consent has been confirmed or obtained for the potential donor, surgeons initiate the process of organ recovery.

1.3 Data

Donation and transplantation services in the United States are coordinated and provided by the Organ Procurement Transplantation Network (OPTN), under directive from the 1984 National Organ Transplant Act. The OPTN is also responsible for collection and management of data from every donation and transplant occurring in the United States. This study uses data from the Scientific Registry of Transplant Recipients (SRTR), which includes data on all donors, wait-listed candidates, and transplant recipients in the United States, submitted by the members of the OPTN.[7] Beginning April 1st, 1994, the files also report the donor's cause of death, with separate categories for motor vehicle accidents, other accidents such as falls, child abuse,

[7] The data reported here have been supplied by the Minneapolis Medical Research Foundation (MMRF) as the contractor for the Scientific Registry of Transplant Recipients (SRTR). The interpretation and reporting of these data are the responsibility of the author and in no way should be seen as an official policy of, or interpretation by, the SRTR or the U.S. Government. The Health Resources and Services Administration (HRSA), U.S. Department of Health and Human Services provides oversight to the activities of the OPTN and SRTR contractors. For more information about the SRTR, please see http://www.srtr.org/.

suicide, homicide, and, as of October 25th, 1999, natural causes.[8] For the purposes of this study, the individual-level data were collapsed into state-level counts of deceased donors by month for the years 1994 through 2004.

Panel A of Table 1.1 displays summary statistics for organ donors by year and cause of death. Each cell represents the average number of donors per million persons across all 50 states (and D.C.) and 12 months of the year.[9] Before October 25th, 1999, donors dying of natural causes were coded as "None of the Above." Once the natural cause category was added, donations in the "None of the Above" category gradually, yet substantially decrease as health care professionals began incorporating the new category. Natural cause donors increased 22.3 percent from 2000 to 2004.

The mortality data comes from the National Vital Statistics Multiple Cause-of-Death Mortality Data, which I have compressed into monthly, state-level fatality counts using International Classification of Diseases (ICD) codes that match the cause of death found in the organ donor data.[10] In most cases, the match is straightforward; however, death by "natural causes" does not have a strict medical definition, and therefore does not appear in the ICD codes underlying the Vital Statistics mortality data. Fatalities coded as cerebrovascular disease, which include strokes and other instances of intracranial hemorrhage, were used to match with organ

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[8] The OPTN refers to these categories as the donor's circumstance of death. To avoid confusion, I will instead refer to them as the donor's cause of death.

[9] Due to limitations of the organ donor data, figures for 1994 are from April-December. The figure for "Natural Causes" in 1999 is from November-December.

[10] For the years 1994-1998, I use the following ICD 9th revision codes: MVA (E810-E825), suicide (E950-E959), homicide (E960-E978), and cerebrovascular disease (430-438). For the years 1999-2004, I use the following ICD 10th revision codes: MVA (V02-V04, V09.0, V12-V14, V19.0-V19.2, V19.4-V19.6, V20-V79, V80.3-V80.5, V81.0-V81.1, V82.0-V82.1, V83-V86, V87.0-V87.8, V88.0-V88.8, V89.0, V89.2), suicide (X60-X84, Y87.0), homicide (X85-Y09, Y87.1), and cerebrovascular disease (I60-I69).

donors in the natural cause category. While 75 percent of natural cause donors died of a stroke or other intracranial hemorrhage, this classification is admittedly less clean than the other categories of interest.[11] Panel B of Table 1.1 displays the corresponding average monthly mortality per million persons by year and cause of death from April 1994 to December 2004. Cerebrovascular deaths are three times more common than motor vehicle deaths, averaging 50.1 fatalities per month compared to an average 14.9 MVA fatalities per month. Despite this, the number of natural cause donors is similar to that seen in motor vehicle accidents. In addition to the relative magnitudes, it is also important to note that Table 1.1 shows a decreasing trend in the mortality rate for all four cause-of-death categories over this time period, including a 37 percent drop in homicides. While motor vehicle fatalities decreased over 9 percent from 1994-2004, MVA donors decreased just 4 percent. Table 1.1 illustrates differences in the probability that a fatality turns into an organ donation, even among the categories most likely to result in brain death.

Another simple way to evaluate differences in the probability of becoming an organ donor is by comparing the ratio of organ donors to mortality counts by cause of death. These results, which I will refer to as the yield rates, can be seen in Table 1.2. Sample means for monthly mortality levels are displayed in brackets below each estimate. Row 1 of Table 1.2 shows the yield rates for the pooled-age sample, aggregating over all years. Similar to the implicit pattern of Table 1.1, higher levels of organ donation can be seen following motor vehicle accidents, relative to the other causes of death. Frezza et al. (1999) previously noted that MVA deaths are associated with the highest conversion ratio from potential donor to actual

[11] Source: Author's calculation from OPTN data. Intracranial hemorrhage / Stroke is a category within the OPTN's mechanism-of-death variable, however, this mechanism also occurs in other circumstances of death, such as motor vehicle accidents.

donor, partly because time available for conversion for MVA deaths is greater than in those dying from anoxia.[12] Rows 2-6 of Table 1.2 show the yield rates separately by age group, illustrating that there are higher rates of organ donation when the deceased is younger. Younger individuals tend to have healthier bodies relatively speaking, with fewer occurrences of contraindications (Frezza et al. 1999). While the under 18 age group only represents about 13 percent (= 1.9051 / 14.9114) of all MVA deaths, the yield rate is nearly twice as high as any other age category. The yield rates are highest for those under 34 years old in the natural causes category, the sample sizes show the 65 and older age group represents over 88 percent (= 43.2185 / 49.0252) of all cerebrovascular deaths. With a large organ shortage, recent attention focuses on the use of more elderly donors, although disagreement still exists as to what conclusions can be drawn from the results. Karatzas et al. (2011) concluded that elderly donors do not affect patient and graft survival in many patients, while Di Cocco et al. (2011) and Cassini et al. (2010) found the use of elderly kidney donors had a significant negative effect on transplant survival.

Figures 1.1 through 1.4 show the national time trends of seasonally-adjusted mortality and the unemployment rate from 1994 through 2004 by cause of death.[13] Seasonally-adjusted data better illustrate the identification of the regression results to follow, which will include a full set of binary month indicators. The inclusion of individual month controls is important in analyzing correlations between the unadjusted unemployment rate and unadjusted mortality rates due to the large body of evidence that seasonal trends exist in mortality rates. Seasonal trends

[12] Marconi et al. (2011) and Singhal et al. (2009) conclude that donor cause of death was not a strong predictor of graft survival.

[13] The mortality data depicted are residuals from a regression of deaths on a full set of month dummies. While a large increase in homicides occurs in September 2001, the regression results that follow are not sensitive to inclusion of this month.

have been previously documented in cerebrovascular disease or strokes, consistently finding high mortality rates during winter months (Khan et al. 2005, Myint et al. 2007, Nakaji et al. 2004, Turin et al. 2009, Wang et al. 2003). Motor vehicle fatalities, on the other hand, have seasonal peaks in the summer months (Liu et al. 2005, Trudeau 1997). Research has also found seasonal patterns in suicide (Warren et al. 1983, Bridges et al. 2005) and homicide mortality (Cheatwood 1988, Warren et al. 1983, Tennenbaum and Fink 1994).[14] Despite the use of seasonally-adjusted data, the mortality counts and the unemployment rate in Figures 1.1 - 1.4 are still aggregated to the national level, resulting in a noisy representation of the state-level identification strategy to follow. Nevertheless, suicide mortality appears to have a procyclical trend while cerebrovascular disease mortality appears to have a countercyclical trend. Cyclical patterns for motor vehicle accidents and homicides cannot be easily identified in the figures. Figures 1.5 through 1.8 show the time trend of seasonally-adjusted deceased organ donations with the national unemployment rate over the same time period.[15] The cyclical patterns in organ donations are far less clear in this graphical representation than the mortality trends. I next turn to assessing the strength of the relationship between macroeconomic conditions and the supply of deceased organ donors (and mortality) in a regression framework.

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[14] Warren et al. (1983) find suicide counts were high in the spring and low in the winter. Cheatwood (1988) finds high homicide rates in specific months (December, July, and August), as opposed to entire seasons. Warren et al. (1983) and Tennenbaum and Fink (1994) find homicide spikes from July-September as well as December.

[15] The donation data depicted are residuals from a regression of donations on a full set of month dummies.

1.4 Empirical Specification and Results

My primary empirical strategy involves estimating state- and month-specific organ donation rates and mortality rates as a function of the state unemployment rate. The main estimation equation is:

(1) $$y_{sm} = \alpha_s + \mu_m + \gamma(\textit{unemployment rate})_{sm} + X_{sm}\beta + \varepsilon_{sm}\,,$$

where y_{sm} measures the number of deceased organ donors (or fatalities) per million persons by cause of death, s indexes the state, m indexes the month, and $(\textit{unemployment rate})_{sm}$ represents the unadjusted unemployment rate. All specifications include a full set of state and month indicators (α_s and μ_m respectively), and the time-varying state-level variables X_{sm}. The vector X_{sm} includes separate measures of the state's population aged 0-17, 18-34, 35-49, 50-64, and 65 and above, indicators for whether the state had an organ donor registry, whether online registration was available, and whether an OPO in the state enforced a first-person consent paradigm.[16] I weight each observation by the state's population in that month using U.S. Census Bureau estimates.[17] Estimates of γ based on (1) capture the association between within-state variation over time in organ donation rates (or mortality rates) and economic conditions, proxied by the unemployment rate.

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[16] Monthly, unadjusted, state-level unemployment rates were obtained from the U.S. Bureau of Labor Statistics. Data on first-person consent practices, the existence of state donor registries and the ability to sign up for those registries online came from the author's interviews with OPO employees.

[17] State-level population estimates are available annually from the U.S. Census Bureau. These estimates were expanded to monthly estimates by assuming a constant population growth rate between years.

Thus far, all work on the cyclical nature of mortality has used annual variation. Using a monthly, state-level aggregation instead would provide another test of the reliability of previous findings. Annual aggregation absorbs variation in unemployment rates that may be of interest, such as summer increases in employment due to breaks in the school year. If changes in motor vehicle congestion are an important part of the cyclical mortality patterns, the monthly aggregation level would have an advantage. Monthly results that were noticeably different from previous estimates would be cause for concern that the results could not be interpreted as causal due to a misspecified model or omitted variable bias. Replicating the previous estimates at the monthly aggregation level, including binary month controls, strengthens the premise that identification is coming from changes in the unemployment rate. Another reason for choosing monthly aggregation in this study is the nature of the organ donation data available. As previously noted, cause of death was first recorded in April of 1994, and the natural causes category was added in October of 1999.

Row 1 of Table 1.3 shows results for estimating γ in (1) with cause-of-death-specific, monthly, state-level mortality rate as the dependent variable.[18] Much like previous studies, the results show a very strong procyclical relationship for motor vehicle fatalities. A 1 percentage-point increase in the monthly unemployment rate results in a decrease in motor vehicle fatalities of 0.3799 per million persons, with a standard error of 0.0530 (all standard errors are robust to clustering at the state level to allow for arbitrary serial correlation patterns within-state over time). In addition, a 1 percentage-point increase in the unemployment rate is associated with a

[18] The estimates in Table 1.3 and Panel A of Table 1.4 were generated using the command *areg* in Stata SE 11.2.

decrease in cerebrovascular deaths of 0.7772, with a standard error of 0.0931.[19] Compared to sample means, these estimates reflect a 2.5 percent (= 0.3799 / 14.9114) decrease in motor vehicle fatalities and a 1.6 percent (= 0.7772 / 49.0252) decrease in cerebrovascular deaths. These estimates are similar to Ruhm (2000), who found a 3.02 percent decrease in MVA fatalities, as well as Miller et al. (2009) who found a 2.94 percent decrease in MVA fatalities.[20] Also similar to the previous literature, a strong countercyclical pattern can be seen in suicides, with a 1 percentage-point increase in the unemployment rate leading to 1.2 percent (=0.1233 / 10.3454) increase in suicides.[21] Ruhm (2000) and Miller et al. (2009) estimated a 1 percentage-point increase in unemployment was associated with an increase of 1.27 percent and 1.68 percent in suicides, respectively.

One interesting departure from the previous studies is the estimated strong countercyclical relationship for homicides. A 1 percentage-point increase in the unemployment rate is associated with a 4.2 percent (= 0.2380 / 5.7119) increase in homicides. Ruhm (2000) showed a procyclical trend in homicides using data from 1972-1991, as did Miller et al. (2009) after expanding the sample through 2004. They find that a 1 percentage-point increase in the unemployment rate is associated with decreases in homicides of 1.89 percent and 1.62 percent,

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[19] The cerebrovascular disease mortality was separately estimated in column 5 of Table 1.3 using a restricted sample of November 1999 through 2004 in order to match the results with organ donation estimates in column 4 of Panel A in Table 1.4.

[20] Miller et al. (2009) also found a 1.66 percent increase in degenerative brain deaths. Although this may not be a perfect substitute for cerebrovascular disease, it is the closest reported category found.

[21] Hamermesh and Soss (1974) present a utility maximization model in which an increase in the unemployment rate decreases an individual's expected future income and utility, leading to an increase in suicide. Yang (1992) found statistically significant countercyclical variation in suicide rates, but only for white males. Ruhm (2000, 2003) suggests macroeconomic conditions have an adverse effect on the mental health of working-age adults, leading to increased suicide rates during economic downturns.

respectively. One reason this discrepancy is expected is because of the drastic decrease in crime rates throughout the country in the 1990s, as studied before by numerous authors (for example, see: Blumstein and Rosenfeld 1998, Blumstein and Wallman 2006, Levitt 2004, Rosenfeld 2004, Zimring 2006). Explanations for this decline in crime rates include levels of police officers, increased incarceration, capital punishment, racial profiling, concealed-carry laws, the decline in the crack epidemic, and the legalization of abortion, although there is no widespread support for any one hypothesis. Whatever the cause, this sharp crime rate decline has a strong impact on my estimation results for the homicide mortality category, as my sample begins in 1994.

Rows 2-6 of Table 1.3 show the results separately for five different age groups: 0-17, 18-34, 35-49, 50-64, and 65 and over. The cyclical estimates for motor vehicle fatalities can be seen in each age group. Miller et al. (2009) found a similar pattern, leading to their assertion that the procyclical pattern in motor vehicle fatalities is most likely due to business-cycle externalities stemming from increased traffic. Similarly, a significant countercyclical pattern in homicides can be seen in each age category older than 17. The cyclical pattern in the cerebrovascular disease estimates above is almost completely due to those 65 and older. The fact that the countercyclical pattern in suicides is concentrated in the 35-49 and 50-64 year old categories, which are responsible for roughly 50 percent of all suicides, also supports previous findings. Hamermesh and Soss (1974) hypothesized and showed that changes in unemployment have an increasing effect on suicide as workers age, explained by the fact that older workers have lower expected future incomes.

Unemployment and organ donation

Panel A of Table 1.4 shows results for estimating γ in (1) with organ donation rates by cause of death as the dependent variable instead of mortality rates. Row 1 shows the results when all ages are pooled. Similar to the mortality results, I estimate that the unemployment rate is negatively correlated with the rate of motor vehicle accident donations, and positively correlated with the rate of suicide and homicide donations. However, only the motor vehicle accident donation results are statistically significant. In contrast to the mortality results for cerebrovascular disease, organ donations from natural causes are countercyclically related to macroeconomic conditions and highly significant. Data coding may be one explanation for the unexpected positive estimate. To test this theory, I combine the natural cause donors with those in the none of the above category, which experienced a sharp decline when the natural cause category was added. The results of this exercise are reported in Column 5 of Table 1.4 Panel A. The point estimate in the pooled sample now comes out negative, and although it is not statistically different from zero, it is consistent with the expected sign, given the fatality results.

When the regressions are run separately by age of donor in Rows 2-6 of Table 1.4 Panel A, the patterns underlying the pooled-sample estimates become clear. In the case of MVA donors, a highly significant procyclical correlation for donors under the age of 18 emerges. Among this group, which represents one third of all MVA donors and 39 percent of all donors under 18, a 1 percentage-point increase in the unemployment rate is associated with a decrease in donors of 0.0127 per million persons, with a standard error of 0.0038. This represents a 9.1 percent (=0.0127 / 0.1403) change in organ donors in this age group relative to the sample mean. This is a dramatic change, especially considering MVA victims under the age of 18 only experienced a 2.5 percent (= 0.0478 / 1.9051) change in mortality rate. For comparison, the 1 percentage-point increase in the unemployment rate would translate to an annual decrease of 8 MVA donors per million (= 0.127 × 12 × 51) and 29 total lives per million (= 0.0478 × 12 × 51) in the under 18 age group. Additionally, donation decisions for this age group are far more likely to be made by the donor's family, suggesting that age and cause of death may play a significant role in the family's decision to consent. Table 1.4 also shows that the strongest results in the natural causes category are for those aged 50-64, a group that experienced an insignificant change in mortality rate due to changes in the unemployment rate. When natural causes are combined with the none of the above category, the under 18 age group is the only one with statistically significant results, with a 1 percentage point increase in the unemployment rate resulting in an 8.9 percent (= 0.0071 / 0.0794) decrease in organ donors. While suicides and homicides showed strong countercyclical mortality patterns in Table 1.3, the donation trends for these causes of death do not appear to have a cyclical pattern. One possible explanation for this is that the types of suicides and homicides that occur may differ throughout the business cycle. Although rising unemployment is having a significant impact on mortality rates, there may be differences in the proportion of suicides and homicides that result in potential donors. However, without individual hospital records for each suicide and homicide, I am unable to formally test this theory.

Instrumental variables estimates of the effect of fatalities on organ donation rates

The estimates of γ in (1) displayed in Table 1.3 and Panel A of Table 1.4 characterize reduced-form relationships between macroeconomic conditions and mortality rates or organ donation rates, respectively. The ratio of the two estimates therefore is numerically equivalent to an instrumental variables estimate of the effect of mortality rates on organ donation rates. Such an estimate captures the effect of excess deaths due to economic conditions on the supply of organ donors. The instrumental variables estimates are shown in Panel B of Table 1.4 by cause of death.[22] In the case of motor vehicle accidents, the estimate of 0.0400 (= -0.0152 / -0.3799) implies that for each additional motor vehicle death caused by economic expansion, there will be an additional 0.04 organ donors. The results in the under 18 age group are staggering, with 0.2651 (= -0.0127 / -0.0478) additional organ donors per motor vehicle death. This IV estimate represents the conversion from death to donor specifically for the marginal individuals who die in motor vehicle accidents *because of the increase in the unemployment rate*, and are over three times larger than the overall yield rate of 0.0795 in Table 1.2.

Instrumental variables estimates of the number of organ donors per fatality for the other causes of death are not statistically different than zero.[23] This does not mean that additional deaths in these categories do not lead to organ donors, but that no relationship is found for the marginal individuals whose death was caused by changes in economic conditions. As seen in Panel A of Table 1.4, the unemployment rate does not appear to have a strong effect on organ donation rates in these categories. Within natural causes, this is most likely due to the large proportion of cerebrovascular deaths in the 65 and older category. This is an age category that is rarely deemed suitable for organ donation, as demonstrated by the substantially lower yield rates for those 65 and older in Table 1.2 across all categories of death. While, 58.7 percent of all

[22] The estimates in Panel B of Table 1.4 were generated using the command *ivreg* in Stata SE 11.2. Standard errors for the IV estimates account for the two-stage estimation process.

[23] As previously discussed, the estimates for natural causes are plagued by the late addition and gradual recognition of the category as a cause of donor death in OPTN forms. The negative estimate implies that increased cerebrovascular disease fatalities are associated with decreases in donors due to natural causes. The negative estimate goes away in column 5 where the none of the above category is added to the natural cause donors.

fatalities in the data in 2004 occurred in those 65 and older, just 9.7 percent of all deceased organ donors were 65 or older.[24]

The instrumental variables estimates do suggest there are differences in the marginal benefit of a death resulting from a change in macroeconomic conditions. Those dying in motor vehicle accidents are more likely to become organ donors. This result is compounded by differences occurring in the suitability of each fatality for organ donation, as explained in the next section.

Transplants per donor

The final link between fatalities and the supply of organs centers on the number of transplantable organs recovered from each donor. According to the OPTN, an overall average of 2.7 organs are transplanted from each deceased organ donor.[25] The information available in the SRTR data makes it possible to calculate the number of transplants by cause of death of the donor. Just as certain causes of death lead to more donors than others, the number of transplants per donor is not uniform either. As seen in Panel A of Table 1.5, motor vehicle accidents, suicides, and homicides (3.5, 3.5 and 3.6 transplants per donor, respectively) lead to nearly an entire organ more per donor than the OPTN average. In the previous section we saw that each motor vehicle accident death leads to 0.04 additional donors. Using the number of transplants per donor and the assumption that each transplant saves one life, 0.14 (= 0.04 × 3.5) lives are saved for each motor vehicle accident death caused by economic expansion.

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[24] Source: Author's calculation from SRTR and U.S. Vital Statistics data.

[25] This figure is the author's calculation based on publically available OPTN data for the year 2009, the most recent full year of data available at the time of the calculation.

Donors who died of natural causes do not yield as many transplants per donor (2.3), which suggests that age is an important factor in determining the number of transplants per donor. When looking at transplants per donor broken down instead by the age of the donor in Panel B of Table 1.5, that theory is confirmed. Donors under the age of 18 average 3.4 transplants and donors age 18-34 average 3.7 transplants, well over the OPTN average of 2.7. Therefore, the marginal benefit of a young donor is greater than that of older donors, *ceteris paribus*. For those under 18, one in four motor vehicle accident fatalities become an organ donor, meaning for each motor vehicle accident death caused by economic expansion, an incredible 0.90 (= 0.2651 × 3.4) other lives are saved via organ transplantation, on average.

1.5 Conclusions

The United States faces a critical organ shortage, as demonstrated by the growing transplant waiting list. One way to bridge the gap between the supply and demand for organs is to improve the conversion of potential donors into actual donors. A more complete understanding of factors that affect mortality rates, especially among the causes of death that most commonly lead to potential donors, would aid future efforts to increase the number of deceased organ donors. Cyclical changes in macroeconomic conditions lead to fluctuations in mortality.   This study is the first to examine whether or not corresponding trends are present in deceased organ donations as well.

Consistent with a larger literature, I find that mortality rates in the United States exhibit a strong cyclical pattern. Specifically, a 1 percentage-point increase in the monthly unemployment rate decreases motor vehicle accident and cerebrovascular deaths by 2.5 and 1.5 percent, respectively. On the other hand, deaths from suicide and homicide follow a countercyclical pattern, with a 1 percentage-point increase in the unemployment rates associated with mortality increases of 1.2 and 4.2 percent, respectively. While it is expected that similar trends extend to deceased organ donations for these causes of death, a statistically significant relationship is only found among motor vehicle accident victims. Among this group, increasing the unemployment rate by 1 percentage-point decreases monthly organ donation rates by 0.0152 per million persons, a 3.5 percent change. The results by age of donor are even more telling, as a 1 percentage-point increase in the monthly unemployment rate decreases MVA organ donors under the age of 18 by 9.1 percent.

Combining the estimates of the effect of unemployment rates on mortality rate with those on organ donation rates yields an instrumental variables estimate of the impact of mortality on the supply of organ donors. Using this approach, I find that each motor vehicle accident death resulting from economic expansion leads to 0.04 additional organ donors and 0.14 additional life-saving organ transplants. Motor vehicle accident victims under the age of 18 have an even stronger estimated effect, resulting in 0.27 additional donors and 0.90 additional transplants from each fatality.

These results show that changes in macroeconomic conditions have a substantial impact on both mortality rates and organ donation rates. While the additional deaths represent an unfortunate consequence of improving economic conditions, these additional deaths also positively contribute to the supply of transplantable organs. In light of these findings, expected cyclical fluctuations should be incorporated into future organ donation policy analysis.

CHAPTER 1 APPENDICIES

APPENDIX 1.1

CHAPTER 1 TABLES

Table 1.1: Summary Statistics

Panel A: Average Monthly Organ Donors, by Year and Cause of Death, per Million Persons

	Motor Vehicle Accident	Suicide	Homicide	Natural Causes	Non Motor Vehicle Accident	Child Abuse	None of the above	All Causes
1994	0.460	0.182	0.113		0.110	0.015	0.712	1.591
1995	0.430	0.182	0.090		0.114	0.014	0.791	1.620
1996	0.421	0.164	0.105		0.163	0.012	0.787	1.653
1997	0.404	0.160	0.087		0.172	0.019	0.725	1.574
1998	0.438	0.162	0.080		0.163	0.018	0.841	1.706
1999	0.419	0.146	0.075	0.310	0.178	0.012	0.730	1.713
2000	0.460	0.150	0.073	0.386	0.145	0.012	0.558	1.783
2001	0.415	0.152	0.087	0.433	0.150	0.016	0.492	1.745
2002	0.422	0.158	0.087	0.441	0.131	0.016	0.494	1.749
2003	0.430	0.154	0.090	0.455	0.145	0.019	0.511	1.805
2004	0.441	0.178	0.098	0.472	0.170	0.017	0.619	1.995

Panel B: Average Monthly Mortality, by Year and Cause of Death, per Million Persons

	Motor Vehicle Accident	Suicide	Homicide	Cerebrovascular Disease
1994	16.04	10.99	7.56	49.99
1995	15.32	10.90	7.04	50.98
1996	15.10	10.65	6.70	50.87
1997	15.21	10.59	6.21	50.78
1998	15.02	10.43	5.66	49.77
1999	14.64	9.88	5.20	51.78
2000	14.51	9.68	4.96	50.21
2001	14.44	10.10	5.23	48.64
2002	14.86	10.23	5.07	48.20
2003	14.60	10.17	4.93	46.13
2004	14.56	10.35	4.74	43.31

Notes:

1) The OPTN began reporting circumstances of death on April 1, 1994. Natural Causes was added on October 25, 1999. Averages are taken across 50 states (plus Washington, D.C.) and 12 months of the year, except where noted.

2) For the years 1994-1998, I use the following ICD 9th revision codes: MVA (E810-E825), suicide (E950-E959), homicide (E960-E978), and cerebrovascular disease (430-438). For the years 1999-2004, I use the following ICD 10th revision codes: MVA (V02-V04, V09.0, V12-V14, V19.0-V19.2, V19.4-V19.6, V20-V79, V80.3-V80.5, V81.0-V81.1, V82.0-V82.1, V83-V86, V87.0-V87.8, V88.0-V88.8, V89.0, V89.2), suicide (X60-X84, Y87.0), homicide (X85-Y09, Y87.1), and cerebrovascular disease (I60-I69).

3) Source: Author's calculation from SRTR and U.S. Vital Statistics data.

Table 1.2: Organ Donor Yield Rates by Cause of Death and Age, 1994-2004

Sample means for age and cause-of-death specific mortality (per million persons per month) are shown in brackets.

	MVA	Suicide	Homicide	Natural Causes
all ages	0.0307	0.0153	0.0175	0.0100
	[14.9114]	[10.3454]	[5.7119]	[49.0252]
<18	0.0795	0.0766	0.0326	0.2818
	[1.9051]	[0.4324]	[0.6490]	[0.1115]
18-34	0.0400	0.0271	0.0194	0.2127
	[5.1991]	[3.0895]	[2.8782]	[0.2357]
35-49	0.0229	0.0114	0.0124	0.1023
	[3.2858]	[3.2194]	[1.4153]	[1.5428]
50-64	0.0128	0.0061	0.0096	0.0454
	[1.9883]	[1.8365]	[0.4768]	[3.9149]
65+	0.0020	0.0008	0.0025	0.0017
	[2.5264]	[1.7629]	[0.2794]	[43.2185]

Notes:

1) Calculations are the ratio of organ donors to mortality counts by cause of death from April 1994 to 2004. The natural causes category only covers the period of November 1999 through 2004.

2) The mortality data are calculated using the ICD codes listed in the notes to Table 1.1.

3) Source: Author's calculation from SRTR and U.S. Vital Statistics data.

Table 1.3: The Effect of the Unemployment Rate on Mortality, per Million Persons

(robust standard error), [sample mean]; ***, **, and * indicate significance at 1, 5, and 10%

	MVA	Suicide	Homicide	Cerebrovascular Disease	
all ages	-0.3799***	0.1233***	0.2380***	-0.7772***	-0.8359***
	(0.0530)	(0.0340)	(0.0437)	(0.0931)	(0.1447)
	[14.9114]	[10.3454]	[5.7119]	[49.0252]	[47.4885]
<18	-0.0478***	0.0004	0.0152*	-0.0061*	-0.0052
	(0.0152)	(0.0061)	(0.0088)	(0.0033)	(0.0047)
	[1.9051]	[0.4324]	[0.6490]	[0.1115]	[0.0846]
18-34	-0.1291***	0.0025	0.1357***	0.0010	-0.0030
	(0.0269)	(0.0173)	(0.0245)	(0.0048)	(0.0072)
	[5.1991]	[3.0895]	[2.8782]	[0.2357]	[0.2077]
35-49	-0.0613***	0.0876***	0.0490***	0.0069	0.0186
	(0.0196)	(0.0182)	(0.0182)	(0.0130)	(0.0202)
	[3.2858]	[3.2194]	[1.4153]	[1.5428]	[1.4765]
50-64	-0.0262*	0.0291**	0.0246***	0.0029	-0.0116
	(0.0147)	(0.0136)	(0.0093)	(0.0204)	(0.0323)
	[1.9883]	[1.8365]	[0.4768]	[3.9149]	[3.8397]
65+	-0.1162***	0.0029	0.0114**	-0.7816***	-0.8348***
	(0.0178)	(0.0133)	(0.0056)	(0.0883)	(0.1372)
	[2.5264]	[1.7629]	[0.2794]	[43.2185]	[41.8788]
Years	**1994-2004**	**1994-2004**	**1994-2004**	**1994-2004**	**1999-2004**

Notes:

1) Estimation samples for Columns 1-4 consist of 50 states (plus Washington, D.C.) from April 1994 through 2004. Samples for Column 5 cover November 1999 through 2004. The unit of observation is a state-month, and observations are weighted by state population.
2) The mortality data are calculated using the ICD codes listed in the notes to Table 1.1.
3) All models include indicators for state, month, and year as well as controls for the age distribution of the state's population, and indicators for whether the state has a donor registry, whether the state allows online donor registration, and whether organs can be donated without the consent of family members of the prospective donor.
4) Standard errors, in parenthesis, are robust to clustering within state over time.
5) Source: Author’s calculation from U.S. Vital Statistics data.

Table 1.4: The Effect of the Unemployment Rate on Organ Donation, per Million Persons

(robust standard error), [sample mean]; ***, **, and * indicate significance at 1, 5, and 10%

Panel A: The Effect of the Unemployment Rate on Organ Donation, per Million Persons

	MVA	Suicide	Homicide	Natural Causes	Natural Causes + None of the Above
all ages	-0.0152*	0.0039	0.0016	0.0485***	-0.0012
	(0.0086)	(0.0042)	(0.0034)	(0.0169)	(0.0142)
	[0.4301]	[0.1621]	[0.0888]	[0.4334]	[0.8776]
<18	-0.0127***	0.0011	-0.0015	0.0023	-0.0071**
	(0.0038)	(0.0016)	(0.0013)	(0.0030)	(0.0029)
	[0.1403]	[0.0313]	[0.0194]	[0.0211]	[0.0794]
18-34	-0.0055	0.0025	0.0033	0.0056*	-0.0022
	(0.0047)	(0.0029)	(0.0024)	(0.0033)	(0.0028)
	[0.1954]	[0.0819]	[0.0504]	[0.0440]	[0.1105]
35-49	0.0022	0.0001	0.0004	0.0073	0.0003
	(0.0032)	(0.0017)	(0.0011)	(0.0075)	(0.0059)
	[0.0675]	[0.0369]	[0.0150]	[0.1440]	[0.2877]
50-64	0.0001	-0.0003	-0.001**	0.0254***	0.0027
	(0.0014)	(0.0009)	(0.0004)	(0.0094)	(0.0071)
	[0.0224]	[0.0105]	[0.0038]	[0.1650]	[0.2956]
65+	0.0006	0.0005	0.0003*	0.0078*	0.0051
	(0.0007)	(0.0004)	(0.0002)	(0.0044)	(0.0037)
	[0.0045]	[0.0016]	[0.0003]	[0.0594]	[0.1044]

Table 1.4 (cont'd)

Panel B: Number of Organ Donors per Fatality

	MVA	Suicide	Homicide	Natural Causes	Natural Causes + NA
all ages	0.0400*	0.0317	0.0068	-0.0580**	0.0015
	(0.0202)	(0.0323)	(0.0142)	(0.0271)	(0.0182)

Notes:

1) Estimation samples for Columns 1, 2, 3, and 5 consist of 50 states (plus Washington, D.C.) from April 1994 through 2004. Samples for Column 4 are from November 1999 through 2004. The unit of observation is a state-month, and observations are weighted by state population.

2) All models include indicators for state, month, and year as well as controls for the age distribution of the state's population, and indicators for whether the state has a donor registry, whether the state allows online donor registration, and whether organs can be donated without the consent of family members of the prospective donor.

3) Standard errors, in parenthesis, are robust to clustering within state over time.

4) Source: Author's calculation from SRTR and U.S. Vital Statistics data.

Table 1.5: Average Number of Transplants per Donor

Panel A: By Cause of Death of Donor

All Causes	2.9
MVA	3.5
Suicide	3.5
Homicide	3.6
Natural Causes	2.3
Non-MVA	2.9
Child Abuse	2.6
None of the Above	2.5

Panel B: By Age of Donor

All Ages	2.9
0-17 years old	3.4
18-34 years old	3.7
35-49 years old	2.9
50-64 years old	2.2
65+ years old	1.3

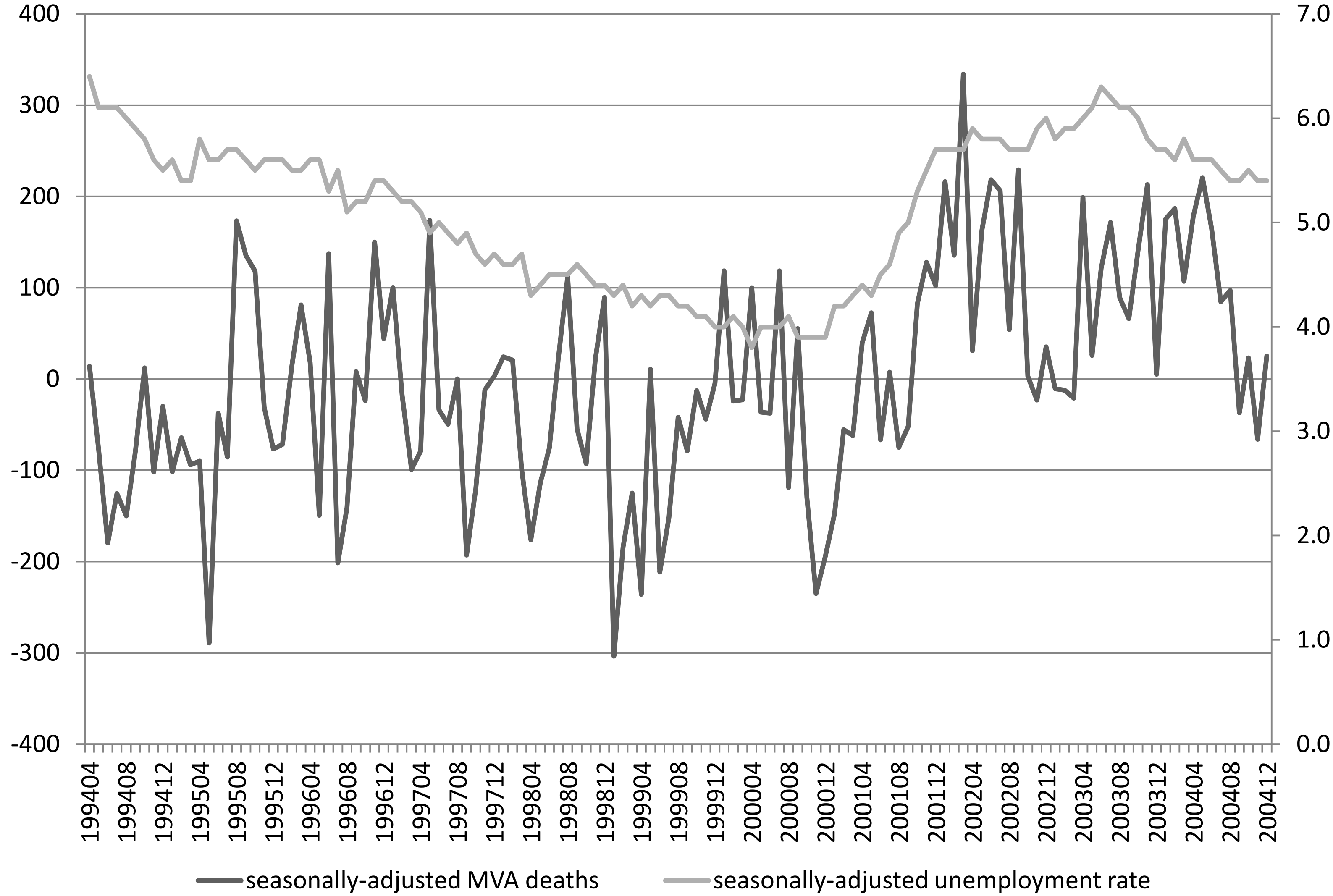
Notes:

1) Source: Author's calculation from SRTR data.

APPENDIX 1.2

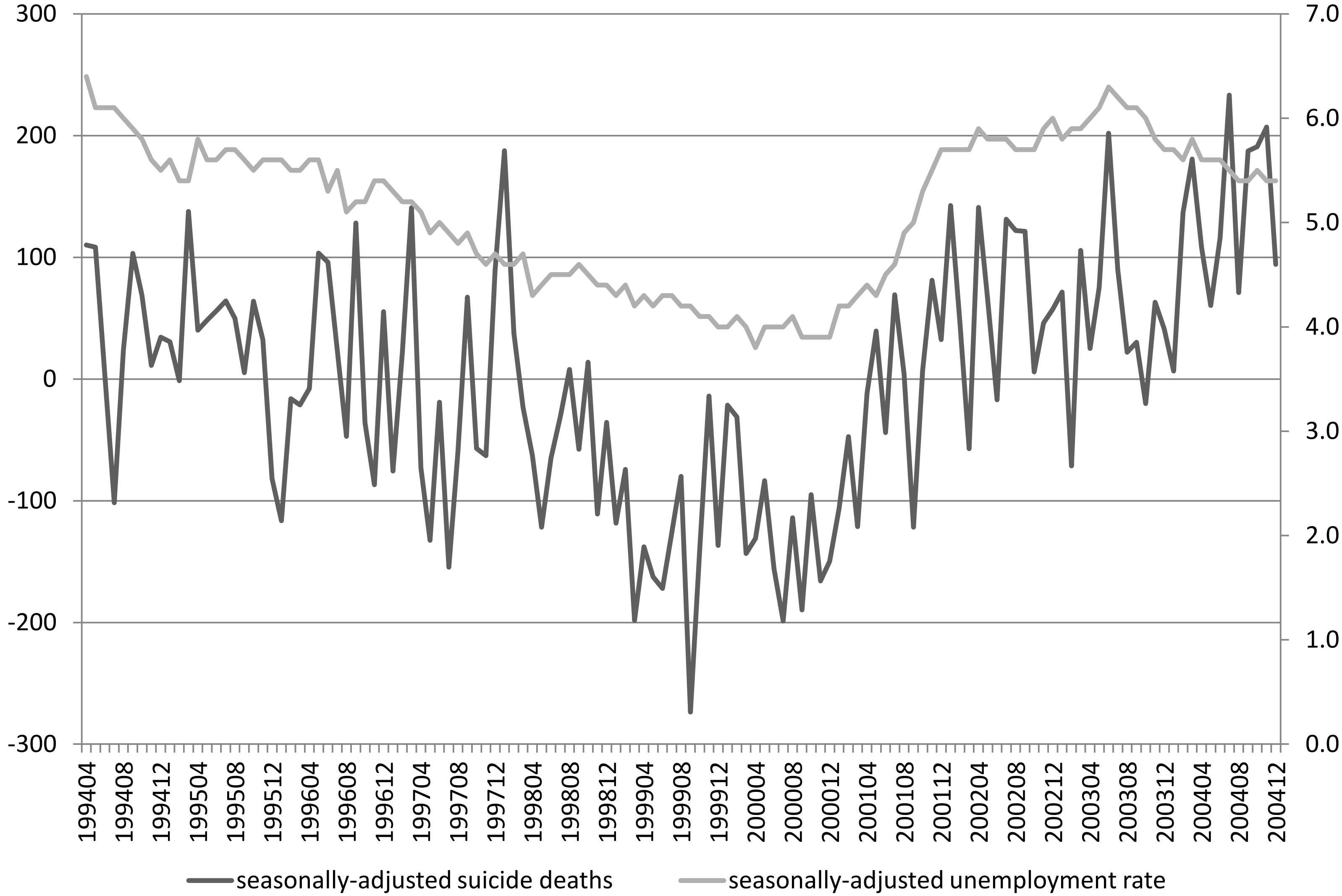
CHAPTER 1 FIGURES

Figure 1.1: Motor Vehicle Accident Mortality and the Unemployment Rate



Note: Author's calculation from U.S. Vital Statistics and Bureau of Labor Statistics data. The mortality data depicted are residuals from a regression of deaths on a full set of month dummies. For a specific list of the ICD codes used, see the notes to Table 1.1.

Figure 1.2: Suicide Mortality and the Unemployment Rate

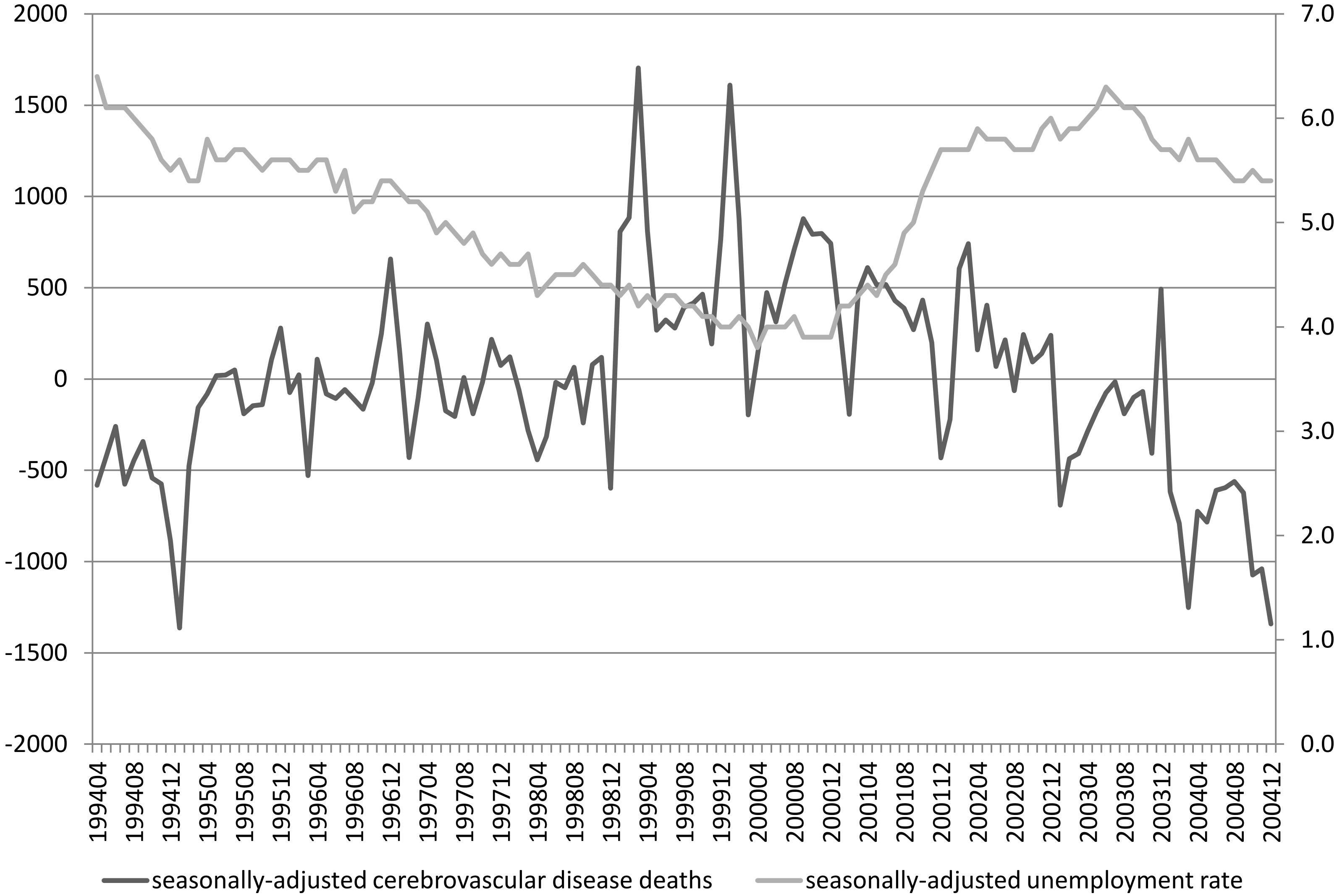


Note: Author's calculation from U.S. Vital Statistics and Bureau of Labor Statistics data. The mortality data depicted are residuals from a regression of deaths on a full set of month dummies. For a specific list of the ICD codes used, see the notes to Table 1.1.

Figure 1.3: Homicide Mortality and the Unemployment Rate

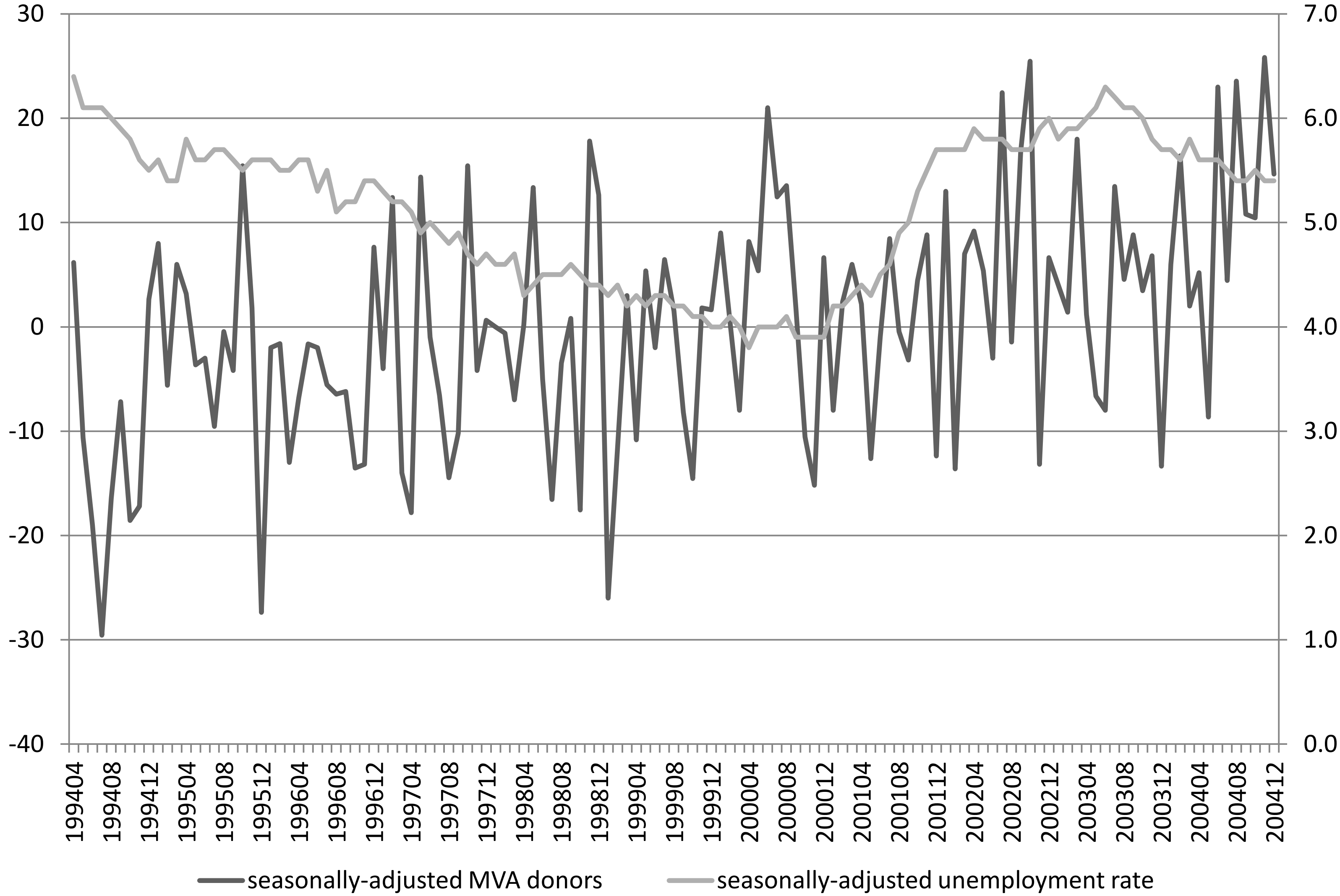
Note: Author's calculation from U.S. Vital Statistics and Bureau of Labor Statistics data. The mortality data depicted are residuals from a regression of deaths on a full set of month dummies. For a specific list of the ICD codes used, see the notes to Table 1.1.

Figure 1.4: Cerebrovascular Disease Mortality and the Unemployment Rate



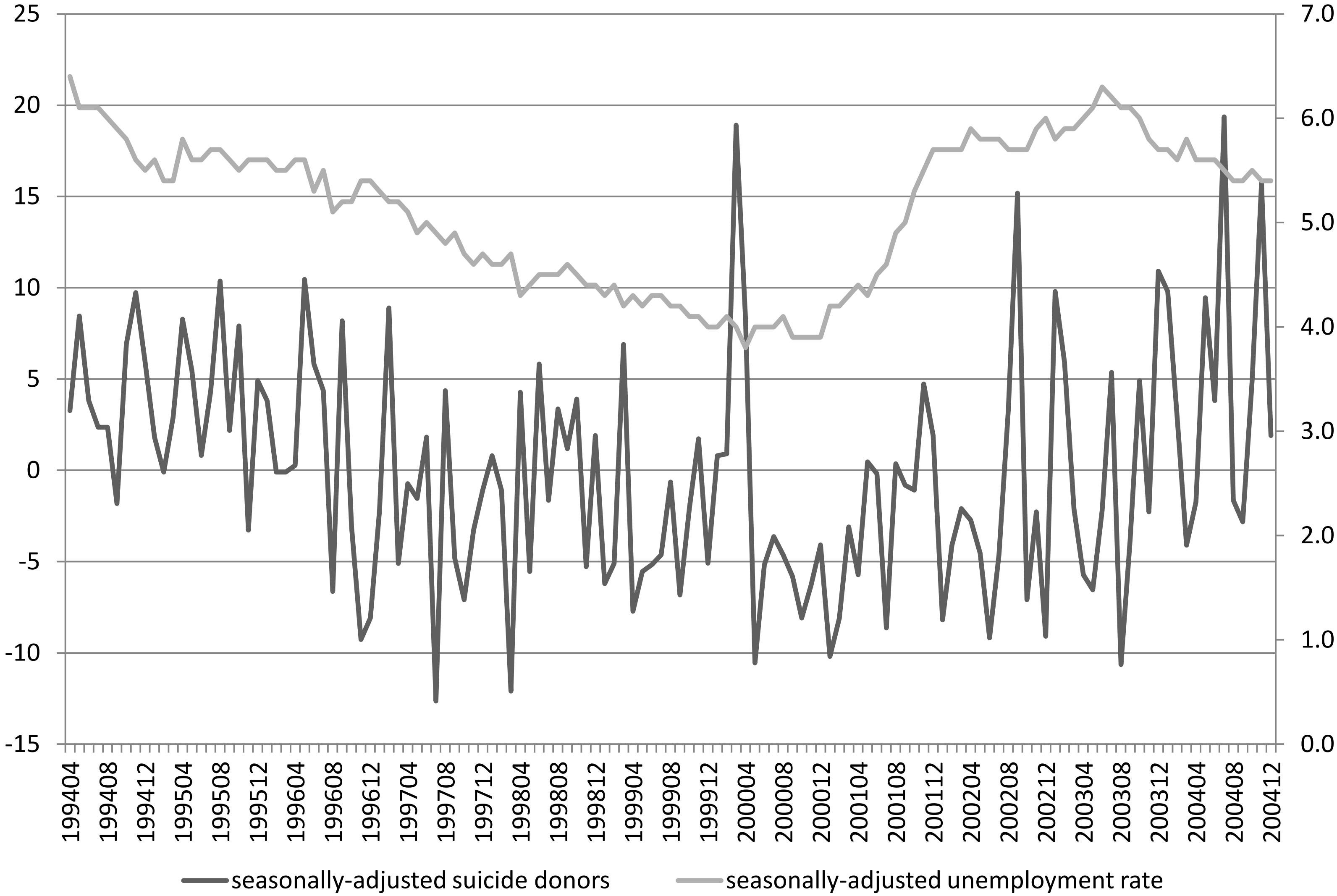
Note: Author's calculation from U.S. Vital Statistics and Bureau of Labor Statistics data. The mortality data depicted are residuals from a regression of deaths on a full set of month dummies. For a specific list of the ICD codes used, see the notes to Table 1.1.

Figure 1.5: Motor Vehicle Accident Organ Donors and the Unemployment Rate



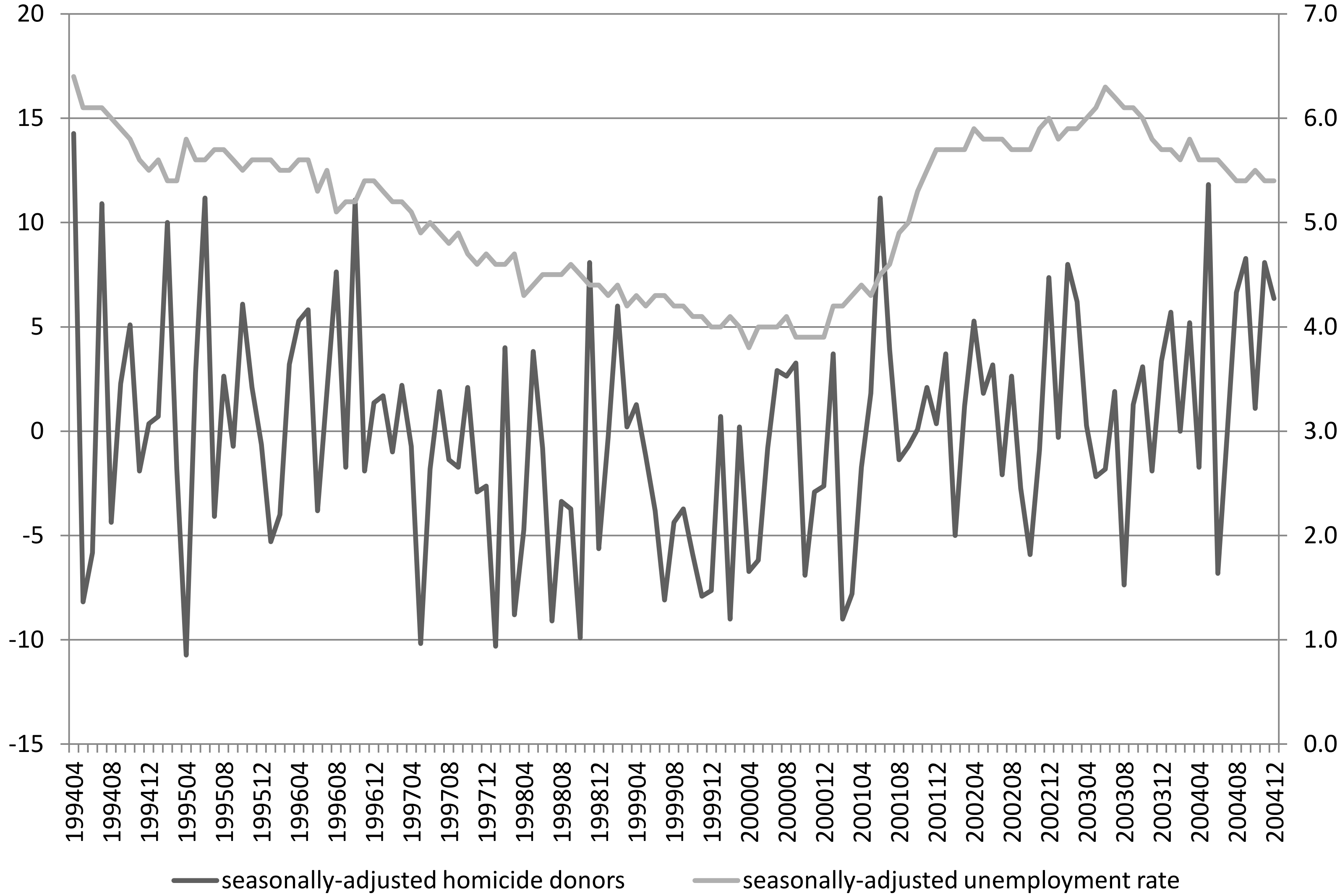
Note: Author's calculation from SRTR and Bureau of Labor Statistics data. The organ donation data depicted are residuals from a regression of deaths on a full set of month dummies. For a specific list of the ICD codes used, see the notes to Table 1.1.

Figure 1.6: Suicide Organ Donors and the Unemployment Rate



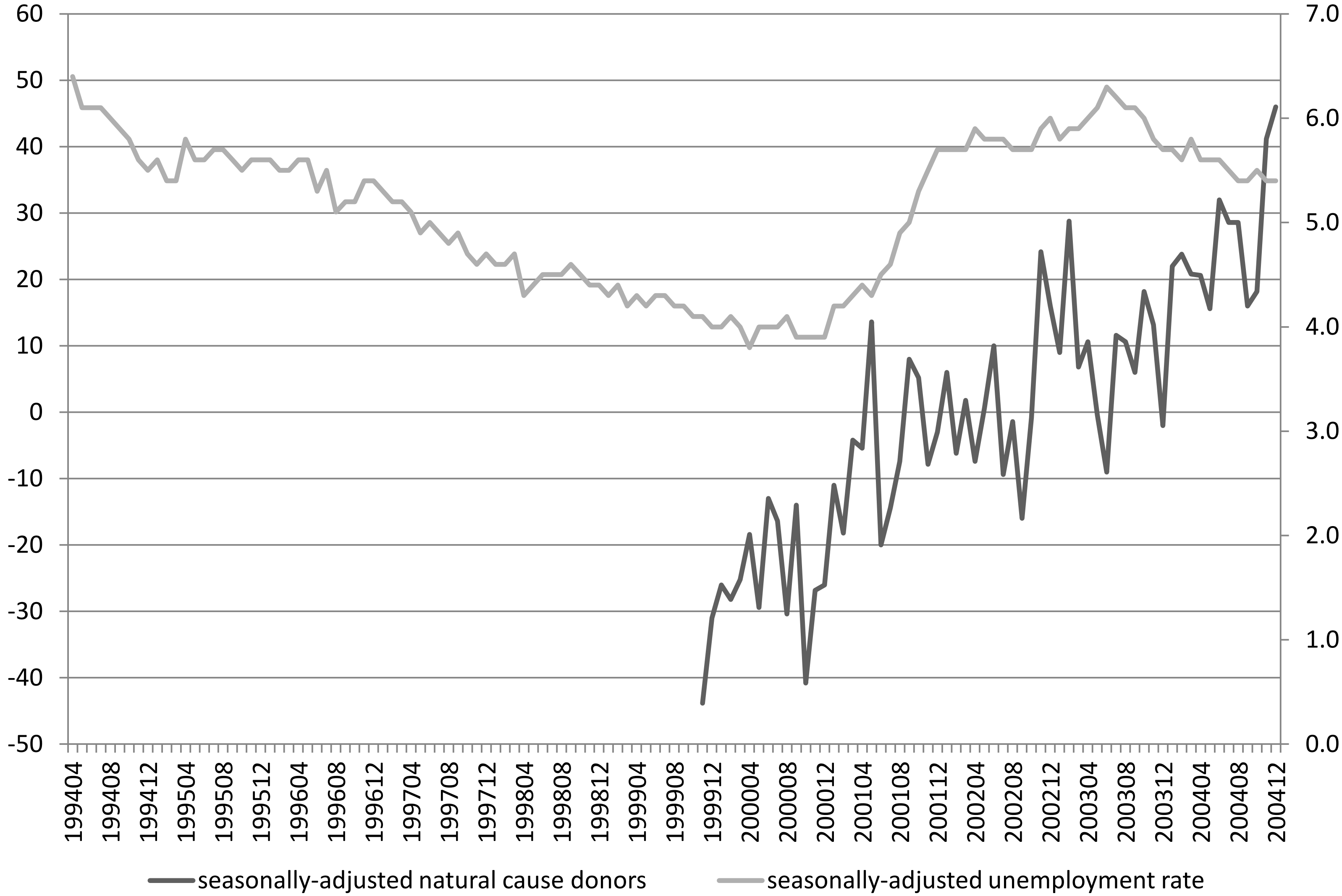
Note: Author's calculation from SRTR and Bureau of Labor Statistics data. The organ donation data depicted are residuals from a regression of deaths on a full set of month dummies. For a specific list of the ICD codes used, see the notes to Table 1.1.

Figure 1.7: Homicide Organ Donors and the Unemployment Rate



Note: Author's calculation from SRTR and Bureau of Labor Statistics data. The organ donation data depicted are residuals from a regression of deaths on a full set of month dummies. For a specific list of the ICD codes used, see the notes to Table 1.1.

Figure 1.8: Natural Cause Organ Donors and the Unemployment Rate



Note: Author's calculation from SRTR and Bureau of Labor Statistics data. The organ donation data depicted are residuals from a regression of deaths on a full set of month dummies. For a specific list of the ICD codes used, see the notes to Table 1.1.

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CHAPTER 2

NOT ON MY BACK ROADS:
ARE THERE SPILLOVER EFFECTS IN MOTORCYCLE HELMET LEGISLATION?

“Every year, millions of dollars are leaving our state on two wheels. Michigan's outdated mandatory helmet law is sending motorcyclists racing across our borders and discouraging motorcyclists in neighboring states from visiting our state.”

-“Opposing points of view: By giving motorcyclists choice on helmets, Michigan would gain millions”; 30 June 2011, The Detroit Free Press (Rhoades, 2011).

2.1 Introduction

Motorcycle helmet laws remain a fiercely contested issue for public debate across the country, over fifteen years since the federal government last removed its mandatory coverage requirement for riders of all ages. Most recently, the legislature of the state of Michigan, which has had a mandatory helmet law since 1969, proposed a repeal of the law.[26] Supporters of the bill claim the state loses tourism dollars to states without mandatory laws, both by inducing Michigan motorcyclists to leave the state and discouraging riders from other states from entering. “(Tom Casperson, R-Escanaba) said he favors the bill because many Upper Peninsula motorcyclists vacation in Wisconsin, where they aren't required to wear a helmet,” (Brenzing, 2011). The spillover of motorcycle traffic from a state with a mandatory helmet law to one without is clearly a topic of concern to lawmakers. Although anecdotal evidence is strong, to date there is no empirical evidence to support claims of a shift in riding location based on the helmet laws of neighboring states.

[26] See http://www.legislature.mi.gov/documents/2011-2012/billintroduced/Senate/pdf/2011-SIB-0291.pdf for more details about Michigan Senate Bill No. 291.

Variation in helmet laws across state borders creates incentives for motorcyclists who prefer helmetless riding to avoid mandatory law states. Tourism revenues, however, are not the only potential economic impact of spillover traffic. Motorcycle lobbying groups have long argued that helmets should be a personal choice, and that not wearing a helmet only affects the individual, thereby negating the need for government action. Accidents involving helmetless motorcyclists affect more than just the individual, and some courts have ruled that the government has an interest in "minimizing the resources directly involved" in caring for these injuries, including emergency response, medical care, and possible disability income transfers (Jones and Bayer, 2007). If part of this increased expenditure is attributable to residents of other states, governments may be justified in eliminating crossover incentives by imposing helmet laws within their borders.

Previous literature has examined state motorcycle helmet laws and their impact on fatality rates (Dee 2009, Dickert-Conlin, Elder, and Moore 2011, Houston and Richardson 2008, Sass and Zimmerman 2000), but no attention has been paid to the interaction created by adjacent states with different helmet laws. A spillover of motorcyclist traffic into states without a helmet law could partially explain why fatality rates in those states are higher relative to states with universal helmet laws, resulting in negative externalities due to the high rates of injury or death of helmetless motorcyclists involved in accidents (GAO 1991).[27] My paper will directly incorporate this incentive change into the analysis of motorcycle helmet legislation, testing for evidence of spillover traffic.

The remainder of the paper proceeds as follows: the next section reviews the history and effectiveness of motorcycle helmet legislation, and describes the conceptual framework behind cross-state spillovers in motorcycle fatalities. Section 3 presents the data used and descriptive evidence. Section 4 details my empirical specifications and estimation results. Section 5 concludes.

2.2 Background

Motorcycle Helmet Law History

Motorcycle helmet laws in the United States can be grouped into three categories: universal laws, partial laws and no laws. Universal laws are the most encompassing, mandating helmet use for riders of all ages. Partial helmet laws only cover part of the population, most often younger riders.[28] Motorcycle helmet laws are enacted at the state level, although federal legislation has played a significant role in shaping these state-level regulations.

Universal helmet laws began to appear in response to the 1966 Highway Safety Act, which authorized the Secretary of Transportation to withhold up to 10 percent of federal highway construction funds from any state that did not adopt universal helmet laws. By 1976, 48 states and Washington, D.C. had adopted a helmet law, with California and Illinois being the exceptions. In 1976, Congress passed an amendment to the Highway Safety Act that eliminated the universal helmet law mandate and removed the Secretary of Transportation's authority to

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[27] Increased accident response expenditures or costs to public health programs are possible examples of negative externalities incurred due to helmetless riding.

[28] The most common partial coverage requires helmets for operators age 17 and younger. Some states have restrictions for riders under age 14, 18, or 20. A handful of states currently require helmets for operators with instructional permits, less than one year of riding experience, or less than $10,000 of personal injury insurance. Passengers riding with operators covered by helmet laws are also often required to wear helmets (http://www.iihs.org/laws/helmet_history.html).

withhold federal funds for noncompliance. This amendment prompted 25 states to reduce their helmet laws to partial coverage or no laws by 1980.

From 1980 to 1991, eight of the ten helmet law changes moved toward stronger helmet laws (five states instituted a universal helmet law) amid mounting evidence that the helmet laws were effective at reducing fatalities. In 1991 the federal government again made a push for stronger helmet regulations by passing the Intermodal Surface Transportation Efficiency Act of 1991, a two-pronged approach aimed at getting states to adopt laws encouraging public safety. Under the act, states that enacted both universal helmet laws and seat belt laws would be eligible to receive federal grant money and states that did not comply would be subject to a 3 percent reallocation of federal highway funds towards highway safety programs. California and Maryland each reacted by instituting a universal helmet law in 1992.

In November 1995, Congress removed the threat of reallocating funds from non-compliant states with the passage of the National Highway System Designation Act.[29] Since then, six states switched from universal to partial-coverage helmet laws and only two states, Louisiana and Colorado, adopted stronger helmet laws. Today, only 20 states and the District of Columbia require all riders to wear helmets. 27 states have partial coverage laws, while Illinois, Iowa, and New Hampshire do not require any riders to wear a helmet.[30]

Helmet Use and Fatality Rates

Motorcycle helmet laws matter for behavior changes: affecting people's decision to wear a helmet and whether or not to ride. These decisions translate to an outcome I can measure:

[29] New Hampshire’s partial coverage law was no longer binding without the federal penalties (http://www.iihs.org/laws/helmet_history.html).

fatalities. Numerous studies, mostly observational in design, show that helmet usage in states with universal helmet laws are very close to 100 percent, while helmet usage following repeal is approximately 55 percent (Berkowitz 1981, Dare et al. 1978, Gilbert et al. 2008, Kraus et al. 1995, Lund et al. 1991, Preusser et al. 2000, Struckman-Johnson and Ellingstad 1980, Ulmer and Northrup 2005, and Ulmer and Preusser 2003). Moreover, the change in helmet use is almost immediate following universal law adoption or repeal (see Gilbert et al. 2008). In addition to affecting the intensive margin for helmet use within the riding population, helmet mandates also affect the extensive margin, creating fluxuations in the size of the riding population. Recent estimates suggest the motorcycle registrations per capita decrease by between 14 percent (Dickert-Conlin et al. 2011) and 22 percent (Dee 2009) following the enactment of universal helmet laws. These results, combined with the observational studies on helmet use, suggest that a non-negligible proportion of motorcyclists prefer not to wear helmets, and alter their behavior in response to changes in helmet laws.

Recent work on motorcycle helmet laws provide evidence about the effectiveness of statewide mandates of helmet use. Using a state-year panel data specification from 1976 to 1997, Sass and Zimmerman (2000) find that universal helmet laws decrease per capita motorcyclist fatalities by 29 to 33 percent. Using a similar specification from 1976 to 2004, Houston and Richardson (2008) find reductions in fatality rates of 29 percent. Dee (2009) and Dickert-Conlin et al. (2011) also estimate fatality reductions of 26 to 32 percent and 39 percent, respectively.[31] While these studies testify to the effectiveness of mandatory helmet laws, none

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[30] See http://www.iihs.org for more details on the history of motorcycle helmet laws.
[31] Dee (2009) covers the years 1988 to 2005 while Dickert-Conlin et al. (2011) uses data from 1994 to 2007.

of them considers the impact statewide helmet laws have on the fatality rates of motorcyclists in adjacent states.

A final area of the helmet literature attempts to estimate a helmet's ability to reduce a rider's fatality risk. Specifications exploiting variation in survival and helmet use among within-vehicle panel data provide convincing evidence of the effectiveness of motorcycle helmets at reducing fatality risk. Using this approach along with data from 1975 to 1986, Evans and Frick (1988) find that wearing a helmet reduces a rider's fatality risk by 28 percent. With data from 1993 to 2002, Deutermann (2004) finds that helmet effectiveness for reducing fatalities and brain injuries to be 37 percent and 69 percent respectively. Most recently, Dee (2009) finds that helmets reduce fatality risk by 34 percent using data from 1988 to 2005. With an alternative meta-analysis format, a Cochrane Collaboration found helmets reduce risk of death by 42 percent and risk of head injury by 69 percent (Liu et al. 2008). These results demonstrate that motorcyclists crossing a state border in order to remove their helmet would expose themselves to greater risk of fatality in the event of an accident. The following section will more carefully develop the conceptual framework of incentives created by state-to-state variation in motorcycle helmet laws and lay out empirical predictions of the resulting incentive structure.

Incentives and Predictions for Cross-State Spillover

Although to my knowledge no other study of motorcycle fatalities has considered the effect of variation in helmet laws across states, the phenomenon of crossing borders to evade laws of one's own state has been tested in other literatures. In a review of the welfare migration literature, Brueckner (2000) finds evidence for strategic interaction between states in the choice of welfare benefits, but only mixed evidence for actual migration by welfare recipients.

Lovenheim (2008) estimates that between 13 and 25 percent of consumers purchase cigarettes in border localities to avoid higher tax levels in their own state, and that the relationship between quantity demanded and home state price is sensitive to the distance to the closest lower-price border.

Consider two states sharing a border, both with no helmet law (or a partial helmet law covering only young riders). Then suppose one state enacts a universal helmet law covering all riders. Let *State N* designate the state with no helmet law and *State H* designate the state with a universal law. In this case, motorcyclists in *State N* are free to ride with or without a helmet as they choose. Motorcyclists in *State H* are faced with the decision to wear a helmet while riding, ride illegally without a helmet, or discontinue riding in *State H*. As seen above, only a small percentage of riders in this circumstance choose to ride illegally without a helmet. Although the choice to discontinue riding cannot be measured directly, evidence on registration numbers suggest about 14 to 22 percent of motorcyclists stop riding following the implementation of a universal helmet law.[32] Whether or not motorcyclists from *State H* cross into *State N* depends on, among other things, the strength of their preferences for helmetless riding and the travel distance to *State N*. If cross-state spillover of this nature does occur, given the strong evidence showing helmetless riding is correlated with fatalities, I expect that some motorcyclists from *State H* will be involved in fatal accidents in *State N*.

Increased fatal accidents in *State H* are also possible, as any rider hoping to evade the helmet law in *State H* must ride to and from the bordering state. Previous work on increased travel for access to alcohol finds effects of this type. In particular, with respect to minimum legal drinking age (MLDA) laws, Lovenheim and Slemrod (2010) find that restrictions on drinking age do not reduce youth involvement in fatal accidents for counties within 25 miles of lower-MLDA state. They also find that fatal accident involvement increases for 18 and 19-year-old drivers in this circumstance. Baughman et al. (2001) examine variation in local access to alcohol within the state of Texas, finding that the sale of beer and wine within a county actually decreases alcohol-related accidents by 8 percent. These results are both consistent with people driving less when alcohol is more accessible, which in turn reduces accidents. Therefore, enactment of a helmet law that induces people to ride to and from a bordering state to avoid the helmet mandate may result in increased accidents and fatalities in the state that enacted the law.

Having a larger percentage of neighboring states with a universal helmet law is expected to lead to increased incentives for spillover traffic and therefore more fatalities, particularly from non-resident motorcyclists. The rationale for this hypothesis is that more motorcyclists in neighboring states would be exposed to the situation described above, preferring to ride helmetless and legally unable to do so in their own state. Some of these motorcyclists are expected to be within a critical travel distance from a neighboring state with more lenient laws. On the other hand, having a larger percentage of neighboring states with a universal helmet law could instead result in reductions to the riding populations in those states, leading to decreased motorcyclist fatalities. Therefore, the overall effect of neighboring state laws on own state fatalities is ambiguous.

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[32] Changes in registrations could also represent a change in the number of motorcycles per owner. It is also possible that motorcyclists ride without proper registration.

2.3 Data and Descriptive Evidence

The fatality data used in this paper come from the National Highway Traffic Safety Administration (NHTSA) Fatality Analysis Reporting System (FARS), available for public download at http://www.nhtsa.gov/FARS. The full FARS file contains detailed information on every accident on public roads that leads to at least one death within 30 days. Among the variables contained in the full file are both the state of crash occurrence and the vehicle license state. For this analysis, I aggregate the data to annual counts of fatalities of operators and passengers on motorcycles for the 48 contiguous states for the years 1976 to 2008 (n = 1,584). A subset of the analysis will focus on non-resident fatality counts, which is calculated as a mismatch of license state and state of crash occurrence. Data on statewide motorcycle helmet law histories come from the Insurance Institute for Highway Safety website (http://www.iihs.org/laws/default.aspx).

Before testing my predictions with regression models, I illustrate my identification strategy with a few cases where we might expect cross-state spillover effects. Figures 2.1 through 2.6 show annual trends in non-resident motorcycle fatalities, both in levels and as a percentage of all motorcycle fatalities, for six different states from 1990 to 2008.[33] Figure 2.1 represents Arizona, which is the only one of California's neighbors without a universal helmet law at the time of California's universal helmet law adoption in 1992. If cross-state spillover was to occur, Arizona would be expected to experience an increase in the number and percentage of non-resident motorcycle fatalities beginning in 1992, as California's riding population looks to avoid the new helmet mandate. While there is an increase in non-resident fatalities for 1992

[33] A non-resident fatality is one in which the state of crash occurrence does not match the state of residence for the deceased, and is calculated by the author using the detailed FARS data files. The years 1990 to 2008 were selected for simplification and consistency.

and 1993, there were also large decreases in both 1991 and 1994. Figure 2.2 represents Pennsylvania, which repealed its own helmet law in September 2003. The surrounding states of Maryland, New Jersey, New York, and West Virginia all have universal helmet laws at this time, with New Jersey being a particularly interesting border state because of the large number of New Jersey residents living in the greater Philadelphia region in close proximity to the Pennsylvania border. Non-resident motorcyclist fatalities in Pennsylvania initially drop in 2004, but display a large and steady upward trend thereafter, supporting my hypothesis. Figure 2.3 shows data for Ohio, which has had a partial helmet law, covering riders 17 and under, since 1978. Ohio is of interest because two of its neighboring states repealed universal helmet laws in this time frame, each with populous metropolitan areas near the border to increase likely pass-through. In addition to Pennsylvania mentioned above (September 2003), Kentucky repealed its universal helmet law in July 1998. Each of these helmet law repeals are expected to decrease non-resident fatality rates in Ohio, as Pennsylvania and Kentucky residents are no longer required to cross state borders in order to ride helmetless. Despite a slight initial decrease following Kentucky's repeal in 1998 and in the year in 2003 when Pennsylvania repealed, Ohio's non-resident motorcyclist fatalities display sharp increases in subsequent years. Figure 2.4 shows non-resident fatality trends for Oklahoma, which also bordered two states that repealed universal helmet laws since 1990, Arkansas and Texas, both in 1997. Similar to the Ohio case, non-resident fatalities are expected to decrease following repeal in neighboring states. While there appears to be a large spike in the fraction of motorcyclist fatalities attributable to non-residents in 1998, there is only a small change in non-resident fatality counts. Figure 2.5 represents Texas. In addition to Texas' own universal helmet law repeal in 1997, the state shares a border with Louisiana. Louisiana repealed a universal helmet law in August 1999, which it reinstated in

August 2004. As such, non-resident fatalities are expected to decrease in 2000 and increase in 2005. However, Texas' non-resident fatalities slightly increase following Louisiana's repeal before large increases after the reinstatement. Lastly, Figure 2.6 displays data for New Mexico, which also borders Texas, with likely pass-through from El Paso. New Mexico has had a helmet law covering only those 17 and younger since 1978. Therefore, decreases in non-resident fatalities are expected following the Texas helmet law repeal, as Texas residents no longer have incentives to cross in New Mexico to remove their helmets. The data show no such decrease in New Mexico, which actually experienced increased non-resident fatalities shortly after the Texas repeal. With no conclusive evidence of cross-state spillover in Figures 2.1 - 2.6, I turn to estimating the effect in a regression framework.

2.4 Methods and Results

My main estimating equation for assessing the effect of motorcycle helmet laws on fatality rates is as follows:

$$(1) \qquad y_{it} = \alpha_i + \delta_t + \gamma(law)_{it} + \lambda \textstyle\sum_{j \neq i} \omega_{ij}(law)_{jt} + \mathrm{X}_{it}\beta + \varepsilon_{it},$$

where y_{it} is the natural log of motorcyclist fatalities per capita in state *i* and year *t*, α_i and δ_t represent a full set of state and year fixed effects, law_{it} is an indicator for whether or not state *i* had a universal helmet law for at least six months in year *t*, and X_{it} represents a vector of time-varying, state-level observables expected to influence fatality rates.[34] The vector X_{it} includes

[34] For the purposes of this study, partial coverage helmet laws are assumed to be equivalent to having no helmet law due to enforcement difficulties and the high proportion of the riding population facing no restrictions.

state population, annual precipitation, heating degree days, gas prices, the state unemployment rate, an indicator for whether or not the state speed limit is 65 mph or above, and an indicator for whether or not the state had a primary enforcement seat belt law.[35] Some specifications also include controls for state motorcycle registration levels per capita. While it may be preferable to normalize the dependent variable by a measure of motorcycle miles travelled in addition to, or in place of, normalization by state population levels, estimates of motorcycle miles travelled are not available.[36] It would also be of interest to include a measure of motorcycle miles travelled in the set of explanatory variables to control for the amount of riding activity in the state.

The effect of helmet laws in neighboring states is captured by the term, $\sum_{j \neq i} \omega_{ij}(law)_{jt}$, where law_{jt} represents the helmet law in other states j, $j \neq i$, and ω_{ij} represents a weighting scheme corresponding to the importance state i attributes to state j's helmet law. Due to the difficulty in creating a perfect variable for neighboring state helmet laws, I will implement four different configurations. One simple formulation is to set law_{jt} equal to one for each state with a universal motorcycle helmet law and use ω_{ij} equal to $1/n_i$ for each state j sharing a border with state i, and zero otherwise, where n_i represents the number of states that

[35] Heating degree days are intended to capture heating requirements for a home or business. Heating degree days are calculated by taking the average daily temperature and subtracting it from the base temperature of 65 degrees. The result would be the number of heating degree days for that day. Average daily temperatures greater than 65 degrees are counted as 0 heating degree days. The observations from multiple weather recording stations are combined to form a single statewide heating degree day observation, weighted by state population. Heating degree days are measured in 1,000s in this paper. Primary enforcement stipulates a driver may be stopped and issued a citation for not wearing a seat belt. With secondary enforcement, a driver must be stopped for another violation before a seat belt citation may be issued.

[36] Houston and Richardson (2008) normalize fatalities by population, motorcycle registrations, and vehicle miles travelled with only small changes in results.

share a border with state i. This configuration mimics the "contiguity" weighting scheme utilized in the "race to the bottom" welfare migration literature.[37] The second formulation sets law_{jt} equal to the number of shared border miles with an adjacent state containing a universal helmet law.[38] The weights, ω_{ij}, are slightly modified to equal $1/m_i$ for each state j sharing a border with state i, and zero otherwise, where m_i represents the total number of shared border miles for state i. The third formulation sets law_{jt} equal to the number of National Highway System (NHS) roadway crossings from state j with a universal helmet law into state i.[39] The weights, ω_{ij}, are again modified, this time to equal $1/c_i$ for each state j sharing a border with state i, and zero otherwise, where c_i represents the total number of NHS crossings for state i. The final configuration will be identical to the first with different weights. In this specification, ω_{ij} is equal to s_i/n_i for each state j sharing a border with state i, and zero otherwise, where n_i again represents the number of states that share a border with state i and s_i is an indicator equal to one if state i doesn't have a universal helmet law. This weighting scheme has the effect of controlling for the fraction of neighboring states that have stronger helmet laws than state i, and

[37] See Brueckner (2000) for more details.

[38] Data on state border lengths came from the website of Dr. Thomas Holmes at the University of Minnesota, accessed April 1, 2008. 109 borders exist between states, and the data contained the length (in miles) of each border. Please see http://www.econ.umn.edu/~holmes/data/BorderData.html for more details.

[39] National Highway System crossings in this study include the Eisenhower Interstate Highway System, other NHS routes and non-interstate STRAHNET routes. This data was obtained from the U.S. Department of Transportation, Federal Highway Administration website, accessed May 15, 2011. Please see http://www.fhwa.dot.gov/planning/nhs for more details.

will test the possibility that the helmet laws of adjacent states will only affect state *i*'s fatality rates if state *i* does not have a universal helmet law of its own.[40]

The state fixed effects, α_i, account for unobserved differences between states that do not change over time, such as safety preferences. The full set of year dummies, δ_t, will control for time-specific factors that are constant among all states, such as changes in motorcycle engine technology or the size of other vehicles on the road. Variation within states over time identifies the model. Of principle interest, estimates of γ capture the effect of a statewide helmet mandate on motorcyclist fatalities while estimates of λ capture the effect of helmet laws in adjacent states. Using the natural log of motorcyclist fatalities per capita as the dependent variable allows the results to be easily compared to previous estimates of the effect of a state's own helmet law, while observing any additional effect due to the helmet laws of neighboring states. Each of the configurations for neighboring state helmet laws controls for the expected exposure of state *i* to non-resident motorcyclists in a different way. States with long shared borders, such as those in the western United States, tend to have fewer border crossings than the more highly populated eastern states. Having a high number of border-crossing roadways serves as a proxy for the population density near the border and the ease of travel from one state to another. Having a higher percentage of adjacent states containing universal helmet laws is expected to induce more non-resident motorcyclists to enter state *i*, some of which will be involved in fatal accidents as a result. Therefore, estimates of λ are hypothesized to have a positive sign.

Table 2.1 presents summary statistics for the included variables.[41] Of note, there are 76 annual motorcycle fatalities in an average state and the average fraction of motorcycle fatalities

[40] This is because riders forced to wear a helmet following a mandate do not face spillover

involving non-residents is 0.122. A universal helmet law is in effect in 46.2 percent of all state-year observations in the data. States have an average of 4.5 neighboring states, with the minimum being 1 (Maine) and the maximum being 8 (Missouri and Tennessee). The average fraction of neighboring states with a universal helmet law is 0.489.

The Effectiveness of Statewide Motorcycle Helmet Laws

Results from estimating (1) are shown in Table 2.2 using the natural log of motorcyclists' fatalities per capita as the dependent variable.[42] In column (1), the model is estimated without controlling for the helmet laws of adjacent states to ensure that the estimates are consistent with those of previous work on the effectiveness of motorcycle helmet laws. As seen in the first row, adopting a universal helmet law is associated with an approximate 36.4 percent decrease in state motorcyclist fatalities per capita, which is statistically significant at the 1 percent level, consistent with previous estimates.[43] There are also highly significant estimates for population, precipitation, and the state unemployment rate. The estimate of -0.3636 (0.1355) for the coefficient on the natural log of total state population implies that a 1 percent increase in state population is associated with a decrease in per capita motorcycle fatalities of 0.36 percent. This statistically significant estimate suggests there is a difference in fatality patterns between states with large populations, as the dependent variable has already been normalized by state population. One explanation for this effect is that states with large urban populations may have

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incentives if all neighboring states also have a universal helmet law in place.

[41] Data sources for all variables are available in Table 2.6.

[42] The estimates in Table 2.2, columns (1) and (2) of Table 2.3, and Table 2.5 were generated using the command *xtreg, fe* in Stata SE 11.2.

[43] Standard errors shown throughout the paper are robust to clustering at the state level, which allows for arbitrary correlation in the error terms within state over time.

significantly different motorcycle riding patterns, with a smaller proportion of the population "at-risk" for fatal accidents.

There are two obvious ways precipitation levels could affect motorcycle fatality levels. First, precipitation during a motorcycle ride would make conditions more dangerous, and would lead to increased accident levels. Secondly, motorcyclists most likely avoid riding during these spells of inclement weather, reducing the "at risk" riding population and reducing accident levels. States with large amounts of snowfall would therefore be expected to have shorter riding seasons, and fewer fatal accidents, *ceteris paribus*. The precipitation variable used in this study is an aggregate measure of state-year precipitation levels, and not an indicator for inclement weather at the time of any specific fatal accident, meaning the latter explanation is probably more credible. The estimate of -0.1737 (0.0438) for the coefficient on the natural log of precipitation shows that a 10 percent increase in precipitation would decrease motorcyclist fatalities by 1.74 percent. Once precipitation levels, which include snowfall, are controlled for, there is no statistically significant effect of heating degree days on motorcycle fatality rates, although the estimates corresponding to heating degree days are negative as expected. This result suggests the variable is capturing a reduction in riding season faced by northern states as opposed to the decreased safety motorcyclists face in inclement weather.

An increase in the unemployment rate of 1 percentage point is associated with a decrease in fatalities of 2.94 percent, significant at the 1 percent level. This result is consistent with previous work on cyclical trends in mortality rates (Miller et al. 2009, Moore 2011, Ruhm 2000), which explains increased motor vehicle accident mortality rates by the roadway congestion caused by increases in both business and leisure travel during economic expansions. Higher gas prices are also expected to decrease motorcycle fatalities rates, as previous research has found higher gas prices decrease the total amount of vehicle miles travelled (Grabowski and Morrisey, 2004). The estimates for the coefficient on the natural log of gas price are negative, but statistically significant, in all specifications.

Column (2) of Table 2.2 replicates the estimates in column (1), including an additional control variable measuring the number of motorcycle registrations per 10,000 persons. This is done to test the possibility that changes in registration rates are responsible for changes in motorcycle fatality rates, and that the helmet law indicators merely absorb the large changes in registrations that follow a law change. The results are nearly identical, both in terms of significance and point estimates, albeit with a slight reduction in the absolute value of the point estimates. The estimate for the effect of registrations is 0.0965 (0.0667), indicating that the universal helmet law variables are not inflated due to the omission of registrations from the model in the other columns, as might be expected if a helmet law enactment resulted in an immediate change in the number or type of motorcycle riders on the road.[44]

Columns (3) through (6) present the results for estimating the full model in equation (1). Each column corresponds to a different formulation of helmet laws in adjacent states, as described above. A positive sign for the coefficient representing neighboring state laws would support the spillover hypothesis. The results are nearly identical throughout. The effect of a state's own helmet law on fatalities ranges from a 33.7 percent to 36.0 percent decrease, representing almost no change from columns (1) and (2). The effect of an increase in the strictness of neighboring state helmet laws is positive in just one of the four formulations, although insignificantly different from zero in all four cases. The point estimate in column (3)

[44] Motorcycle rider type could change in one of two ways following helmet law enactment. The law could deter more risk-seeking riders, leaving a safer riding population. Alternatively,

for the fraction of adjacent states with a universal helmet law implies that an increase of 0.2 would decrease motorcyclist fatalities by 2.1 percent (= -0.1067 × 0.2 × 100).[45] In columns (4) and (5), increases of 0.2 in the fraction of border length shared with, or road crossings into, a state with a universal helmet law yield corresponding changes in motorcyclist fatalities of -0.9 percent (= -0.0434 × 0.2 × 100) and -1.9 percent (= -0.0945 × 0.2 × 100), respectively. Finally, column (6) shows that fatalities increase by 1.3 percent (= 0.0625 × 0.2 × 100) when the fraction of adjacent states with stronger laws increases by 0.2. Point estimates for the other independent variables also follow the patterns seen in columns (1) and (2), both in magnitude and significance. These results suggest the helmet laws of adjacent states do not have a strong effect on the overall motorcyclist fatality rate, after controlling for a state's own helmet law.

Estimating the Fraction of Motorcycle Fatalities Involving Non-residents

In this section, I specify an alternative model for testing cross-state spillover effects with respect to motorcycle helmet laws. Rather than attempting to identify changes in the full motorcycle fatality rate, I will estimate the fraction of motorcycle fatalities involving non-residents. Over the sample range, an average of 6 non-resident fatalities occurred for each state-year combination, accounting for 12.2 percent of annual motorcyclist fatalities, on average.

I first estimate a linear fixed effects model identical to equation (1) with y_{it} now equal to the fraction of motorcyclist fatalities involving non-residents in state *i* and year *t*. The estimation results for this linear model are displayed in column (1) and (2) of Table 2.3. The presence of a

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following the Peltzman risk-compensating hypothesis (Peltzman, 1975), riders who stay may exhibit more risky riding patterns as a result of feeling safer.

[45] The mean state has 4.5 neighboring states, meaning a change of 0.2 represents a change in the helmet law of approximately one neighboring state.

universal helmet law, either within the state or among its neighbors, has a positive but statistically insignificant affect in this model. Population is still highly significant, where a 1 percent increase is associated with a decrease of 0.0675 (0.0227) in the fraction of motorcyclist fatalities involving non-residents, representing a 55.3 percent (= 0.0675 / 0.122) decrease relative to the sample mean. While it seems plausible that a change in a state's population, and therefore potential riding population, is an important determinant of the fraction of non-resident fatalities, the size of this estimate could indicate a problem in the estimation strategy.

With a dependent variable bounded between 0 and 1, a linear model cannot provide accurate estimates at all points of the distribution, as literal interpretation of the estimates would predict levels of y_{it} above 1 or below 0. Following Papke and Wooldridge (2008), I next turn to estimation that accounts for the fractional nature of the dependent variable, using a fractional probit model of the form:

(2) $$E(y_{it} \mid X_{i1}, X_{i2}, \ldots, X_{iT}) = \Phi(\psi_a + X_{it}\beta_a + \bar{X}_i\xi_a),$$

where y_{it} is again the fraction of motorcyclist fatalities involving non-residents. X_{it} now includes all state-year observables as before as well as the controls for a state's own helmet law and the helmet laws of adjacent states. $\bar{X}_i$ represents a set of time-demeaned variables contained in X_{it}, and is similar to the state-level fixed effects in the linear regression framework. A full set of year indicators are also included in the estimation.[46]

Columns (3) through (6) of Table 2.3 display the results from estimating equation (2). I display results for only two of the adjacent state helmet law specifications for brevity. Columns

(3) and (4) of Table 2.3 represent the marginal effects using a pooled fractional probit (PFP) estimator, obtained by maximizing the pooled probit log-likelihood.[47] Columns (5) and (6) use a generalized estimating equation (GEE) approach with an exchangeable working correlation matrix.[48] The GEE approach is asymptotically equivalent to a multivariate weighted nonlinear least squares in this case, and will potentially increase the efficiency of the estimates.[49] The point estimates follow a very similar pattern, with the exception of those in column (3), where an increase of 0.2 in the fraction of neighbors with a universal helmet law is estimated to increase the fraction of non-resident fatalities by 11.4 percent (= (0.0695 × 0.2) / 0.122) relative to the sample mean. While this result is consistent with the story that an increase in the percentage of adjacent states with a universal helmet law will induce more non-resident motorcyclists to cross the border to ride helmetless, it is the only one of six specifications resulting in an estimate significantly different than zero. The estimates of Table 2.3 do not provide conclusive evidence about the effect of helmet laws on non-resident motorcyclist fatalities. It is conceivable that the presence of helmet laws has a significant effect on both the numerator and denominator of the dependent variable, potentially masking the relationship of interest. In the next section, I test for this possibility by estimating the number of non-resident motorcyclist fatalities separately with count data methods that account for the panel nature of my data.

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[46] The subscript *a* denotes the use of a scale factor. The results reported in Table 2.3 are average partial effects that depend on the scale factor, and are comparable to the linear fixed effect estimates.

[47] The estimates in columns (3) and (4) of Table 2.3 were generated using the command *glm* with *family(binary)* and *link(probit)* in Stata SE 11.2.

[48] The estimates in columns (5) and (6) of Table 2.3 were generated using the command xtgee with *family(binary), link(probit)* and *correlation(exchangeable)* in Stata SE 11.2.

Fixed Effects Poisson Estimation of Motorcyclist Fatalities

To address the possibility that the numerator and denominator in the above dependent variable move together, I will separately estimate the number of non-resident motorcyclist fatalities and the total number of motorcyclist fatalities using count data methods. Count data methods are necessary here because the nature of the dependent variable requires that the conditional mean be non-negative for all values of X_{it}, and a linear model can result in negative predicted values. Developed by Hausman et al. (1984), I estimate a fixed effects Poisson model based on the following conditional mean equation:

$$E(y_{it} | X_{it}, c_i) = c_i \exp{(X_{it}\beta)} \tag{3}$$

where X_{it} includes all explanatory variables from equation (1) as well as the controls for helmet laws in state *i* and *j*≠*i*, and a full set of year indicators. c_i represents unobserved heterogeneity at the state level, and takes the form $c_i = \exp(\alpha_i)$ in practice. y_{it} will be either the number of motorcyclist fatalities or the number of motorcyclist fatalities involving non-residents. The fixed effects Poisson estimator has very strong robustness properties, requiring only proper specification of the conditional mean for consistency (Wooldridge 1999).[50]

Table 2.4 displays results from estimating equation (3).[51] The dependent variable in columns (1) and (2) is the number of non-resident motorcyclist fatalities, while columns (3) and (4) correspond to estimates for the total number of motorcyclist fatalities. The estimates for the coefficient on a state's own helmet law are all negative and statistically significant. This

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[49] See Papke and Wooldridge (2008) for more details on the fractional probit estimation.

[50] The estimation is robust to serial correlation and misspecification of the Poisson distribution by using the standard errors from Wooldridge (1999).

suggests the fractional probit estimates of the previous section failed to capture a significant effect of helmet laws due to shifts in both the numerator and denominator of the dependent variable. A similar conclusion can be reached for the effect of the state unemployment rate, which is associated with statistically significant decreases in both non-resident and total motorcyclist fatality counts. The pattern of estimates for the natural log of state population show that states with large populations are expected to have more motorcycle fatalities, but are not expected to face significant differences in non-resident fatalities. Due to the fact that the dependent variable is no longer normalized by state population, the positive and significant estimates in columns (3) and (4) do not contradict the negative and highly significant estimates seen in Table 2.3 for the population variable, as they capture different effects. Separately estimating the numerator and denominator did not lead to a significant estimate for the coefficient on the helmet laws of adjacent states.

The Effect of Helmet Laws on Riding Population

The results in Tables 2.2 though 2.4 provide evidence that a state's own helmet law substantially affects motorcyclist fatalities, but the helmet laws of adjacent state's do not have a significant differential effect. As discussed earlier, enacting a universal helmet law affects motorcycle fatality rates in two distinct manners, by reducing the probability of death conditional on an accident, and by reducing the size of the riding population. The last possibility to explore is whether or not universal helmet laws in adjacent states affect fatality rates by reducing the riding population of those states, and therefore the pool of potential border-crossing motorcyclists. I next turn to estimating the effect of helmet laws on motorcycle registrations

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[51] The estimates in Table 2.4 were generated using the command *xtpqml, fe* in Stata SE 11.2.

based on the linear model in equation (1). Table 2.5 shows the results of estimating the equation (1) with the natural log of motorcycle registrations per 10,000 persons as the dependent variable. The estimates in column (1) show that universal helmet laws are associated with a 24 percent reduction in motorcycle registrations, which is consistent with Dee (2009) and Dickert-Conlin et al. (2011). Columns (2) and (3) add in the controls for helmet laws in adjacent states. Similar to fatalities, motorcycle registrations do not appear to respond to changes in the helmet laws of neighboring states.

2.5 Conclusions

Although it has been nearly 50 years since the federal government first mandated universal motorcycle helmet laws, the merit of such government involvement continues to be debated. The argument has traditionally pitted civil liberties against the additional costs to public health programs and emergency response imposed by helmetless motorcyclists, and is now being updated to include the cost of lost state revenue. Despite the fact that empirical studies have shown both helmets and helmet laws to be effective at substantially reducing fatalities, no previous work has incorporated the helmet laws of adjacent states into the analysis. Neighboring states with different helmet laws facilitate an incentive structure for motorcyclists to cross state borders to avoid helmet mandates.

I find a state’s own helmet law to be an important factor in determining motorcyclist fatality rates, as it strongly influences both the helmet status, and therefore safety, of an individual rider in the event of a crash and the size of the local riding population. Consistent with a larger literature, my results show a universal law is associated with a decrease in motorcyclist fatalities of 34.1 percent. After conditioning for a state’s own helmet law, the laws of adjacent states do not appear to have an additional effect on fatality rates. In addition, I find that neither a state's own helmet law, nor those of its neighbors affect the fraction of motorcyclist fatalities involving non-residents. Lastly, I find own-state helmet laws are associated with significant reductions in non-resident fatality counts, although the helmet laws of adjacent states remain insignificant. While cross-state spillover effects could be used to support strategic interaction among state governments, either to reduce negative externalities or boost tourism revenues, my examination of fatality patterns shows no conclusive evidence of a shift in motorcycle riding location based on the helmet laws of neighboring states. I am also unable to rule out the possibility that motorcyclists do respond to changes in helmet laws by shifting riding locations, but are also involved in increasing levels of fatal accidents in their own state as a result.

CHAPTER 2 APPENDICES

APPENDIX 2.1

CHAPTER 2 TABLES

Table 2.1: Summary Statistics

Variable	Mean	Standard Deviation	Min	Max
Motorcycle fatalities (annual count)	76.083	98.283	1	879
Motorcycle fatalities per 100,000 persons	1.537	0.746	0.127	5.156
Universal helmet law	0.462	0.499	0	1
Total population	5,343,621	5,666,977	396,126	36,580,371
Motorcycle registrations per 10,000 persons	235.934	136.762	2.474	1,160.33
Annual precipitation (in inches)	36.719	15.037	5.37	80.58
Heating degree days (in 1,000s)	5.238	2.026	0.414	10.758
State unemployment rate	5.788	1.952	2.3	17.4
Gas price ($ per gallon)	1.32	0.57	0.57	3.41
Speed limit ≥ 65 mph	0.616	0.487	0	1
Primary seat belt law	0.171	0.377	0	1

Table 2.1 (cont-d)

Variable	Mean	Standard Deviation	Min	Max
Number of adjacent states	4.5	1.595	1	8
Length of borders (in miles)	905.013	397.670	102.9	1,563.2
NHS road crossings	21.083	9.272	4	48
Fraction of adjacent states with universal helmet law	0.489	0.316	0	1
Fraction of border miles of adjacent states with universal helmet law	0.486	0.335	0	1
Fraction of NHS road crossings from adjacent states with universal helmet law	0.490	0.342	0	1
Fraction of adjacent states with stronger relative helmet law	0.197	0.280	0	1
Number of non-resident motorcycle fatalities (annual count)	6.044	6.036	0	62
Fraction of motorcycle fatalities involving non-residents	0.122	0.094	0	0.667

Note: Sources available in Table 2.6.

Table 2.2: Estimates of the Effect of Helmet Laws on Motorcycle Fatalities per Capita

	(1)	(2)	(3)	(4)	(5)	(6)
Universal helmet law	-0.3641***	-0.3408***	-0.3549***	-0.3603***	-0.3542***	-0.3369***
	(0.0428)	(0.0450)	(0.0456)	(0.0446)	(0.0456)	(0.0548)
ln (motorcycle registrations)		0.0965				
		(0.0667)				
Fraction of neighbors w/ universal law			-0.1067			
			(0.0751)			
Fraction of border miles w/ universal law				-0.0434		
				(0.0640)		
Fraction of National Highway System					-0.0945	
road crossings into a state w/ universal law					(0.0650)	
Fraction of neighbors w/ stronger						0.0625
relative helmet law						(0.0806)
ln (total population)	-0.3636**	-0.3243**	-0.3304**	-0.3471**	-0.3167**	-0.3754***
	(0.1355)	(0.1305)	(0.1354)	(0.1420)	(0.1437)	(0.1357)
ln (heating degree days)	-0.0491	-0.0465	-0.0468	-0.0494	-0.0494	-0.0505
	(0.0638)	(0.0620)	(0.0638)	(0.0636)	(0.0639)	(0.0644)
ln (precipitation)	-0.1737***	-0.1725***	-0.1763***	-0.1738***	-0.1744***	-0.1708***
	(0.0438)	(0.0441)	(0.0442)	(0.0440)	(0.0441)	(0.0435)

Table 2.2 (cont'd)

State unemployment rate	-0.0294***	-0.0263***	-0.0290***	-0.0292***	-0.0294***	-0.0288***
	(0.0087)	(0.0087)	(0.0087)	(0.0087)	(0.0087)	(0.0087)
ln (gas price)	-0.1895	-0.2105	-0.1444	-0.1742	-0.1401	-0.1996
	(0.2742)	(0.2749)	(0.2691)	(0.2698)	(0.2665)	(0.2785)
Speed limit ≥65 mph	-0.0285	-0.0269	-0.0240	-0.0273	-0.0256	-0.0279
	(0.0384)	(0.0386)	(0.0378)	(0.0381)	(0.0380)	(0.0379)
Primary seat belt law	0.0205	0.0155	0.0160	0.0187	0.0183	0.0245
	(0.0406)	(0.0393)	(0.0398)	(0.0402)	(0.0400)	(0.0402)

Notes:
1) The dependent variable is the natural log of motorcyclist fatalities per 100,000 persons. All estimation samples consist of 48 states from 1976 to 2008. The unit of observation is a state-year. All models include indicators for years and states.
2) Standard errors, in parentheses, are robust to clustering within state over time. ***, **, and * indicate statistical significance at 1, 5, and 10 percent.

Table 2.3: Estimates of the Effect of Helmet Laws on the Fraction of Motorcycle Fatalities involving Non-Residents

Model:	Linear		Fractional Probit		Fractional Probit	
Estimation method:	Fixed effects		Pooled QMLE		GEE	
	(1)	(2)	(3)	(4)	(5)	(6)
Universal helmet law	0.0043	0.0062	0.0006	0.0052	0.0041	0.0054
	(0.0069)	(0.0086)	(0.0063)	(0.0073)	(0.0061)	(0.0072)
Fraction of neighbors w/ universal law	0.0148		0.0695***		0.0222	
	(0.0190)		(0.0176)		(0.0146)	
Fraction of neighbors w/ stronger relative law		0.0013		-0.0013		-0.0010
		(0.0160)		(0.0137)		(0.0137)
ln (total population)	-0.0675***	-0.0632***	-0.0800***	-0.0579***	-0.0654***	-0.0584***
	(0.0227)	(0.0213)	(0.0183)	(0.0165)	(0.0169)	(0.0161)
ln (heating degree days)	0.0079	0.0082	0.0060	0.0066	0.0062	0.0065
	(0.0164)	(0.0163)	(0.0131)	(0.0131)	(0.0131)	(0.0131)
ln (precipitation)	0.0270**	0.0267**	0.0254**	0.0241**	0.0246**	0.0242**
	(0.0132)	(0.0131)	(0.0111)	(0.0106)	(0.0107)	(0.0105)
State unemployment rate	0.0001	0.0001	-0.0006	-0.0002	-0.0003	-0.0002
	(0.0019)	(0.0019)	(0.0017)	(0.0017)	(0.0017)	(0.0017)
ln (gas price)	0.0022	0.0082	-0.0079	0.0181	0.0093	0.0172
	(0.0584)	(0.0585)	(0.0517)	(0.0510)	(0.0507)	(0.0509)
Speed limit ≥65 mph	-0.0177**	-0.0171**	-0.0186***	-0.0160***	-0.0167***	-0.0160***
	(0.0068)	(0.0068)	(0.0060)	(0.0058)	(0.0058)	(0.0058)
Primary seat belt law	-0.0076	-0.0081	-0.0003	-0.0036	-0.0024	-0.0034
	(0.0068)	(0.0067)	(0.0057)	(0.0052)	(0.0053)	(0.0052)

Notes: 1) The dependent variable is the fraction of motorcyclist fatalities involving non-residents (sample mean 0.1222). All estimation samples consist of 48 states from 1976 to 2008. The unit of observation is a state-year. All models include a full set of year indicators. The fractional probit estimation includes the time averages of the explanatory variables. Average partial effects are displayed for the fractional probit estimation.

2) Standard errors, in parentheses, are robust to clustering within state over time. ***, **, and * indicate statistical significance at 1, 5, and 10 percent.

Table 2.4: Fixed Effects Poisson Estimates of the Effect of Motorcycle Helmet Laws on Total and Non-Resident Fatalities

Dependent Variable:	Number of Motorcycle Fatalities			
	Non-residents Only		Total Fatalities	
	(1)	(2)	(3)	(4)
Universal helmet law	-0.2451***	-0.2620*	-0.3787***	-0.3745***
	(0.0656)	(0.1381)	(0.0358)	(0.0641)
Fraction of neighbors w/ universal law	-0.1399		-0.1104	
	(0.2257)		(0.0989)	
Fraction of neighbors w/ stronger law		-0.0076		0.0278
		(0.1469)		(0.0617)
ln (total population)	-0.2056	-0.2511	0.7531***	0.6964***
	(0.1942)	(0.1919)	(0.1149)	(0.1065)
ln (heating degree days)	-0.0310	-0.0387	-0.0797	-0.0918
	(0.0923)	(0.0926)	(0.0662)	(0.0672)
ln (precipitation)	-0.0946	-0.0857	-0.1574***	-0.1448***
	(0.0682)	(0.0712)	(0.0334)	(0.0340)
State unemployment rate	-0.0381**	-0.0383**	-0.0258***	-0.0251***
	(0.0158)	(0.0155)	(0.0065)	(0.0065)
ln (gas price)	0.0316	-0.0477	-0.3971**	-0.4784**
	(0.3728)	(0.3847)	(0.2005)	(0.2062)
Speed limit ≥65 mph	-0.0754	-0.0826	0.0110	0.0020
	(0.0806)	(0.0807)	(0.0292)	(0.0280)
Primary seat belt law	-0.0436	-0.0374	-0.0239	-0.0195
	(0.0733)	(0.0736)	(0.0380)	(0.0381)

Notes:

1) All estimation samples consist of 48 states from 1976 to 2008. The unit of observation is a state-year. All models include a full set of year indicators. The results displayed are coefficient point estimates and not average partial effects.

2) Standard errors, in parentheses, follow Wooldridge (1999). ***, **, and * indicate statistical significance at 1, 5, and 10 percent.

Table 2.5: Estimates of the Effect of Helmet Laws on Motorcycle Registrations, per Capita

	(1)	(2)	(3)
Universal helmet law	-0.2415***	-0.2517***	-0.1931***
	(0.0408)	(0.0396)	(0.0570)
Fraction of neighbors w/ universal law		0.1192	
		(0.0864)	
Fraction of neighbors w/ stronger law			0.1114
			(0.0767)
ln (total population)	-0.4078**	-0.4449**	-0.4288**
	(0.1966)	(0.1989)	(0.2010)
ln (heating degree days)	-0.0274	-0.0300	-0.0299
	(0.0310)	(0.0311)	(0.0312)
ln (precipitation)	-0.0127	-0.0098	-0.0075
	(0.0743)	(0.0746)	(0.0755)
State unemployment rate	-0.0325***	-0.0329***	-0.0314***
	(0.0097)	(0.0097)	(0.0097)
ln (gas price)	0.2174	0.1671	0.1995
	(0.2867)	(0.2924)	(0.2890)
Speed limit ≥65 mph	-0.0172	-0.0222	-0.0160
	(0.0639)	(0.0640)	(0.0632)
Primary seat belt law	0.0518	0.0568	0.0590
	(0.0474)	(0.0475)	(0.0479)

Notes:
1) The dependent variable is the natural log of motorcycle registrations per 10,000 persons. All estimation samples consist of 48 states from 1976 to 2008. The unit of observation is a state-year. All models include indicators for years and states.
2) Standard errors, in parentheses, are robust to clustering within state over time. ***, **, and * indicate statistical significance at 1, 5, and 10 percent.

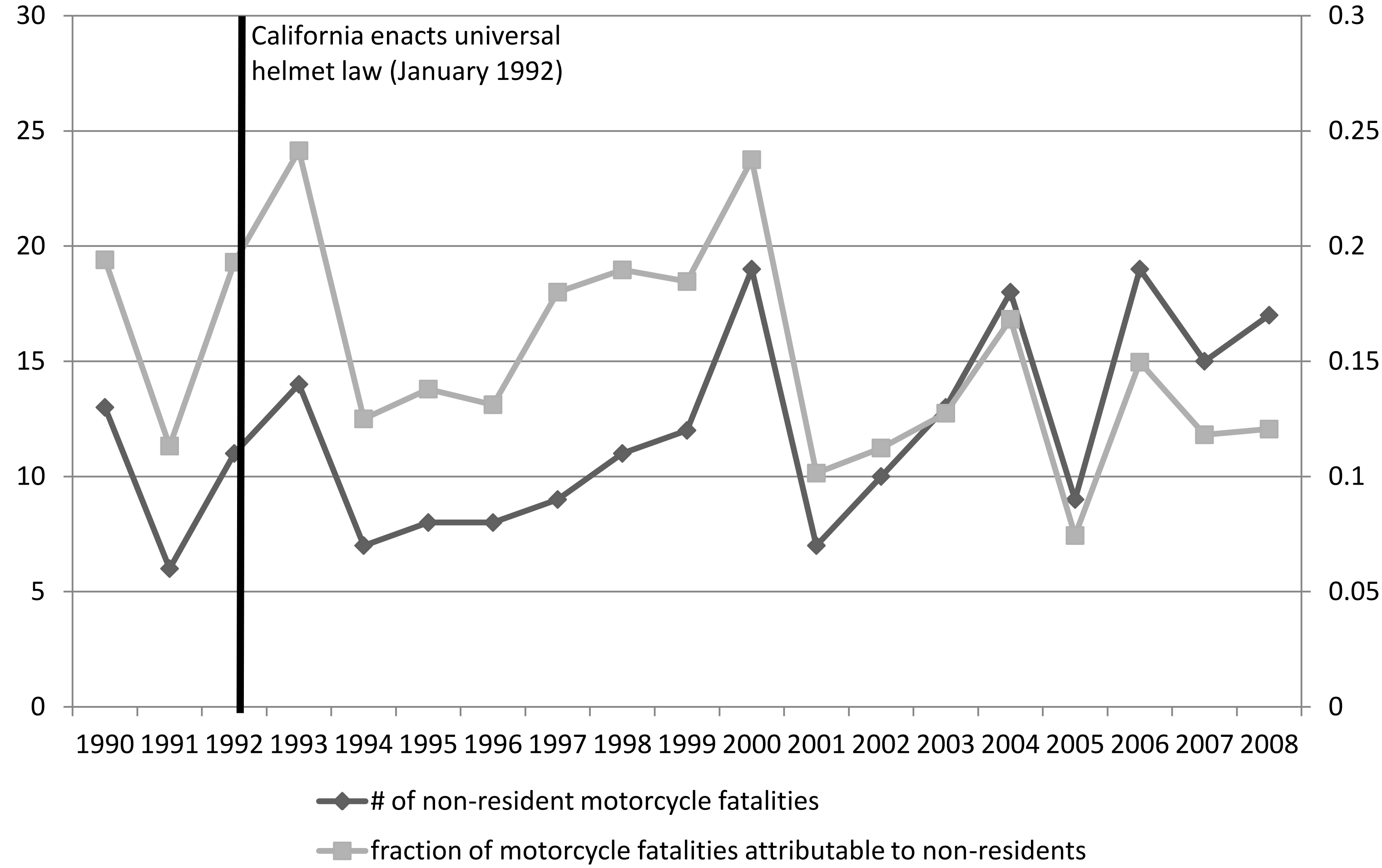
Table 2.6: Data Sources

Variable	Source
Helmet laws, speed limits, seat belt laws	Insurance Institute for Highway Safety http://www.iihs.org/laws/default.aspx
Motorcycle fatality data, state of crash occurrence, license state for deceased riders	National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS) http://www.nhtsa.gov/FARS
Heating degree days, annual precipitation	National Oceanic Atmospheric Administration (NOAA)
Population estimates	U.S. Census Bureau
State unemployment rate	U.S. Bureau of Labor Statistics
Motorcycle registrations	Federal Highway Administration Highway Statistics
Gas prices	U.S. Energy Information Administration State Energy Data System (SEDS) http://www.eia.gov/tools/faqs/faq.cfm?id=26&t=10
Border crossings	Federal Highway Administration National Highway System http://www.fhwa.dot.gov/planning/nhs
State borders and border lengths	Website of Dr. Thomas Holmes (University of Minnesota) http://www.econ.umn.edu/~holmes/data/BorderData.html

APPENDIX 2.2

CHAPTER 2 FIGURES

Figure 2.1: Arizona Non-Resident Motorcycle Fatality Trends

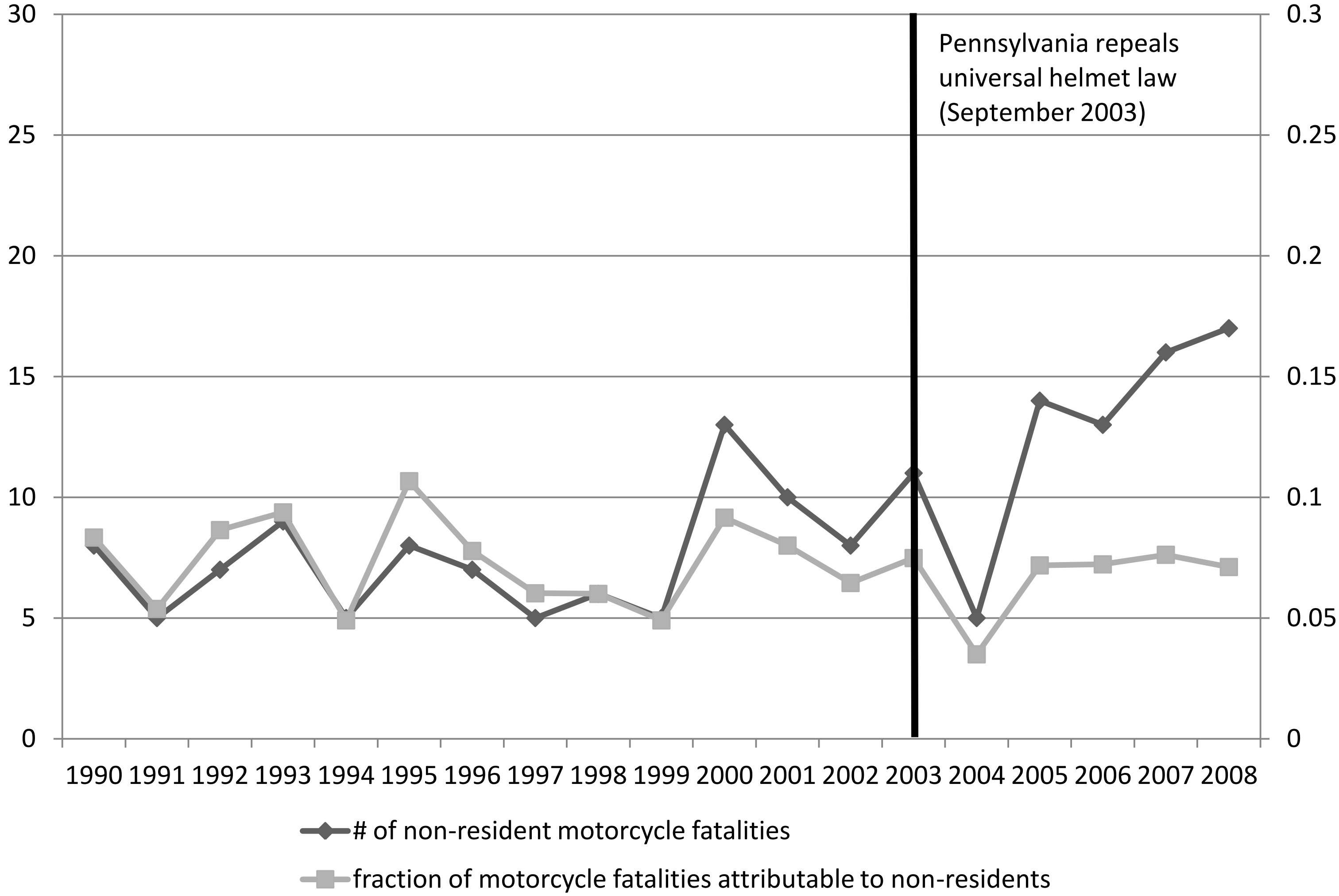


<u>Notes:</u>

1) Arizona has had a partial coverage helmet law since 1976.

2) Source: Author's calculation from FARS data.

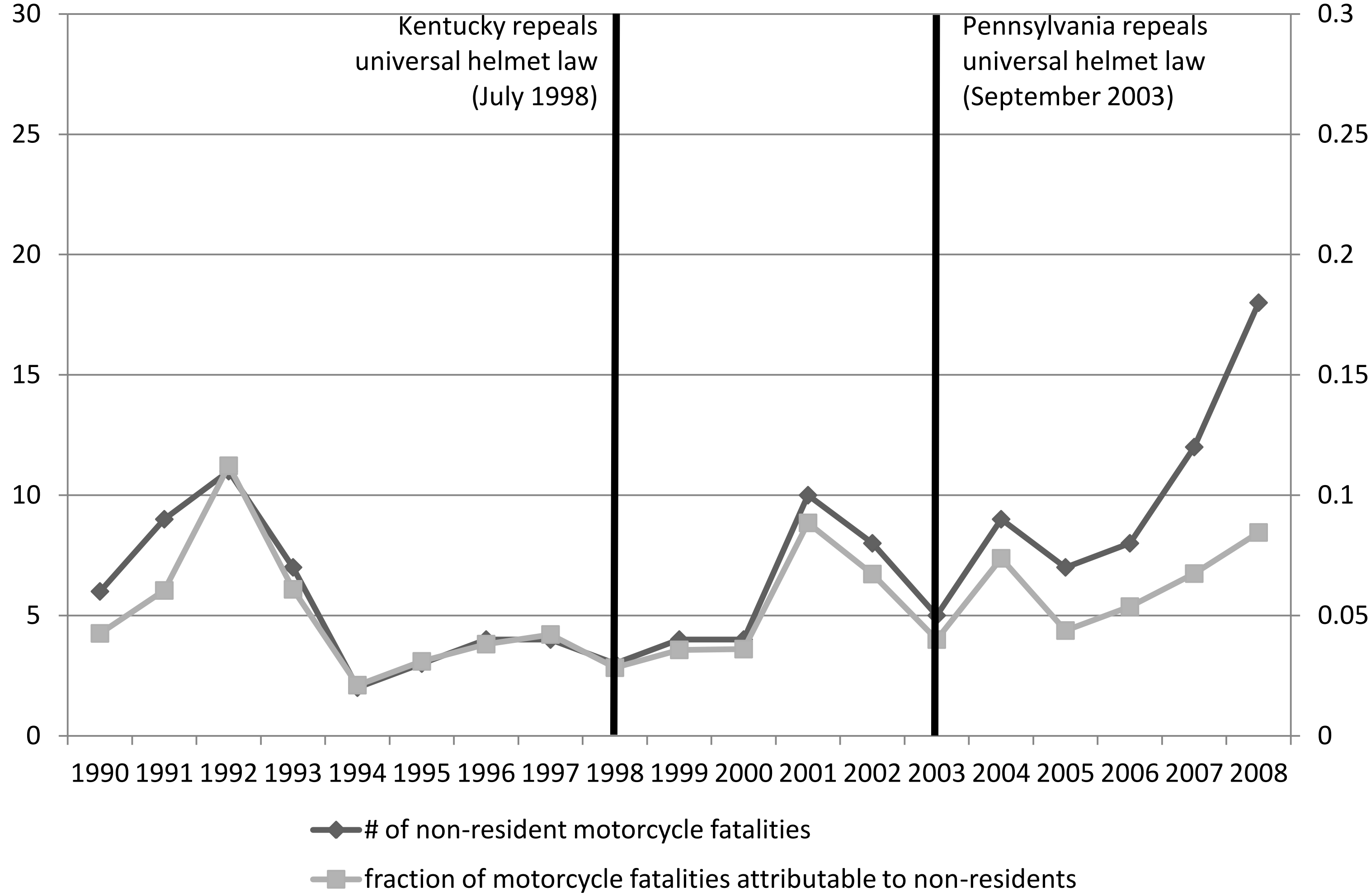
Figure 2.2: Pennsylvania Non-Resident Motorcycle Fatality Trends



<u>Notes:</u>

1) Source: Author's calculation from FARS data.

Figure 2.3: Ohio Non-Resident Motorcycle Fatality Trends

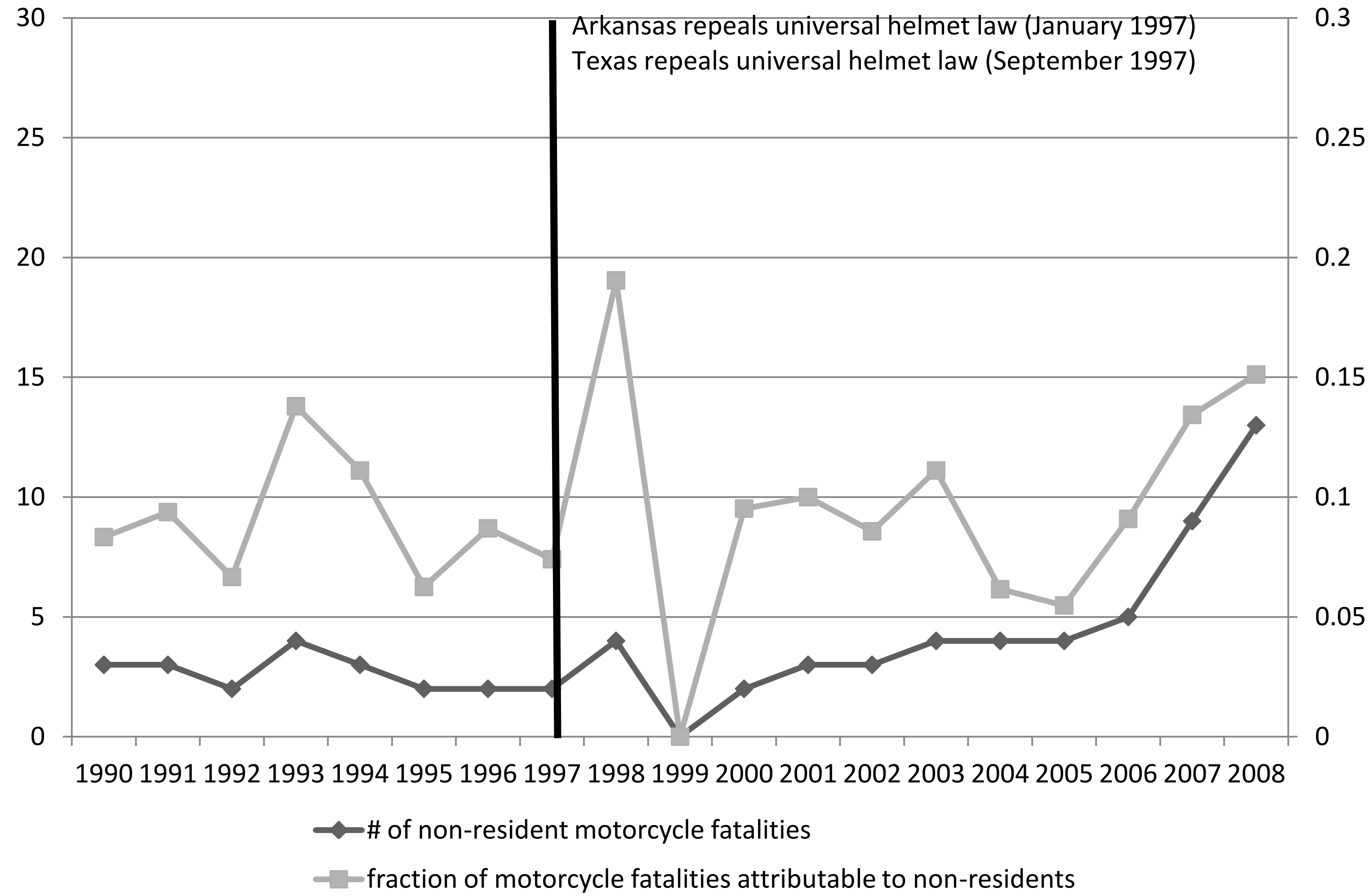


Notes:

1) Ohio has had a partial coverage helmet law since 1978.

2) Source: Author's calculation from FARS data.

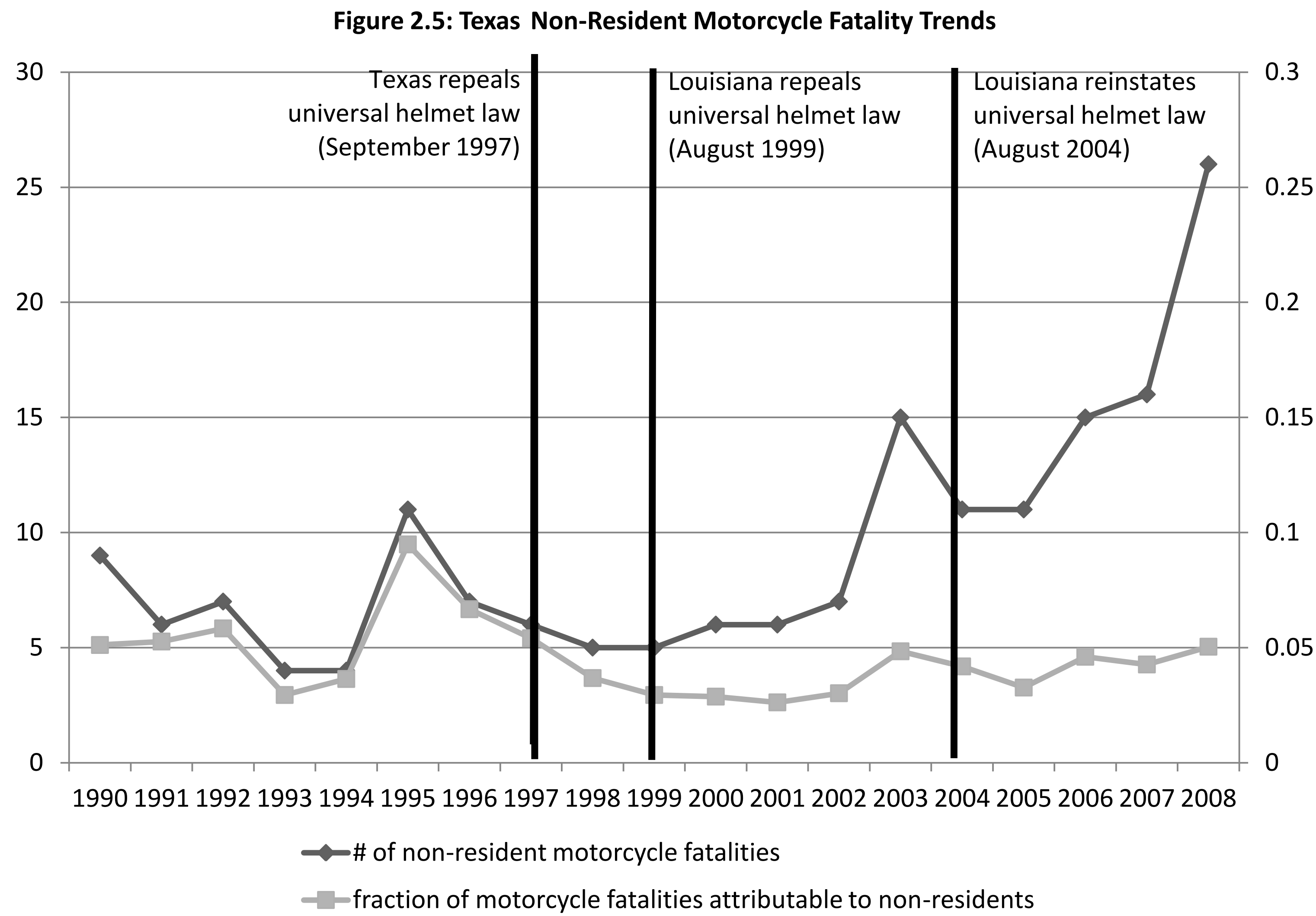
Figure 2.4: Oklahoma Non-Resident Motorcycle Fatality Trends



<u>Notes:</u>

1) Oklahoma has had a partial coverage helmet law since 1976.

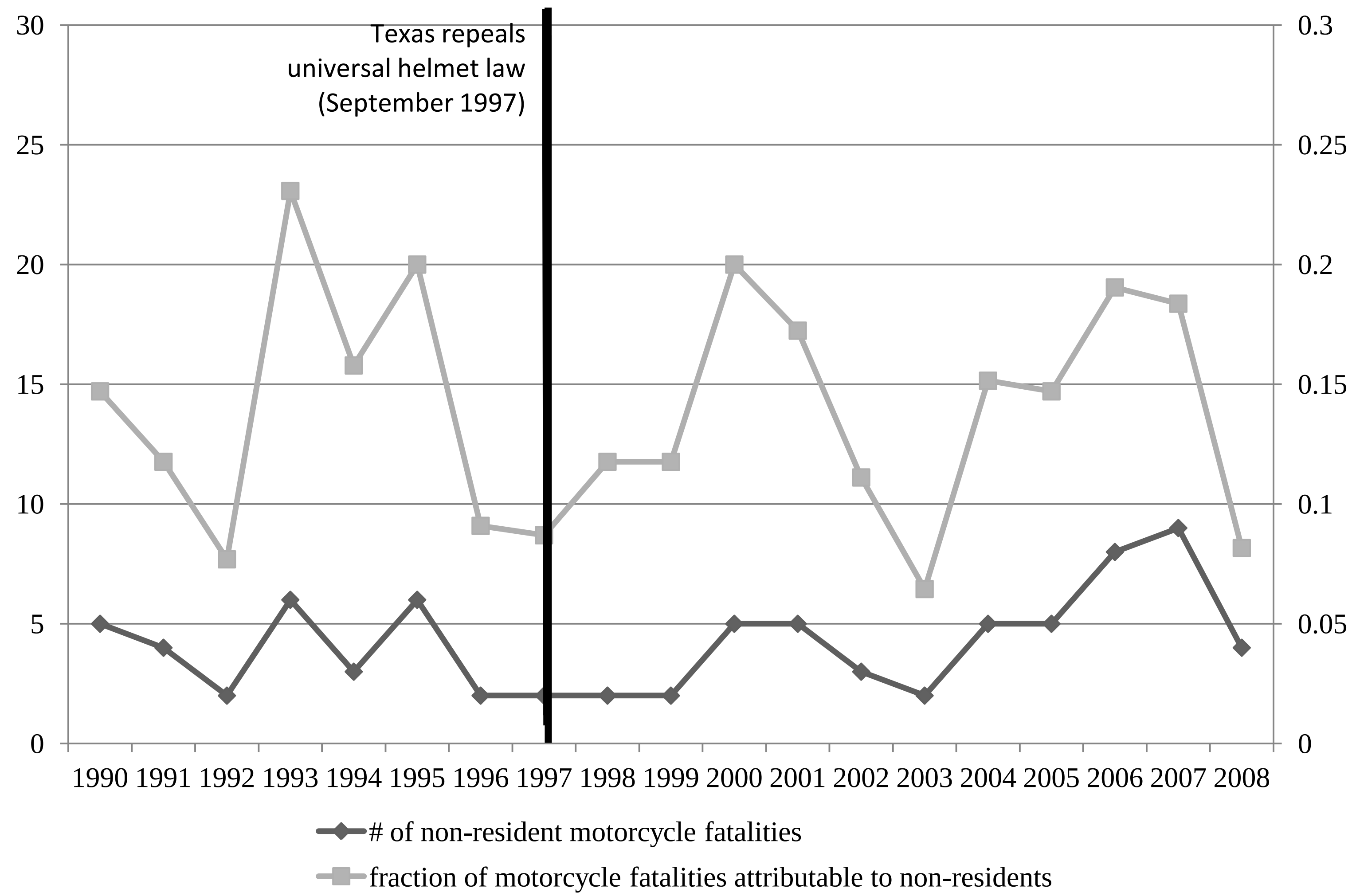
2) Source: Author's calculation from FARS data.



Notes:

1) Source: Author’s calculation from FARS data.

Figure 2.6: New Mexico Non-Resident Motorcycle Fatality Trends



<u>Notes:</u>

1) New Mexico has had a partial coverage helmet law since 1978.

2) Source: Author's calculation from FARS data.

CHAPTER 2 REFERENCES

CHAPTER 2 REFERENCES

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CHAPTER 3

DONORCYCLES:

MOTORCYCLE HELMET LAWS AND THE SUPPLY OF ORGAN DONORS

With

Stacy Dickert-Conlin and Todd Elder

"*Motorcycle fatalities are not only our No. 1 source of organs, they are also the highest-quality source of organs, because donors are usually young, healthy people with no other traumatic injuries to the body, except to the head… [a mandatory motorcycle helmet law] could put us out of business – or at least the business of organ transplants.*"

- Transplant surgeon quoted in "Brain Dead: Why are There No Mandatory Helmet Laws?" (Garrett, 2008)

3.1 Introduction

Empirical evidence consistently shows that motorcyclist deaths are disproportionately concentrated among those riding without a helmet. Based in part on this evidence, helmetless motorcyclists are perceived as a major source of transplantable organs, particularly within the medical trauma community.[52] The perceived link between helmet usage and organ donation even motivated two recently proposed (albeit failed) laws: in 2003, California Assembly Bill 1200 and New Mexico Senate Bill 239 would have made consent for organ donation *presumed*, rather than based on explicit written authorization, for all helmetless motorcyclists killed in traffic accidents.[53]

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[52] For example, Trauma.org, an organization of professionals in trauma and critical care, published a discussion about helmet laws that included several references to "donorcycles" and claims by physicians such as "[w]asn't there a study a couple of years ago which showed organ donations went down by a third when motorcycle helmet laws were strickly (sic) enforced?" http://www.trauma.org/archive/archives/helmet.html (accessed February 9, 2010).

[53] For the specific language of the California and New Mexico bills, see http://info.sen.ca.gov/pub/03-04/bill/asm/ab_1151-1200/ab_1200_cfa_20040109_124839_asm_comm.html and http://www.nmlegis.gov/Sessions/03%20Regular/bills/senate/SB0239.pdf, respectively (accessed February 10, 2010). Under presumed consent paradigms, motorcyclists can "opt out" of being potential donors only by signing a form explicitly prohibiting their organs for use in transplants.

Despite the perception that helmet usage reduces organ donation rates, no previous research has investigated whether such a link exists. Estimates of the strength of this relationship are essential to cost-benefit analyses of government regulation of helmet use. Currently, cost-benefit arguments are based on weighing personal freedoms against the negative externalities associated with helmetless motorcycling, including costs to public health programs resulting from the relatively high rates of injury and death among those involved in accidents (GAO 1991). This study will quantify an additional, unintended cost of government helmet regulation, as measured by its effect on the supply of organ donors.

Using state- and year-specific data on organ donations and variation across states and time in helmet laws, we present evidence that helmet laws reduce organ donation rates. Statewide helmet mandates are associated with roughly 10 percent reductions in the supply of organ donors who died in motor vehicle accidents (MVAs). In contrast, helmet laws are unrelated to the number of organ donors who died due to circumstances other than MVAs, such as homicide or natural causes. As further support for a causal interpretation of the estimates, helmet laws affect organ donation rates only among men, who account for more than 90 percent of annual motorcyclist fatalities, and are also concentrated among those aged 18 to 34. Our central results suggest that every death prevented by motorcycle helmet laws decreases the number of organ donors by 0.12. Based on this estimate, along with published estimates of the number of organs recovered per donor, each death that occurs among helmetless riders saves the lives of 0.33 persons on the vast organ transplant waiting lists.

In the following section, we review the history of helmet laws and describe the mechanisms by which helmet laws could influence the supply of organ donors. Section 3

As described by Abadie and Gay (2006), several European nations currently operate under a presumed consent paradigm.

describes the organ donation data from the Organ Procurement and Transplantation Network and how they relate to publicly available traffic fatality data. Section 4 presents the empirical specifications and results, and Section 5 concludes.

3.2 Institutional Details of Helmet Laws and Their Link to Organ Donation

Between 1966 and 1995, the federal government twice implemented and retracted acts that set guidelines requiring all motorcycle riders to wear helmets. Although federal legislation has never been a strict mandate to states, it has induced substantial swings in state-level legislation through explicit threats to cut federal highway funding to noncompliant states.[54] No federal helmet legislation currently exists, but state legislatures continue to debate and modify their own helmet laws. Currently, 20 states have universal coverage laws, 27 states have partial coverage laws that typically mandate helmet usage for all riders under age 18, and 3 states do not require any riders to wear a helmet.[55] Since the most recent federal guidelines for helmet

[54] The 1966 Highway Safety Act authorized the Secretary of Transportation to withhold up to 10 percent of federal highway construction funds from any state that did not adopt universal helmet laws. The Act was amended in 1976, eliminating the helmet law mandate and removing the authority to withhold funds. Under the 1991 Intermodal Surface Transportation Efficiency Act, states that enacted both universal helmet laws and seat belt laws would be eligible to receive federal grant money, while states that did not comply would be subject to a 3 percent reallocation of federal highway funds towards highway safety programs. The threat of reallocation was removed with the passage of the National Highway System Designation Act in 1995 (Houston and Richardson, 1995).

[55] For the purposes of this study, partial coverage laws are considered equivalent to having no helmet law due to enforcement difficulties and the high proportion of the riding population facing no restrictions. Although the most common partial coverage law requires helmets for riders under age 18, some states have restrictions for riders under age 15, 19, or 21. A handful of states currently require helmets for operators with instructional permits, less than one year of riding experience, or less than $10,000 of personal injury insurance. All helmet statutes specify maximum punishments for violation; for example, in Georgia, riding without a helmet is

mandates were removed in 1995, six states (Arkansas, Texas, Kentucky, Louisiana, Florida, and Pennsylvania) repealed universal helmet laws, with Louisiana reinstituting their universal law in 2004. Table 3.6 lists the timing of these seven law changes, which play a key role in the analyses below.

The decline in the prevalence of helmet laws since 1995 is perhaps surprising in light of strong evidence that these laws increase helmet use and reduce fatalities. Using data from the 2006 and 2007 National Occupant Protection Use Survey (NOPUS), a field study conducted by the National Highway Traffic Safety Administration to measure the use of motorcycle helmets and seat belts, we estimate helmet usage rates in states with a universal law to be 97.8 percent, compared to 54.2 percent in states with partial or no laws. Numerous studies using single-state data also find that helmet usage decreases from nearly 100 percent to roughly 55 percent following universal law repeals (see Berkowitz 1981, Dare et al. 1978, Gilbert et al. 2008, Kraus et al. 1995, Lund et al. 1991, Preusser et al. 2000, Struckman-Johnson and Ellingstad 1980, Ulmer and Northrup 2005, and Ulmer and Preusser 2003).

Several additional studies measure the effectiveness of helmets in protecting riders in the event of a crash, with arguably the most convincing approach based on within-vehicle variation in survival and helmet use among operator-passenger pairs. Using this approach, Dee (2009) finds that helmets reduce fatality risk by 34 percent. A related literature considers the effects of helmet laws on state-level fatality rates. Estimates based on within-state variation in helmet laws over time suggest that universal helmet laws reduce per capita fatalities by more than 20 percent

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punishable by a fine of up to $1,000 and one year in jail, but the typical punishment for a first offense is a fine of $90. See http://www.iihs.org/laws/HelmetUseCurrent.aspx for more information on current state helmet laws.

relative to partial laws and by 27 to 29 percent compared to having no laws at all (Dee 2009, Houston and Richardson 2008, Sass and Zimmerman 2000).[56]

The Logistics of Organ Donation

Although brain death is rare, occurring in less than 1 percent of all deaths in the U.S., almost all non-living organ donors are brain dead at the time of organ recovery. The crucial distinction between brain death, which involves the irreversible cessation of all brain function, and the more common classification of death (known as “cardiac death”) lies in the fact that the heart continues to beat after brain death occurs. Current medical technology allows for essentially indefinite respiration via a ventilator following brain death, so the internal organs receive oxygenated blood and remain viable for transplantation. In contrast, organs deteriorate rapidly following cardiac deaths and are therefore unsuitable for transplantation except in extraordinary circumstances.[57] If the brain dead patient is otherwise healthy and provided informed consent for donation, either directly or through family members, surgeons instigate the process of organ recovery.[58]

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[56] A number of additional studies focus on a single state before and after a helmet law change. For examples, see Auman et al. (2002), Bledsoe et al. (2002), Bledsoe and Li (2005), Eberhardt et al. (2008), Kraus et al. (1994), Mayrose (2008), Mertz and Weiss (2008), and Muller (2004, 2007).

[57] An example of such a circumstance is the growing but controversial practice of “non-heart beating donation”, in which patients with non-survivable brain injuries (who are not brain dead because they retain some minimal brain stem function) become donors. Donation in such cases entails removing the patient from a ventilator, typically in the operating room. Once the patient's heart stops beating, the physician declares the patient dead and organs are removed. See http://www.organtransplants.org/understanding/death/ for more details.

[58] “Otherwise healthy” individuals are defined as those younger than 70 and lacking contraindications to organ donation defined by the International Classification of Diseases.

The perception that helmetless motorcyclists are prime organ donor candidates is based on the notion that they can be killed in low-speed, relatively minor collisions which cause brain death but leave the rest of the body in pristine condition. In contrast, a deceased helmeted cyclist or automobile occupant is likely to have been in a violent collision that caused widespread internal damage and cardiac death, both of which are incompatible with organ donation. Although no previous research has documented the effects of helmet laws on brain deaths in particular, the existing evidence linking helmet use to motorcyclist death rates implies that the incidence of brain death is likely to be lower when helmet laws are in place.

The final link between helmet laws and the supply of organs for transplantation involves the transition from brain death to consenting organ donor. Although federal law has always maintained that health care professionals only need the potential donor's consent to recover organs, a practice known as first-person consent, health care professionals typically also seek permission from the potential donor's next-of-kin. Due primarily to low consent rates, donation rates among all potential donors range from 51 to 60 percent according to Howard et al. (2007). Despite this apparent inefficiency in organ procurement, policies such as helmet laws that affect the incidence of brain death will likely affect the supply of organs for transplantation. In the remainder of this paper, we measure the magnitude of this effect.

3.3 Data

The U.S. first established a unified transplantation network, the Organ Procurement Transplantation Network (OPTN), under directive from the 1984 National Organ Transplant Act.

Table 2 of Guadagnoli et al. (2003) lists these contraindications, which include cancer, HIV, hepatitis, and a number of other blood-borne infections.

This act provided the authority to divide the United States into mutually exclusive donation service areas (DSAs), each of which was assigned to an Organ Procurement Organization (OPO). Each OPO is a local monopoly within its DSA, exclusively responsible for coordinating and facilitating donation services. One of OPTN's key initiatives involves the collection and management of data from every donation and transplant occurring in the United States.[59]

The organ donation data used in this paper are DSA-level counts of deceased donors, available on the OPTN website (http://optn.transplant.hrsa.gov/). There are 57 operational DSAs in the U.S. that provide data to the OPTN. In most cases, the aggregation from DSA to the state level is straightforward, but some DSAs cover part or all of multiple states. In these cases, we assign the donation statistics for the entire DSA to the state where its OPO is headquartered. Because our identification strategy is based on state variation in helmet laws, this assignment may present problems if deaths from a county in a given state are designated to another state's donor counts. However, as we describe below, our results are insensitive to restricting our analysis to DSAs that include only one state. Aggregating to the state level results in 38 observations per year because 13 states do not contain an OPO headquarters (D.C. contains its own OPO); a complete listing of OPO locations can be found at http://unos.org/members/directory.asp.

[59] The National Organ and Transplant Act also outlawed the purchase and sale of organs and established the OPTN with the responsibilities of creating a system for matching organs to individuals. It also established OPOs as clearinghouses for acquiring useable organs, maintaining organ quality standards and allocating donated organs equitably (see http://optn.transplant.hrsa.gov/SharedContentDocuments/NOTA_as_amended_-_Jan_2008.pdf, accessed 5/19/09).

The OPTN website provides donor counts starting in 1988, along with data on the demographics, medical history, and certification of consent for each donor.[60] Beginning April 1st, 1994, OPTN also reports the donor's circumstance of death, with separate categories for MVAs, other accidents such as falls, child abuse, suicide, homicide, and, as of 1999, natural causes.

Table 3.1 presents the number of organ donors per million persons by circumstance of death and gender of donor from 1994 to 2007, with all circumstances other than MVAs aggregated to a single "All Others" category. Because the reporting of circumstance of death began in April of 1994, all donors from the first three months of 1994 are listed in the "All Others" category, making recorded MVA donation rates artificially low in 1994. From 1995 to 2007, donations resulting from circumstances other than MVAs rose steadily for both males and females, increasing by roughly 43 percent from 15.29 to 21.85 per million persons. In contrast, donations due to MVAs were essentially flat from 1995 to 2007, declining from 5.43 to 5.36 per million.

Because the MVA category includes donors killed in all motor vehicle accidents, rather than motorcycle accidents in particular, we also use fatality data from the National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS) to refine our analysis.[61] Table 3.2 summarizes motor vehicle fatalities by vehicle type and gender from 1994 to 2007. Overall fatality rates declined 13 percent, from 156.3 per million persons in 1994 to

[60] For a sample Deceased Donor Registration Worksheet, see http://www.unos.org/docs/Deceased_Donor_Registration.pdf.

[61] The full FARS file contains detailed information on every person involved in an accident on public roads that leads to at least one death within 30 days. Our analysis uses fatalities of vehicle operators and passengers aggregated to the state level.

135.3 in 2007, and declined roughly 20 percent (from 147.4 to 118.3 per million) among those in vehicles other than motorcycles. In contrast, motorcycle fatality rates increased by 90 percent over the same period, from 8.9 to 17.0 per million persons. Taken together, Tables 3.1 and 3.2 show that the per capita supply of donors killed in MVAs remained roughly constant between 1994 and 2007 in spite of steady decreases in traffic fatalities. Based solely on the yearly averages, it is unclear whether MVA donation rates were stable because of the dramatic increase in motorcycle fatalities or because of other factors, such as better technological methods of organ recovery or higher rates of consent.

Tables 3.1 and 3.2 also show substantial differences across gender in donation and death rates. In every year, men account for roughly 90 percent of all motorcycle fatalities but only two-thirds of deaths in other types of vehicles. Because motorcycle fatalities dramatically increased as a share of all MVA deaths between 1994 and 2007, the share of men in MVA deaths also increased over time. Returning to Table 3.1, MVA donations among men rose slightly from 1995 to 2007, from 3.55 to 3.74 per million persons, while female MVA donation rates fell from 1.89 to 1.71. These trends are consistent with increased motorcycle deaths being responsible for holding MVA organ donations roughly constant while non-motorcycle fatalities decreased by 20 percent. Moreover, the dramatic differences across gender contribute to the identification strategy we pursue below.

3.4 Do Motorcycle Helmet Laws Reduce Organ Donation Rates?

We next turn to assessing whether helmet laws affect the supply of organ donors. As described above, 38 states headquarter OPOs that report annual data on deceased organ donors.

Our primary empirical strategy involves estimating state- and year-specific organ donation rates as a function of whether the state had a universal mandatory helmet law in place in that year. We begin by estimating the following model:

$$Donors_{st} = \alpha_s + \delta_t + \gamma(law)_{st} + X_{st}\beta + \varepsilon_{st}\ , \qquad (1)$$

where $Donors_{st}$ is a measure of the number of deceased organ donors, s indexes the state in which the OPO is located, t indexes the year, and law_{st} is an indicator for whether state s had a universal mandatory helmet law for at least six months in year t. All specifications include a full set of state and year indicators (α_s and δ_t, respectively), and we indicate below when we also control for time-varying state-level variables X_{st}. The vector X_{st} includes the state's population, its square, and separate measures of the fractions of the population aged 18-34, 35-49, 50-64, and 65 and above; state maximum speed limits; separate indicators for whether the state had primary-enforcement and secondary-enforcement mandatory seat belt laws; climate variables correlated with motorcycle ridership (heating degree days and annual precipitation); and indicators for whether the state had an organ donor registry, whether online registration was available, and whether an OPO in the state enforced a first-person consent paradigm.[62] We weight each observation by the state's population in that year using U.S. Census Bureau estimates. Estimates

[62] Information on helmet and seat belt laws comes from the Insurance Institute for Highway Safety's website: http://www.iihs.org/laws/default.aspx. Primary-enforcement seat belt laws specify that police officers may stop vehicles solely on the suspicion that an occupant is not wearing a seat belt. Under secondary enforcement laws, officers may cite vehicle occupants for not wearing seat belts but cannot stop a vehicle solely for this purpose. Data on first-person consent practices, the existence of state donor registries and the ability to sign up for those registries online came from interviews with OPO employees. The National Oceanic and Atmospheric Administration supplied the climate data. Finally, estimates of annual state population levels and the corresponding within-state age distributions come from U.S. Census Bureau estimates.

of γ based on (1) capture the association between within-state variation over time in mandated helmet laws and within-state variation in organ donation rates.

Table 3.3 presents estimates of γ from specification (1) separately for donations due to MVAs and donations due to all other circumstances. The top row presents our central estimates, in which $Donors_{st}$ includes only MVA donors. In column (1), we measure $Donors_{st}$ as the number of donors per million state residents, so the estimate implies that a universal helmet law decreases the supply of organ donors by 0.491 per million state residents, with a standard error of 0.157 (all standard errors are robust to within-state clustering over time). As shown in column (2), inclusion of the time-varying covariates X_{st} does not markedly change the point estimate. In both cases, mandatory helmet laws are associated with roughly 10 percent reductions in organ donor rates relative to the sample average of 5.148 per million persons. In order to assess how sensitive these results are to the definition of the dependent variable, in columns (3) and (4) we define $Donors_{st}$ to be the absolute level of organ donors and its natural logarithm, respectively. These specifications lead to similar inferences as those in columns (1) and (2); specifically, we estimate that helmet laws reduce MVA organ donors by roughly 12.7 percent (= -7.696 / 60.595) when measured in levels and by roughly 9.7 percent when measured in natural logs. Using donors per capita as the dependent variable, as in columns (1) and (2), allows us to interpret the estimated coefficients as the effect of helmet laws on donors among the population at risk to donate, and provides a straightforward interpretation below when we consider the effect of helmet laws on the excess demand for organs. We therefore focus on specifications using per capita dependent variables from this point forward.

The principal threat to the internal validity of estimates based on specifications such as (1) stems from differential unmeasured time trends across states in organ donation rates that may be correlated with the presence of helmet laws. These time trends could reflect hospitals improving their procedures for organ recovery, education campaigns encouraging informed consent, or endogenous law changes in response to declining donation rates. If differential trends drive the negative point estimates in the top row of the table, they would likely also induce a negative association between helmet laws and the number of donors due to circumstances other than MVAs. As shown in the "All Others" row of the table, this is not the case – in all four specifications, the point estimates are both positive and statistically indistinguishable from zero at conventional significance levels. The remaining rows of the table present estimates for the individual circumstances that comprise the aggregate "All Others" category. None of these circumstance-specific organ donation rates is significantly related to helmet laws in any specification.

As Tables 3.1 and 3.2 showed above, motorcycle fatalities and MVA donation rates differ substantially across gender, so in Panel A of Table 3.4 we present gender-specific estimates of the effects of helmet laws. Columns (1), (3), and (5) in the first row show the effect of helmet laws on motorcycle fatalities, given by estimates of γ based on a specification similar to that in (1):

$$Deaths_{st} = \alpha_s + \delta_t + \gamma(law)_{st} + X_{st}\beta + \varepsilon_{st} , \qquad (2)$$

where $Deaths_{st}$ denotes the number of annual motorcycle fatalities per million state residents. Population-weighted average death rates are shown in brackets. Column (1) shows that universal helmet laws decrease deaths among male motorcyclists by 3.962 per million persons, a 39 percent decline relative to the baseline male death rate of 10.218. Column (3) shows that the absolute effect is much smaller among women, 0.395 per million persons. This reduction is roughly 38 percent of the baseline death rate of 1.039, similar to the relative effect among men. Column (5) presents estimates of γ based on pooled-gender death rates. The 39 percent reduction (= 4.369 / 11.258) in fatalities is broadly consistent with the findings of Dee (2009), for example, whose estimates range from 27 to 34 percent in specifications using the logarithm of fatalities as a dependent variable. In sum, helmet laws decrease motorcyclist deaths among both genders, but the absolute decrease is much larger among men because men account for over 90 percent of all motorcyclist deaths.[63]

The gender-specific fatality estimates in Table 3.4 suggest an intuitive test for whether the association between helmet laws and organ donors is causal – because helmet laws can influence organ donations only through their effect on motorcycle fatalities, the absolute effects of helmet laws on donation rates should be substantially larger among men than among women. We therefore present gender-specific estimates of the effect of helmet laws on MVA donation rates in the even-numbered columns of Table 3.4. The results are striking. Helmet laws have large, statistically significant effects on the number of male MVA donors, with the estimate of −0.558 (0.109) being over 16 percent of the baseline male MVA donor rate. In contrast, the

[63] The similarity across gender in relative effect sizes may result from a similarity across gender in the effects of helmet laws on helmet usage. Mayrose (2008) finds that in a sample of fatally injured motorcycle riders and passengers, the effects of statewide helmet mandates on helmet use are roughly constant across gender, with 83.8 percent of males and 85.8 percent of females wearing a helmet in states with a universal helmet law, compared to only 36.4 percent and 34.9 percent in states with a partial law. NOPUS observational data, which are not limited to those who were involved in a fatal accident, do not differentiate by gender, but these data do allow for estimates of helmet usage separately for drivers and passengers. In our analyses of these data, available upon request, the relationship between helmet laws and helmet usage does not significantly differ between drivers and passengers, suggesting that it also does not differ across gender.

estimated effect among women is positive, although the point estimate of 0.015 (0.061) is both practically and statistically insignificant. Although helmet laws reduce motorcycle fatalities among women, the reduction is sufficiently small relative to the number of female MVA deaths that it does not produce a measurable effect on the supply of female organ donors; as Table 3.2 showed, only 4 percent of all female MVA fatalities in 2007 involved motorcycles, compared to 16 percent among men. We view this pattern as compelling evidence of a causal effect of helmet laws on organ donations, because the most likely sources of unmeasured confounding trends would affect organ donation rates among both men and women.

As another check on the plausibility of the results shown above, the remaining rows in Panel A present estimates of equations (1) and (2) separately by the age categories included in the OPTN data.[64] We again expect the absolute effects of helmet laws on donation rates to be largest among the groups most likely to be involved in motorcycle fatalities, and the results strongly confirm this expectation. Roughly 47 percent (= 4.759 / 10.218) of motorcycle fatalities among men occur in the 18 to 34 age range, and helmet laws reduce fatalities in this age group by 2.018, or 44 percent of the baseline rate. Men aged 18 to 34 also experience by far the largest decline in MVA donors in response to helmet laws: 31 percent of the baseline donation rate (= -0.526 / 1.716). Among men, the other age groups show much smaller relationships between helmet laws and fatalities, as well as much smaller effects of helmet laws on MVA organ donors, providing further support for a causal interpretation of the relationship between helmet laws and MVA donors among men aged 18 to 34.[65] We also estimated these age- and gender-specific

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[64] We thank an anonymous referee for suggesting these specifications.

[65] We assessed the sensitivity of these central results to four functional form and measurement issues. First, because states enacted or repealed helmet laws in the middle of calendar years (see

models using *total* organ donors as a dependent variable, rather than MVA donors. The results of these specifications, available upon request, are broadly similar to those shown in Table 3.4, albeit less precisely estimated – total donations among men and those aged 18-34 increase in response to helmet law repeals.

As a final set of specification tests, we next consider the effects of helmet laws on the supply of donors who were killed in accidents that did not involve motor vehicles. As noted above, the principal threat to the validity of our central estimates lies in the possibility that state-specific unobserved trends in organ donation rates (or in organ extraction technology) may be correlated with the presence of helmet laws. It is plausible that these changes similarly affect all accident victims, regardless of whether the accidents involved motor vehicles. As a result, we view donors who died in non-motor vehicle accidents (NMVAs) as a more natural "control group" than the full set of donors who were not killed in MVAs (which includes those who died via homicides, suicides, and natural causes, in addition to those who died in NMVAs).

Table 3.5 presents three sets of age- and gender-specific estimates that comprise this final specification test. The first set, in columns (1), (4), and (7), simply reproduces the difference-in-difference estimates for MVA donors shown in Table 3.4. The second set, in columns (2), (5), and (8), shows the analogous estimates based on specifications in which the dependent variable

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Table 3.6), we estimated all models using a measure of the fraction of the year in which a state's helmet law was in place as the key regressor. Second, we treated the dependent variable as a count variable, estimating all models by Poisson quasi-maximum likelihood, and alternatively measured it as the log of per capita death and donor rates. Third, we re-estimate equations (1) and (2) six times, each time dropping one of the six states that repealed its helmet law, in order to test the possibility that only one or two states are driving the results. Finally, we excluded from the analysis all DSAs that cover multiple states (such as the Kansas City-based DSA, which covers parts of both Kansas and Missouri**)** because the state in which a death occurred is ambiguous in these cases. None of these alternative specifications yielded substantively different results from the central ones reported in the text. All alternative results are available upon request.

is the per capita supply of NMVA donors. Finally, the third set of estimates, in columns (3), (6), and (9), is the difference between the first two, representing a triple-difference that identifies the differential effect of helmet mandates on MVA donors relative to NMVA donors. On the whole, the results provide additional support that the central estimates in Table 3.4 capture real effects of helmet laws. NMVAs are not strongly associated with helmet laws in either the full sample or among those most likely to die in motorcycle accidents, such as men and those aged 18-34. Among men aged 18-34, the NMVA estimate is -0.018, compared to -0.526 for the MVA estimate, so the triple-difference estimate is -0.508 (= -0.526 + 0.018). The triple-difference estimates are also negative and statistically significant for males as a whole and in the pooled-gender sample of those aged 18-34. In all other age and gender categories, the estimates are sometimes positive, sometimes negative, but always statistically insignificant. Note that the triple-difference estimate in the overall pooled-gender sample (shown in the top row of column (9)) is not statistically significant, partly due to a smaller point estimate than that in column (7) and partly due to decreased precision because the third difference introduces an additional source of noise to these estimates. Curiously, helmet laws are significantly negatively associated with female NMVA donors aged 35-49, but this finding appears to be an anomaly given that none of the other nine gender- and age-specific estimates are distinguishable from zero.

The estimates in Table 3.5 also shed light on whether motorcycle helmet laws reduce deaths and organ donations overall or merely shift deaths and donations between circumstances in a form of "crowd out". In particular, risk-lovers who die in motorcycle accidents may have instead pursued other risky activities had their state imposed a universal helmet law.[66] Under this hypothesis, helmet laws would increase the supply of donors who died in NMVAs, leading

[66] We thank Anup Malani for suggesting this possibility to us.

to ambiguity in the overall effect of laws on the supply of donors who die in *all accidents*. As Table 3.5 shows, there is no evidence to support such a crowding out hypothesis: none of the NMVA estimates in the table is positive and significant. Among males aged 18-34, who are at the highest risk of all types of accidents and thus most likely to exhibit crowding out, the estimated effect of helmet laws on NMVAs is actually negative and insignificantly different from zero at -0.018 (0.031).

The Dynamic Effects of Helmet Laws

In order to highlight dynamic patterns that the point estimates in Tables 3.3-3.5 do not show, Figures 3.1-3.3 present graphical evidence of the effects of helmet laws. Figure 3.1 shows yearly motorcycle fatality rates among men in two groups: those in the six states that repealed their universal helmet laws from 1994 to 2007, and those in the 32 remaining states and D.C. For the states in the former group, the *X*-axis measures the year relative to the state's law change, with zero denoting the year of the repeal, 1 denoting the following year, and so on. For each state without a law change, "year zero" was randomly generated to equal either 1997, 1998, 1999, 2000, or 2003 with equal probability, because these years corresponded to actual law changes in the other group of states. As the figure shows, death rates in the two groups are roughly equal in the three years prior to the repeals, but starting in year zero deaths increase more rapidly in the treatment states than in the control states. By year 4, the death rates in the treatment states are 14.98 per million persons, compared to 11.88 in the control states.

Figure 3.2 shows MVA organ donation rates among men aged 18 to 34, separately for the treatment and control states. The divergence between the two groups of states is consistent with the estimate of γ of -0.526 shown in Table 3.4 – the difference between the series increases from roughly 0.15 in years minus 1 and zero to nearly 0.7 in year 4. Note that the horizontal axis begins at minus 2 in this figure because the earliest law repeal occurred in 1997, and the OPTN data do not allow for a clean distinction between MVA and non-MVA donors until 1995.

Perhaps surprisingly, Figures 3.1 and 3.2 imply that helmet law repeals induce gradual increases in both motorcyclist death rates and organ donation rates, rather than discrete changes in the year of the repeal. Figure 3.3 illustrates one reason why: per capita motorcycle registrations increase gradually following helmet law repeals. Registration rates increased by 42 percent over the four years following repeals, from 11.7 to 16.6 per thousand state residents. In the control states, registrations increased by 15 percent, reflecting increased ridership in the U.S. in the late 1990s and early 2000s. This pattern implies that at least some of the association between helmet laws and deaths results from an effect of helmet laws on motorcycle ridership.[67] Although speculative, it is also possible that those induced to ride by the repeals are relatively risk-loving or accident-prone compared to those who rode while the laws were in place. In order to assess whether these visual patterns are representative of the trends in each of the six treatment states, Figures 3.6-3.8 show analogous graphs separately for each state. The individual states exhibit patterns that are similar (albeit noisier) to those found in the aggregated treatment group.

The lack of an immediate effect of helmet law repeals on motorcyclist deaths and organ donations may also reflect a gradual effect of repeals on helmet usage. Ulmer and Preusser

[67] In agreement with the evidence in Figure 3.1, models analogous to specifications (1) and (2) with per capita registrations as the dependent variable point to large effects of helmet laws on registration rates. The coefficient on law_{st} is -3.317 (0.675), implying that helmet laws decrease registrations by roughly 14 percent of the baseline registration rate of 23.374 per 1000 persons. Similarly, the estimated effects of helmet laws on death rates decrease slightly when registrations per capita is included as a regressor, from -4.369 to -3.887. All data on registrations come from the U.S. Department of Transportation; see http://www.fhwa.dot.gov/policy/ohpi/hss/index.cfm for data from recent years.

(2003) show that observed helmet usage declined from 96 percent to 76 percent immediately following the elimination of Kentucky's universal law in 1998, but that usage continued to gradually decline over the next three years, falling to 56 percent in 2001. In contrast, observational studies show that the introduction of helmet laws immediately increases helmet usage from roughly 55 percent to nearly 100 percent (Kraus et al., 1995).

The asymmetric effects of helmet law repeals and introductions on helmet usage imply corresponding asymmetries in the effects on death rates and organ donations. To illustrate the possibility that behavior responds more quickly to the introduction of helmet laws than to their repeal, Figures 3.4 and 3.5 show motorcyclist registration and death rates for Florida, Texas, and California, the three most populous states with helmet law changes after 1990.[68] Figure 3.4 shows registration rates in the three states, and Figure 3.5 shows death rates. Each graph includes two series, one labeled "State with law change" based on data from the given state, and the other labeled "States with no law change" based on data from the 40 states which have neither repealed nor enacted helmet laws since 1990. Each series is normed to equal 1 in the first full year of a new regime, represented by the vertical lines in each graph; for example, Florida repealed their helmet law in July of 2000, so each series equals 1 in 2001 in the graphs labeled "Florida". Registrations and death rates in Florida and Texas were similar to those in the control states before the helmet law repeals but increased steadily relative to the control states following the repeals. California, which introduced a universal helmet law in 1992, shows a much different pattern. Death rates dropped by 38 percent between 1991 and 1992 while registration rates changed only negligibly, suggesting that the sudden decline in deaths reflected increased helmet

[68] California is included as a "control" rather than a "treatment" state in our analyses of organ donations because its law change occurred in 1992, before the donation data included circumstance of death.

usage rather than a registration effect. Death rates declined an additional 40 percent from 1992 to 1998, mirroring a 40 percent decline in registered motorcyclists over the same period.

The patterns in Figures 3.4 and 3.5 imply that the results shown above, which are based on an average of seven years of data following law changes, understate the long-run effects of repeals that gradually influence both registration rates and helmet usage. Moreover, because the timeframe of OPTN data availability covers six repeals and only one introduction, the estimates may understate both the short- and long-run impacts of helmet law introductions.

To further investigate the dynamic impacts of helmet law repeals, we estimated variants of specifications (1) and (2) that include a linear trend in the number of years since a state repealed its law, measured by the variable "*years since repeal*":

$$Y_{st} = \alpha_s + \delta_t + \lambda(law)_{st} + \varphi(years\ since\ repeal)_{st} + X_{st}\beta + \varepsilon_{st}\,, \tag{3}$$

where Y_{st} refers to either deaths or donors per capita.[69] For death rates, the pooled-gender estimates of λ and φ are -2.05 (with a standard error of 0.60) and 0.77 (0.15), respectively, implying an effect of 4.75 (0.62) at 3.5 years after a repeal. This number is statistically indistinguishable from the pooled-gender estimate of 4.369 in Table 3.4, which captures the effect at roughly 3.5 years following repeals because it is based on an average of seven years of post-repeal data. Similarly, in models of MVA organ donation rates, the pooled-gender estimates of λ and φ are -0.27 (0.23) and 0.10 (0.05), implying an effect of 0.63 (0.18) at 3.5 years following a repeal. The estimated effect rises to 0.77 (0.21) after 5 years and to 0.98 (0.27) after 7 years.

[69] We did not attempt to estimate dynamic effects of the introduction of Louisiana's law in 2004, primarily because we are hesitant to make inferences based on a single law change.

In order to consider whether the treatment and control states had differential trends in fatalities and organ donations that were not due to variation in helmet laws, we also estimate variants of equation (3) that include state-specific linear time trends. The inclusion of these additional variables does not substantially change the point estimates. For death rates, the pooled-gender estimate of λ declines to -1.66 (0.61) and the estimate of φ increases to 0.85 (0.21), implying an effect of 4.64 (0.72) at 3.5 years after a repeal, similar to (although slightly noisier) the effect of 4.75 implied by the specification that does not include state-specific trends. Similarly, the implied effect on MVA donation rates declines from 0.63 to 0.58 (0.24) when state-specific linear trends are included in (3). Although for brevity's sake we do not report the full set of results based on equation (3), these estimates are available upon request.

Instrumental Variables Estimates of the Effect of Motorcyclist Deaths on Organ Donation Rates

The estimates of γ from models (1) and (2) capture the reduced-form relationships between helmet laws and organ donors and between helmet laws and motorcyclist deaths, respectively. As a result, the ratio of the two estimates of γ represents a Wald IV estimate of the effect of motorcyclist death rates on organ donation rates. A causal interpretation of this ratio essentially requires that helmet laws only influence organ donation rates through their effect on motorcyclist deaths. Although we cannot formally test this assumption, the informal evidence presented above suggests that it holds. Specifically, because helmet laws appear unrelated to non-MVA donation rates and to female MVA donation rates, we interpret the estimated ratio as measuring a causal effect of motorcyclist deaths on organ donation rates.

The Wald IV estimates, shown in Panel B of Table 3.4, are 0.141 for men (= 0.558 / 3.962), -0.037 for women (= 0.015 / -0.395), and 0.124 for the pooled-gender sample (= 0.543 /

4.369). These estimates capture the effect of the excess motorcyclist deaths resulting from state repeals of helmet laws on the supply of organ donors.[70] Taken literally, the pooled-gender estimate implies that every motorcyclist death due to the lack of a universal helmet law produces 0.124 additional organ donors. For men aged 18 to 34, this number increases to 0.261 (= 0.526 / 2.018).

Based on the estimates of the sensitivity of organ donation rates to motorcyclist death rates, we can calculate the size of the positive externality resulting from each helmetless motorcyclist's death, as measured by possible lives saved. According to OPTN data, 2.7 organs are successfully transplanted per deceased donor, on average. Under the upper-bound assumption that each of these organs saves one life, the pooled-gender estimate of 0.124 implies that every death of a helmetless motorcyclist saves the lives of 0.33 (= 0.124 × 2.7) individuals on organ transplant waiting lists.[71] Because this upper-bound estimate is far less than one, helmetless riding is clearly an inefficient means of preserving life in the absence of a basis for making normative judgments about the value of one life relative to another. Nonetheless, this calculation highlights the possibility that helmet laws impact death rates even among those who do not ride motorcycles.

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[70] For instrumental variables estimates to be interpreted as local average treatment effects, Imbens and Angrist (1994) show that a potential instrument must also satisfy the property of monotonicity. Although monotonicity is untestable in the context of a binary instrument such as law_{st}, it is likely to hold in this setting – it implies that the presence of a helmet law does not increase motorcyclist death rates in any state.

[71] According to the 2007 OPTN annual report, one-year survival rates are 94 percent for kidney recipients, 87 percent for liver recipients, and 88 percent for heart recipients, accounting for the three most transplanted organs (Table 1.13). The same report shows that multiple organ transplants are very rare, accounting for fewer than 2 percent of all transplants (Table 1.08). These numbers, and many others in this section, are taken from OPTN's rich national data available for public use on OPTN's website: http://optn.transplant.hrsa.gov/latestData/step2.asp.

As a further gauge of the magnitude of the estimates in Table 3.4, recall that in 2007, 135.3 MVA fatalities resulted in 5.36 organ donors per million persons (see Tables 3.1 and 3.2). Each MVA death therefore produced 0.040 organ donors, and this overall donor rate, D, is a weighted average of the donor rates among helmetless motorcyclists (D_{hm}) and all others involved in MVAs (D_o):

(4) $$D = P \times D_{hm} + (1\text{-}P) \times D_o,$$

where P is the proportion of all MVA fatalities that involve helmetless motorcyclists. The pooled-gender estimate of 0.124 represents an estimate of D_{hm}. Based on 2007 FARS national data on traffic fatalities, P equals 0.050, implying that D_o equals 0.035 – while 12.4 percent of helmetless motorcyclists killed in MVAs eventually become organ donors, the analogous rate among all other MVA fatalities is only 3.5 percent. This discrepancy presumably results from higher rates of brain death among helmetless motorcyclists than among others killed in MVAs. Howard et al. (2007) estimate that 51 percent of viable donors actually become donors, so roughly 24 percent (= 0.124 / 0.51) of deceased motorcyclists are viable donors, i.e., brain dead but otherwise healthy. Although we are wary of interpreting this number literally because it is based on the assumption that consent rates among motorcyclists are equal to those in the rest of the population, to our knowledge it represents the first estimate of the fraction of deceased helmetless motorcycle riders who are viable organ donors.

Helmet Laws and the Excess Demand for Organs

Finally, we consider the magnitude of the effect of helmet laws in the context of a broader question: how would eliminating all existing universal helmet laws affect annual organ donations and transplant waiting lists? According to state-level population estimates from the U.S. Census Bureau, approximately 155 million people lived in states with universal helmet laws in 2007. Based on the estimates from Table 3.4, which measure the average effect over the seven years following a law change, the repeal of all helmet laws would increase the annual number of deceased motorcyclists by roughly 677 (= 4.369 × 155), or 13.2 percent of the 5,128 deaths in 2007. These deaths would produce 84 (= 0.543 × 155) additional organ donors because 12.4 percent of deceased motorcyclists become donors. Based on OPTN's estimate of 2.7 transplanted organs per donor, 227 additional organs would be transplanted. Alternatively, the dynamic estimates from model (3) imply that 332 additional organs would be transplanted five years after a nationwide repeal. These numbers represent 3.1 and 4.6 percent, respectively, of the 7,287 individuals who died in 2007 while waiting for a transplant.[72]

[72] The dramatic shortage of organs has generated a large body of research that evaluates mechanisms for increasing organ donation rates. Abadie and Gay (2006) find that countries using presumed consent donation standards have 25 to 30 percent higher donation rates than observationally similar countries using informed consent regimes. In a special issue of the *Journal of Economic Perspectives* addressing the excess demand for organs, Becker and Elias (2007) focus on financial incentives for increasing living donors, Howard (2007) reviews policies for increasing consent rates and the pool of potential donors, and Roth (2007) discusses the compensation of organ donors in light of "repugnance" for the market trading of organs. In a series of papers, Roth, Sönmez, and Ünver (2004, 2005) design a matching mechanism for organ recipients and donors that has been implemented in New England.

3.5 Conclusions

Motorcycle helmet mandates are effective: consistent with a larger literature, our estimates indicate that state-level motorcyclist fatalities increase by approximately thirty percent when universal helmet laws are repealed. Despite the effectiveness of helmets in preventing deaths, helmet usage rates in states without universal laws suggest that nearly half of all motorcyclists prefer to ride helmetless. Helmet mandates impose costs on these riders, but these costs may be justified by a reduction in the negative externalities imposed by those injured or killed in accidents.

This study is the first to assess the anecdotal belief that helmet laws also decrease the *positive* externalities of helmetless riding by reducing the supply of organ donors. Our central estimates show that organ donations due to motor vehicle accidents (MVAs) increase by 10 percent when states repeal helmet laws. Nearly all of this effect is concentrated among men, who account for over 90 percent of motorcyclist fatalities, and it is also concentrated among those aged 18 to 34. Helmet laws are unrelated to the number of organ donors who died in circumstances other than MVAs, providing further support for a causal interpretation of the association between helmet laws and MVA donors. Under the upper-bound assumption that each recovered organ saves one life, the estimates imply that every motorcyclist death due to the lack of a helmet law saves the lives of 0.33 individuals on organ transplant waiting lists. Quantifying the unintended consequences of helmet laws allows for more informed policymaking by providing a more complete picture of the associated costs and benefits. Based on our preferred estimates, 3.1 to 4.6 percent of those who died while awaiting an organ in 2007 would have instead received a transplant if all helmet laws were repealed. These estimates may understate the long-run impacts of helmet laws on organ donations, but they show that helmet laws profoundly affect the lives of some potential transplant recipients. This unintended consequence merits consideration in cost-benefit analyses of helmet laws, which currently focus on weighing the costs to society of helmetless motorcycling against motorcyclists' freedom of choice.

CHAPTER 3 APPENDICIES

APPENDIX 3.1

CHAPTER 3 TABLES

Table 3.1: Organ Donors by Year, Gender, and Circumstance, per Million Persons

	Circumstance: Motor Vehicle Accident			Circumstance: All Others		
	All	Male	Female	All	Male	Female
1994	4.05	2.71	1.34	15.87	9.50	6.37
1995	5.43	3.55	1.89	15.29	8.90	6.39
1996	5.16	3.44	1.73	15.52	8.88	6.64
1997	5.25	3.39	1.87	15.45	8.84	6.60
1998	5.19	3.40	1.80	16.36	9.11	7.25
1999	4.94	3.24	1.70	16.41	9.02	7.39
2000	5.24	3.52	1.72	16.43	9.10	7.33
2001	4.92	3.37	1.55	16.84	9.41	7.43
2002	5.20	3.55	1.65	16.71	9.54	7.17
2003	5.03	3.33	1.70	17.63	9.97	7.66
2004	5.30	3.54	1.77	19.43	10.68	8.75
2005	5.24	3.50	1.73	20.69	11.57	9.12
2006	5.64	3.86	1.78	21.68	12.41	9.26
2007	5.36	3.74	1.61	21.85	12.68	9.15

Note:
OPTN began reporting circumstances of death on April 1, 1994. For the first three months of 1994, all donors are included in the “All Others” category. Source: OPTN.

Table 3.2: Motor Vehicle Fatalities by Gender, per Million Persons

	All MVA			All Motorcycle Fatalities			All Other Vehicles		
Year	All	Male	Female	All	Male	Female	All	Male	Female
1994	156.3	105.3	51.1	8.9	8.1	0.8	147.4	97.2	50.3
1995	159.0	107.0	52.0	8.5	7.7	0.8	150.5	99.3	51.2
1996	158.5	105.7	52.7	8.1	7.4	0.7	150.3	98.3	52.0
1997	156.8	103.9	52.9	7.9	7.2	0.7	148.9	96.7	52.2
1998	153.4	102.1	51.3	8.5	7.7	0.8	144.9	94.4	50.6
1999	152.8	102.8	50.1	9.1	8.3	0.8	143.7	94.5	49.2
2000	148.4	101.0	47.4	10.2	9.3	1.0	138.1	91.7	46.4
2001	146.5	100.6	45.9	11.1	10.1	1.0	135.4	90.5	44.9
2002	148.9	102.1	46.9	11.3	10.3	1.1	137.6	91.8	45.8
2003	147.1	100.7	46.4	12.7	11.5	1.3	134.4	89.2	45.2
2004	145.8	100.2	45.6	13.7	12.2	1.5	132.1	88.0	44.1
2005	146.8	102.4	44.4	15.4	13.9	1.5	131.3	88.4	42.9
2006	140.7	98.5	42.2	15.9	14.6	1.4	124.8	84.0	40.8
2007	135.3	95.7	39.6	17.0	15.4	1.5	118.3	80.2	38.0

Note:
Authors' calculations using Fatality Analysis Reporting System (FARS) data.

Table 3.3: Estimates of the Effect of Helmet Laws on Organ Donations

	Organ Donors Measured in:					
				Natural	Sample	Sample
	Per Capita		*Levels*	*Logs*	Mean	Mean
Donor Circumstance	(1)	(2)	(3)	(4)	(per Capita)	(Levels)
Motor Vehicle Accident	-0.491	-0.543	-7.696	-0.097	5.148	60.595
	(0.157)	(0.137)	(1.975)	(0.031)		
All Others	0.843	0.046	6.215	0.047	17.668	227.051
	(1.283)	(1.005)	(10.545)	(0.043)		
Accidents Not Involving	-0.039	-0.234	-2.090	-0.106	1.931	23.978
Vehicles	(0.233)	(0.215)	(2.410)	(0.116)		
Homicide, Suicide, or	0.421	0.363	5.783	0.094	3.390	43.516
Child Abuse	(0.267)	(0.251)	(3.873)	(0.058)		
Natural Causes	-0.020	-0.791	-9.762	0.508	4.086	49.795
	(1.348)	(1.295)	(11.534)	(0.634)		
None of the Above	0.466	0.546	13.944	0.258	7.922	105.703
	(2.229)	(2.160)	(33.168)	(0.361)		
Include State-Year Controls?	No	Yes	Yes	Yes		

Notes:

1) All estimation samples consist of 38 states from 1994 to 2007. The unit of observation is a state-year. All models include indicators for years and states.

2) Models in column (2) add controls for the state maximum speed limit, quadratics in the total population of the state, the age distribution of the state's population, heating degree days, annual precipitation, and indicators for whether the state has a donor registry, whether the state allows online donor registration, whether organs can be donated without consent of family members of the prospective donor, and whether the state had primary enforcement of seat belt laws

3) Standard errors, in parentheses, are robust to clustering within state over time.

Table 3.4: Estimates of the Effect of Helmet Laws on per Capita Motorcycle Fatalities and Organ Donations, by Gender and Age

Panel A: The Effect of Helmet Laws on Fatalities and MVA Donations

	Males		Females		Pooled	
	Motorcycle Fatalities (1)	MVA Organ Donors (2)	Motorcycle Fatalities (3)	MVA Organ Donors (4)	Motorcycle Fatalities (5)	MVA Organ Donors (6)
Overall	-3.962	-0.558	-0.395	0.015	-4.369	-0.543
	(0.518)	(0.109)	(0.095)	(0.061)	(0.568)	(0.137)
	[10.218]	[3.445]	[1.039]	[1.702]	[11.258]	[5.148]
Age Categories:						
<18	-0.103	0.047	-0.015	0.092	-0.118	0.139
	(0.045)	(0.055)	(0.016)	(0.072)	(0.047)	(0.101)
	[0.237]	[0.884]	[0.039]	[0.601]	[0.276]	[1.486]
18-34	-2.018	-0.526	-0.149	-0.041	-2.166	-0.568
	(0.305)	(0.122)	(0.050)	(0.049)	(0.323)	(0.136)
	[4.759]	[1.716]	[0.352]	[0.679]	[5.110]	[2.395]
35-49	-0.987	-0.017	-0.236	-0.022	-1.222	-0.039
	(0.170)	(0.098)	(0.049)	(0.049)	(0.181)	(0.138)
	[3.297]	[0.592]	[0.454]	[0.286]	[3.751]	[0.878]
50-64	-0.709	-0.069	-0.018	-0.008	-0.730	-0.077
	(0.150)	(0.027)	(0.037)	(0.030)	(0.163)	(0.052)
	[1.615]	[0.219]	[0.172]	[0.107]	[1.787]	[0.326]
65+	-0.146	0.008	0.012	-0.007	-0.134	0.001
	(0.061)	(0.010)	(0.016)	(0.006)	(0.064)	(0.011)
	[0.311]	[0.034]	[0.022]	[0.028]	[0.333]	[0.063]

Table 3.4 (cont'd)

Panel B: The Effect of Motorcyclist Deaths on MVA Donation Rates

	Males	Females	Pooled
Wald IV Estimates:	0.141	-0.037	0.124
	(0.045)	(0.256)	(0.039)

Notes:

1) All estimation samples consist of 38 states from 1994 to 2007. The unit of observation is a state-year. All models include indicators for years and states.

2) All models include the state-year controls described in the notes to Table 3.3

3) Standard errors, in parentheses, are robust to clustering within state over time.

4) Sample means for relevant dependent variables are listed in brackets

Table 3.5: Estimates of the Effects of Helmet Laws on per Capita MVA, NMVA, and Differenced Organ Donations, by Gender and Age

	Males			Females			Pooled		
	MVAs	NMVAs	Difference	MVAs	NMVAs	Difference	MVAs	NMVAs	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Overall	-0.558	-0.130	-0.428	0.015	-0.104	0.119	-0.543	-0.234	-0.309
	(0.109)	(0.119)	(0.113)	(0.061)	(0.109)	(0.105)	(0.137)	(0.215)	(0.193)
Age Categories:									
<18	0.047	0.011	.0036	0.092	0.015	0.077	0.139	0.026	0.112
	(0.055)	(0.067)	(0.087)	(0.072)	(0.031)	(0.096)	(0.101)	(0.086)	(0.162)
18-34	-0.526	-0.018	-0.508	-0.041	0.002	-0.044	-0.568	-0.016	-0.552
	(0.112)	(0.031)	(0.096)	(0.049)	(0.025)	(0.051)	(0.136)	(0.045)	(0.108)
35-49	-0.017	-0.060	0.043	-0.022	-0.094	0.072	-0.039	-0.154	0.115
	(0.098)	(0.051)	(0.128)	(0.049)	(0.036)	(0.060)	(0.138)	(0.074)	(0.165)
50-64	-0.069	-0.019	-0.050	-0.008	-0.020	0.012	-0.077	-0.039	-0.038
	(0.027)	(0.039)	(0.045)	(0.030)	(0.032)	(0.057)	(0.052)	(0.044)	(0.070)
65+	0.008	-0.043	0.051	-0.007	-0.008	0.001	0.001	-0.051	0.052
	(0.010)	(0.033)	(0.036)	(0.006)	(0.019)	(0.017)	(0.011)	(0.051)	(0.050)

Notes:

1) All estimation samples consist of 38 states from 1994 to 2007. The unit of observation is a state-year.

2) All models include the state-year controls described in the notes to Table 3.3

3) Standard errors, in parentheses, are robust to clustering within state over time.

Table 3.6: Changes in State Helmet Laws, 1994-2007

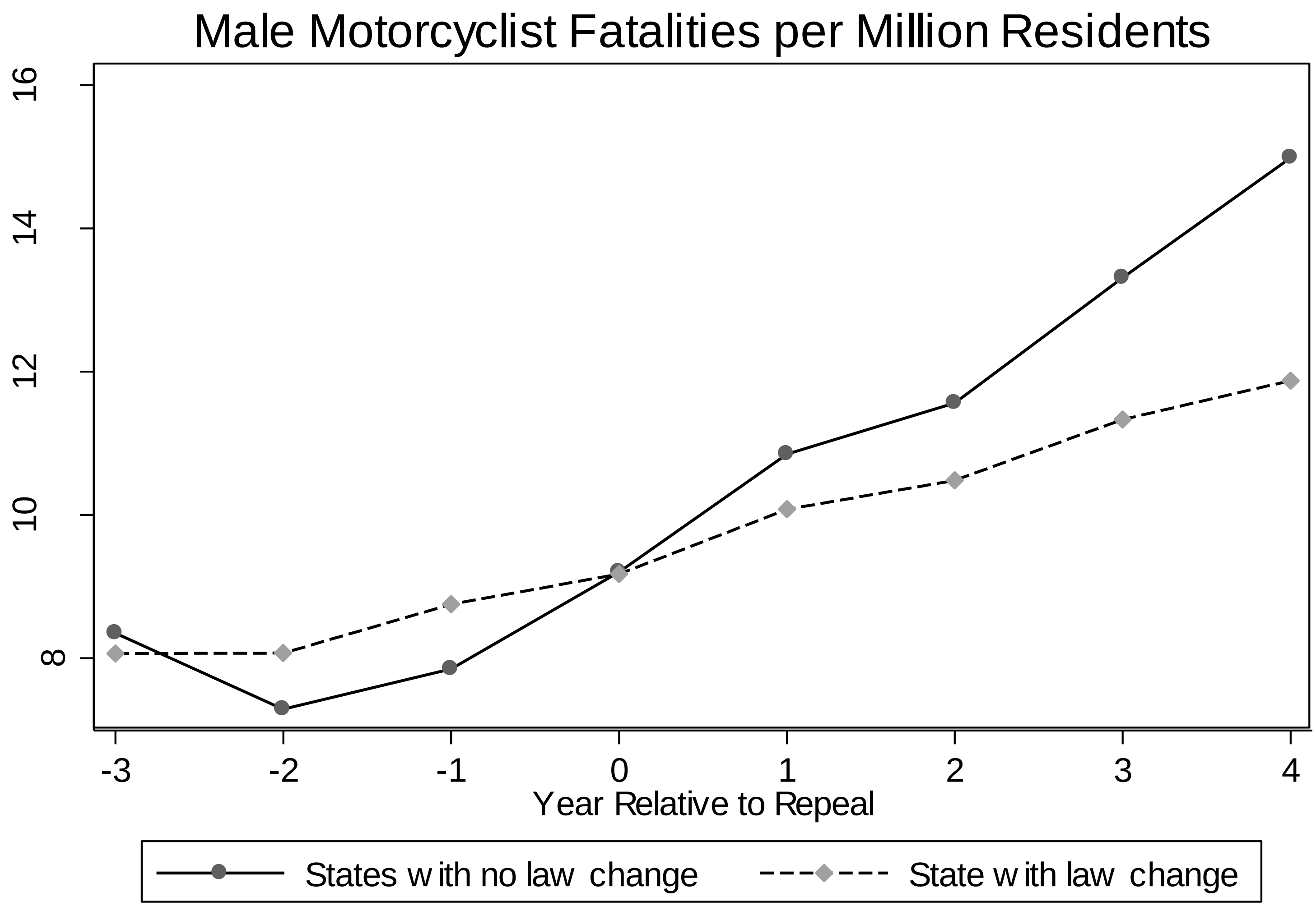
Year	None to Partial	Universal to Partial	Partial to Universal	Partial to None
1994				
1995				NH (9)
1996				
1997		AR (8), TX (9)		
1998		KY (7)		
1999		LA (8)		
2000		FL (7)		
2001				
2002				
2003		PA (9)		
2004			LA (8)	
2005				
2006				
2007	CO (7)			

Note:
The month a law changed is listed in parentheses, where "1" denotes January, "2" denotes February, and so on. Source: Insurance Institute for Highway Safety: http://www.iihs.org/laws/default.aspx

APPENDIX 3.2

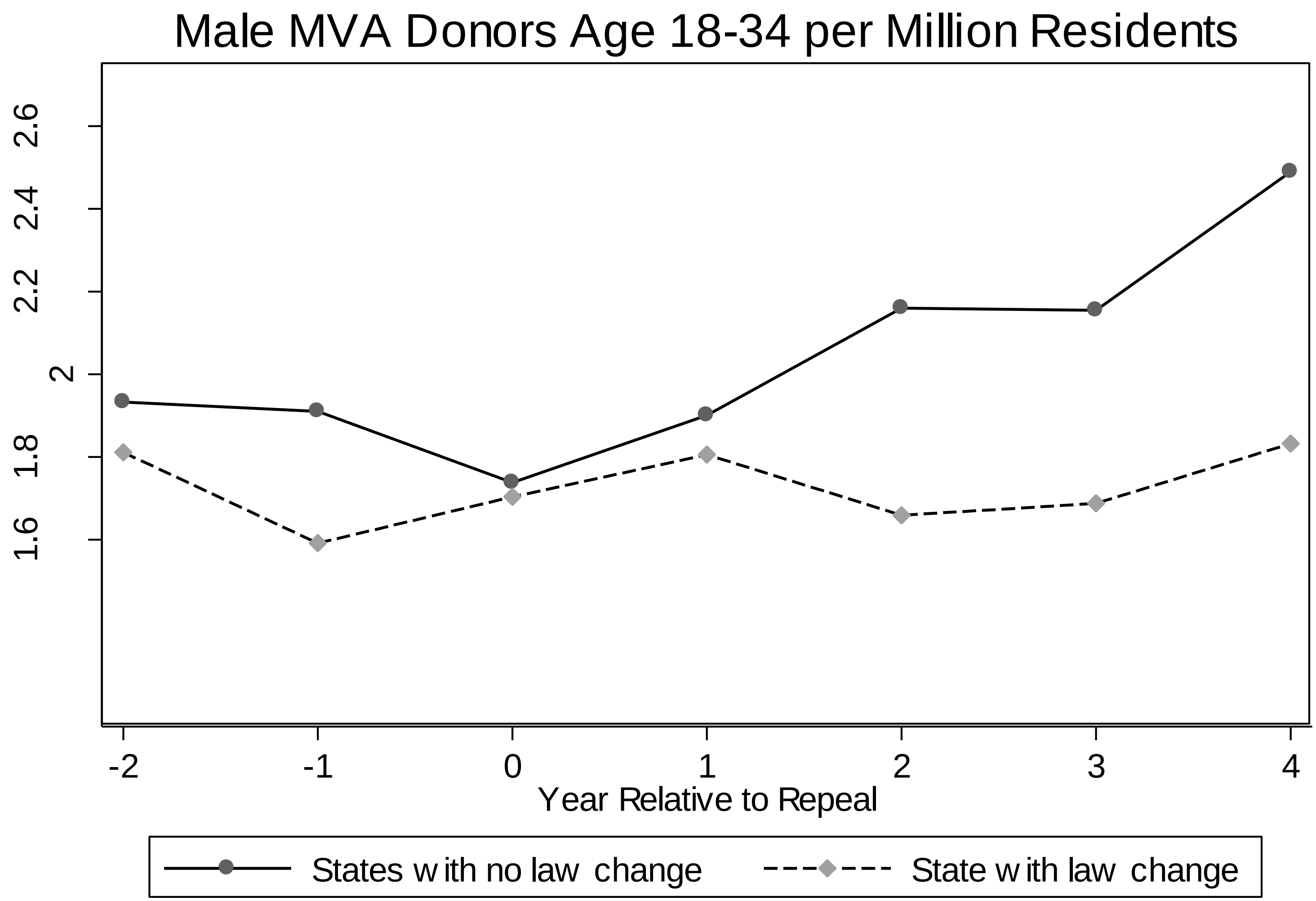
CHAPTER 3 FIGURES

Figure 3.1



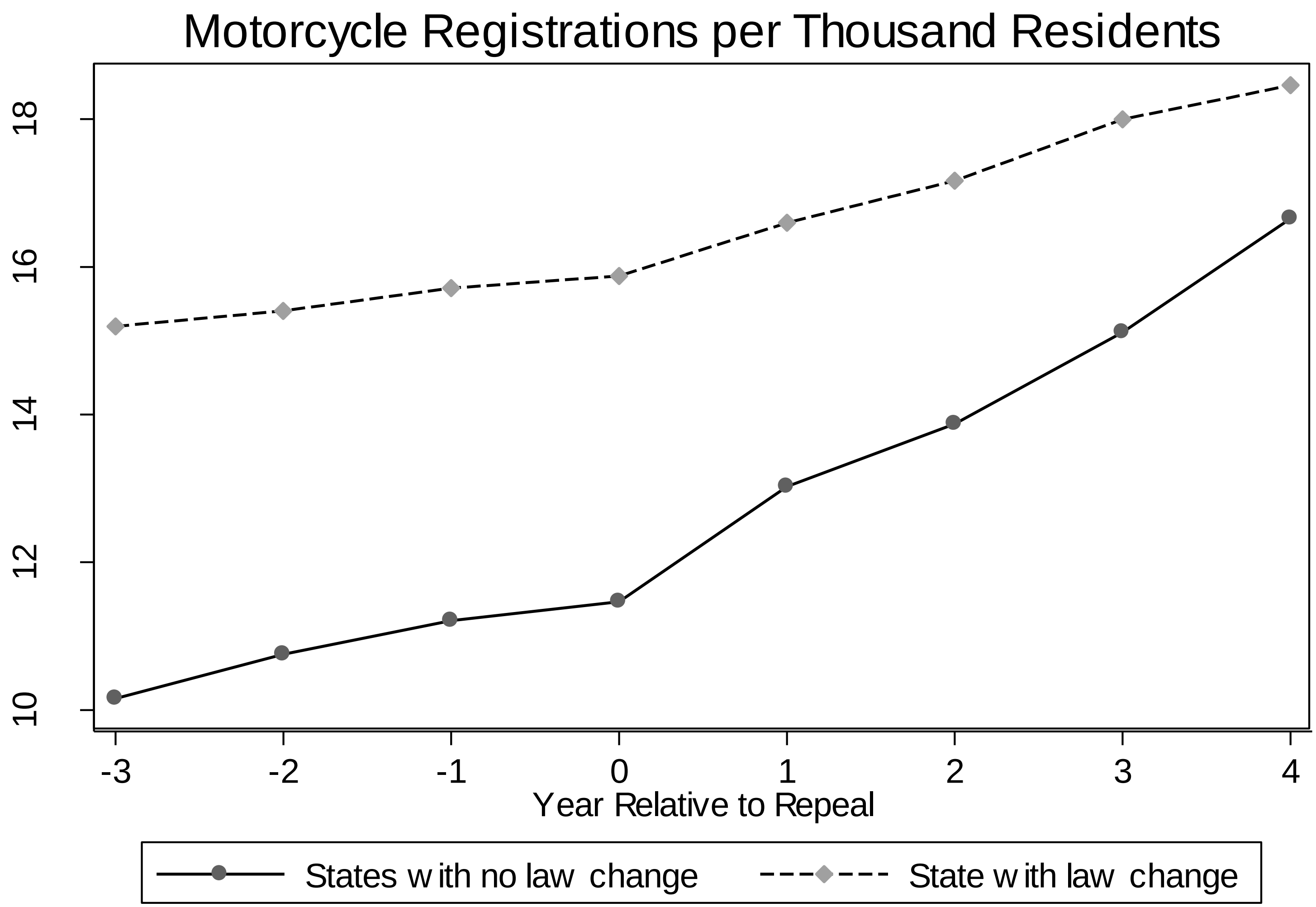
Note: Authors' calculations from FARS, OPTN and Department of Transportation data, 1994-2007.

Figure 3.2



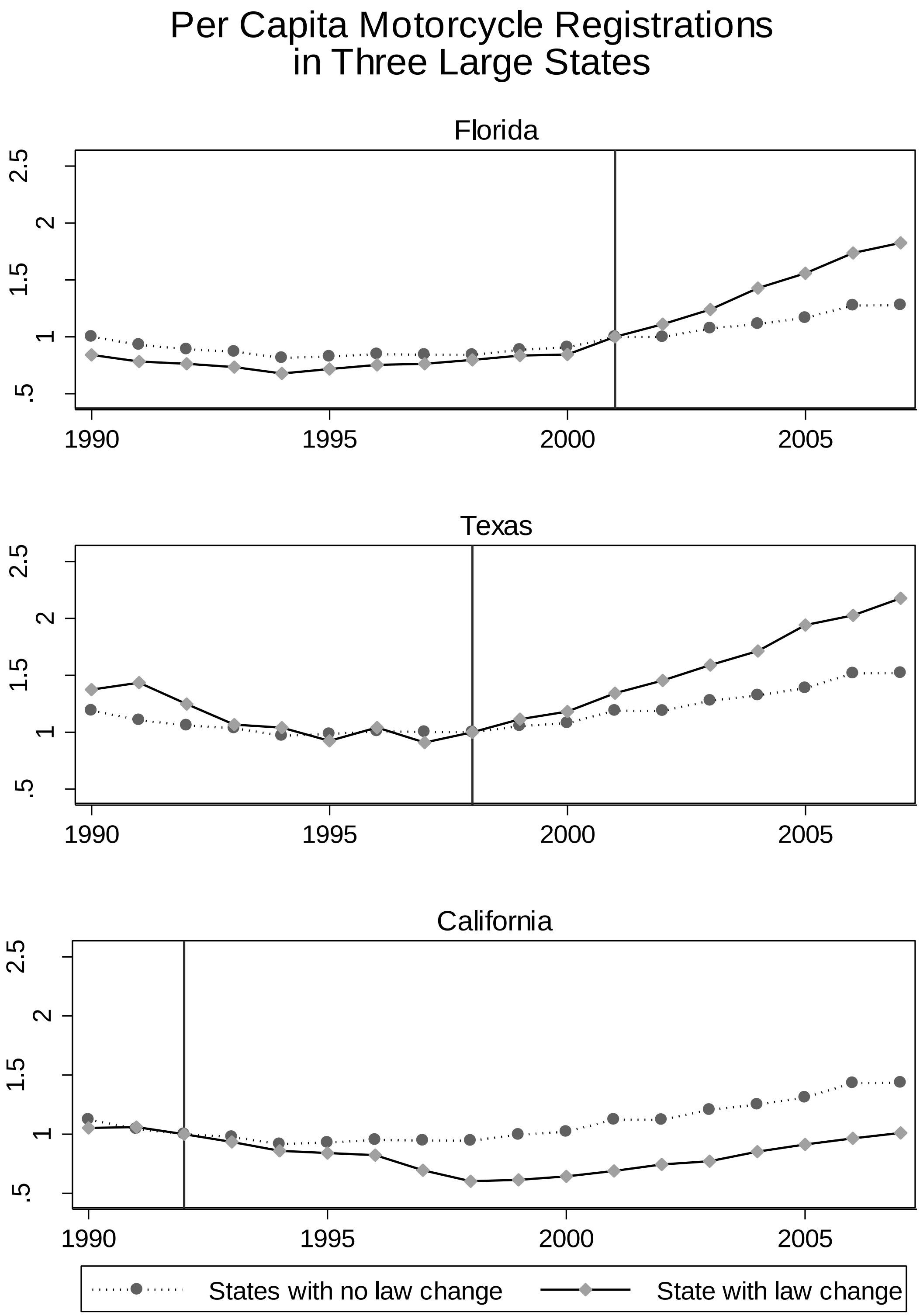
Note: Authors' calculations from FARS, OPTN and Department of Transportation data, 1994-2007.

Figure 3.3



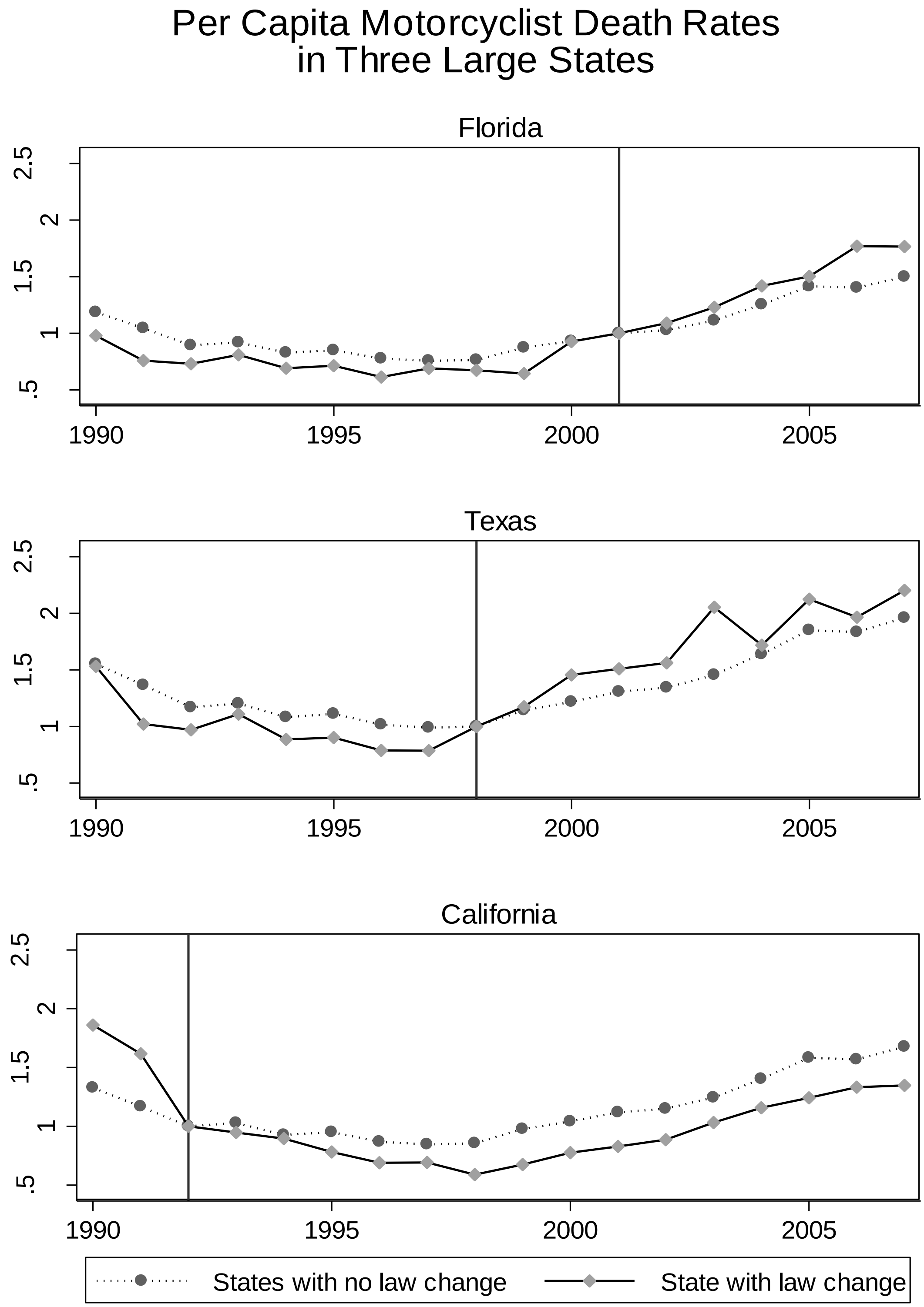
Note: Authors' calculations from FARS, OPTN and Department of Transportation data, 1994-2007.

Figure 3.4



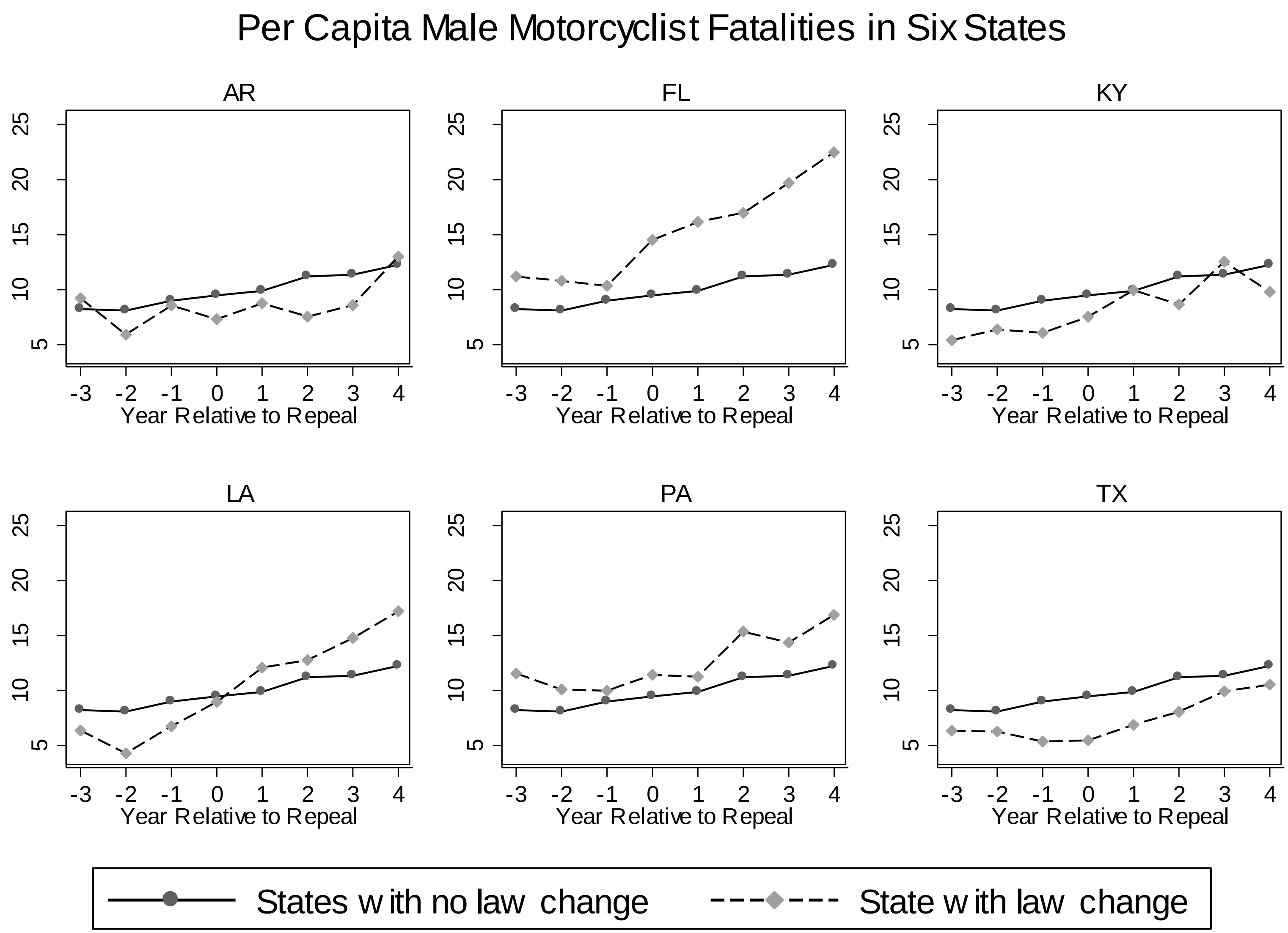
Note: Authors' calculations from FARS and Department of Transportation data. Data are normed to 1 in the first full year following a law change.

Figure 3.5



Note: Authors' calculations from FARS and Department of Transportation data. Data are normed to 1 in the first full year following a law change.

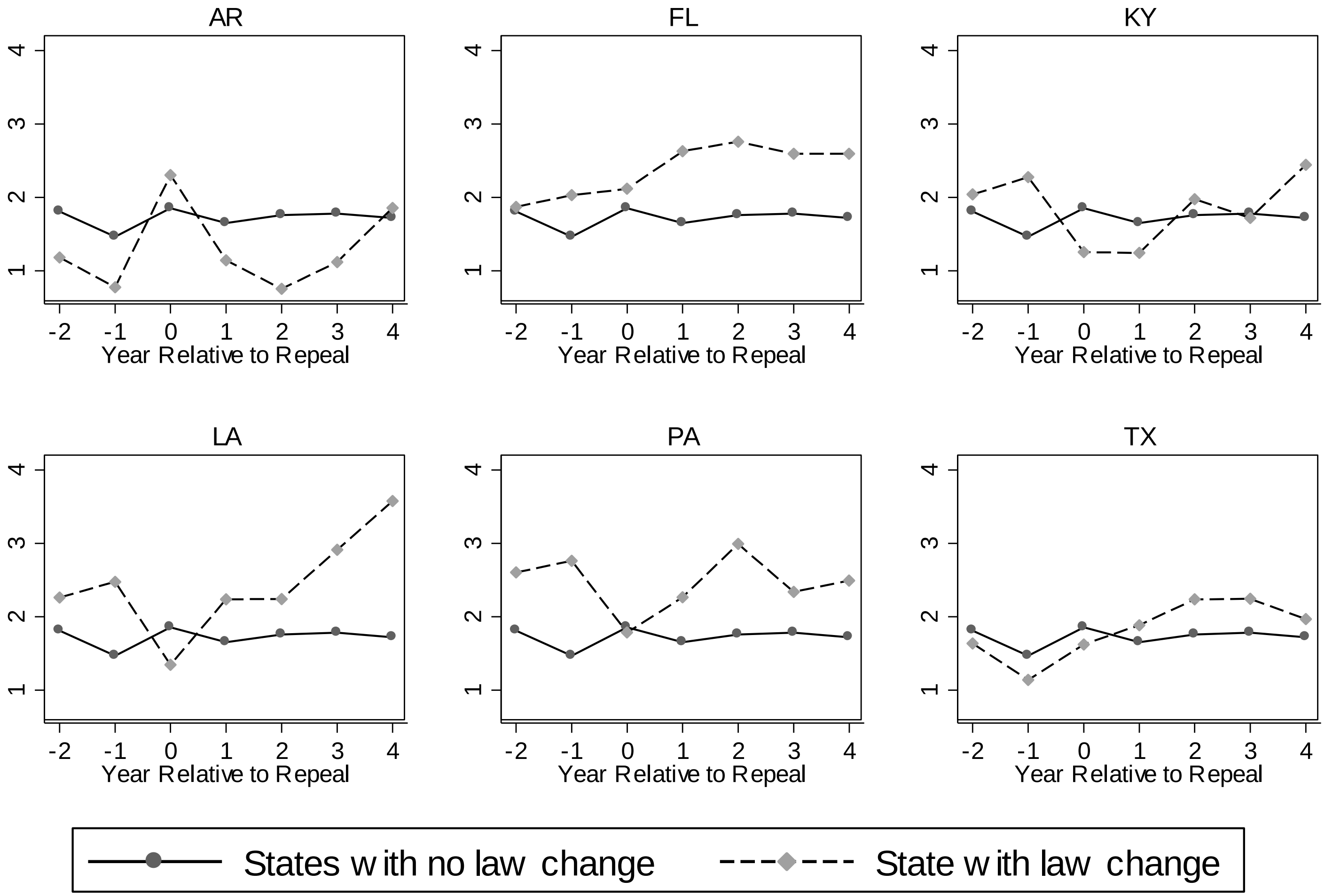
Figure 3.6



Note: Authors' calculations from FARS data, 1994-2007.

Figure 3.7

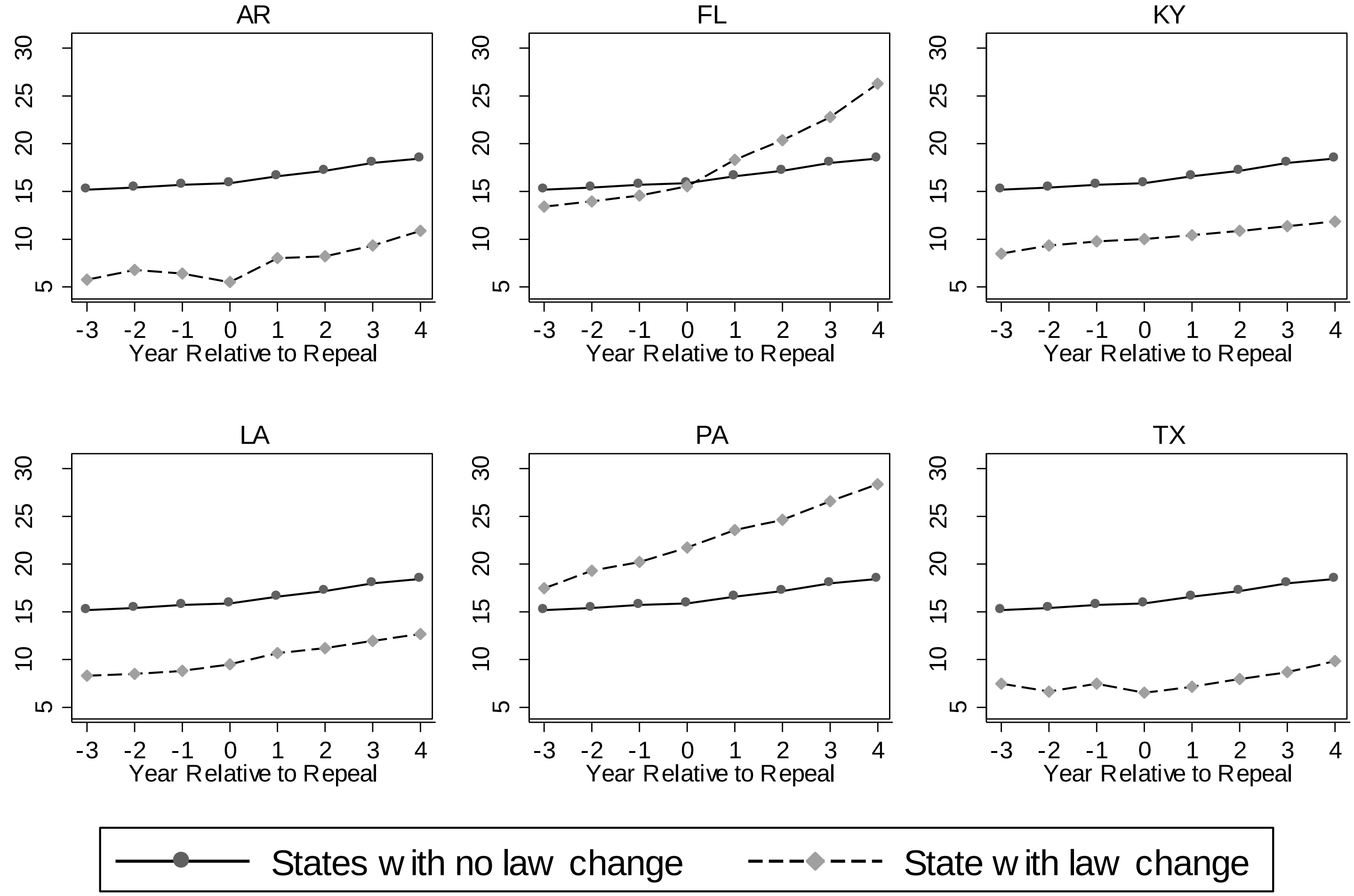
Per Capita Male MVA Organ Donors Aged 18-34 in Six States



Note: Authors' calculations from OPTN data, 1994-2007.

Figure 3.8

Per Capita Motorcycle Registrations in Six States



Note: Authors' calculations from Department of Transportation data, 1994-2007.

CHAPTER 3 REFERENCES

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