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Essays in Labor Economics and Political Economy

DISSERTATION

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in Economics

by

Daniel Joshua Ganz

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2022

DEDICATION

To

Mom, Dad, Joey, Jonathan, and David

for what you taught me and for what you sacrificed for me.

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ABSTRACT OF THE DISSERTATION

Essays in Labor Economics and Political Economy

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In this project I investigate three sets of policy changes related to overtime penalties, school bond measures, and police training. I study the effects that these policy changes have had on labor market and other outcomes. In the first chapter, I examine how the demand for labor and the categorization of labor change in response to a change in penalties for overtime violations. I propose a model for determining labor demand for fully compensated overtime hours, undercompensated overtime hours, and total hours when employers rationally undercompensate overtime labor. I find evidence that, following a decrease in the expected penalty for state overtime violations in Massachusetts, the average number of undercompensated and fully compensated overtime hours worked increased, while the average number of non-overtime hours worked decreased. There is little evidence that employers responded to the change in penalties by reclassifying workers as exempt or nonexempt from the overtime law.

In the second chapter I use multiple methods to examine how school bond measures, and the accountability measures associated with the bond measures, affect home prices and teacher labor market outcomes. I find evidence that school bonds passed with a 55% passage threshold

are correlated with increases in home prices and average teacher salaries. The findings are reversed for bonds passed with a two-thirds passage threshold, which are correlated with reductions in home prices and teacher salaries. The findings suggest that the accountability measures for school bond measures do influence the way the bonds are used, and have downstream effects on home prices and teacher labor market outcomes.

In the final chapter, I evaluate how different types of police training may affect the conduct of police officers. I find that increased Peace Officer Standards and Training (POST) funding leads to a small temporary decrease in civilian complaints sustained. Increased POST funding is also correlated with decreases in police shootings in each of the following three years, though these results are statistically indistinguishable from zero. Differences between how POST and non-POST training programs cover various topics—such as those related to mental health, professionalism, and rights—may explain these results. Policymakers may look to investing in increased police training funding from the state to improve policing outcomes.

Chapter 1

Reducing the Cost of Overtime Violations Leads to More Overtime Hours Worked: Theory and Evidence from Massachusetts

1.1 Introduction

For several decades, economists have studied whether wage-and-hour laws affect the supply and demand of labor. Recent research in this topic includes Meer & West (2016) (minimum wage and employment), Dube et al. (2016) (minimum wage and employment), and Martins (2016) (overtime and employment). Additionally, since at least the eighteenth century, political theorists have examined the role penalties play in improving compliance with the law.[1] In the economics context, prior work studying the relationship between penalties and compliance has focused on tax laws (Graetz & Wilde 1985), antitrust laws (Beckenstein & Gabel 1986; Snyder 1990), and employment laws (Landes 1968; Chang & Ehrlich 1985). Central to the

[1] *See, e.g.*, CESARE BECCARIA, DEI DELITTI E DELLE PENE (ON CRIMES AND PUNISHMENT) (1764); JEREMY BENTHAM, AN INTRODUCTION TO THE PRINCIPLES OF MORALS AND LEGISLATION (1780).

theoretical foundation of this work is the principle that when the monetary sanction for violating the law increases, a rational agent will be increasingly discouraged from violating the law.

In recent decades there has been a renewed interest in studying overtime laws and how they affect labor market outcomes. Hamermesh & Trejo (2000) found that requiring time-and-a-half pay for work beyond eight hours in a given day caused overtime hours and the incidence of overtime workdays to decline substantially for affected workers. Their finding supports the theoretical prediction that raising the cost of overtime will reduce demand for overtime hours, although their research does not address whether employers substitute towards other kinds of labor in response to the change. Kuroda & Yamamoto (2012) leveraged ambiguity in overtime exemption rules in Japan and found that exempt employees worked longer hours than nonexempt employees, though the wages between these two groups were statistically indistinguishable. And researchers found that workers in Japan and South Korea worked fewer hours following a decrease in the standard weekly hours of work in those countries (Lee et al. 2012), which again supports the conclusion that employers reduce demand for labor as the cost of labor increases. The existing literature has primarily focused on extensions of coverage or changes in the requirements of overtime laws, and not changes in the penalty for violating the law. Thus, to date there is little evidence on whether penalties for overtime violations affect demand for overtime or non-overtime hours and the likelihood of receiving overtime pay.

This paper examines whether an increase in penalties for overtime violations causes employers to become more likely to pay overtime wages for overtime hours, or whether they demand fewer overtime hours and more non-overtime (regular) hours. To answer this question, I rely on a change in the amount of damages Massachusetts workers could expect to recover when they experienced an overtime violation. In July of 2000, a decision by the Supreme Judicial

Court of Massachusetts caused the expected penalty for violating the Massachusetts overtime law to decrease. I assess how this change affected the number of overtime hours worked—including the number of fully compensated overtime hours worked (overtime hours compensated at the overtime wage) and the number of undercompensated overtime hours worked (overtime hours compensated at the straight wage)—as well as the number of non-overtime hours worked.

I rely on Current Population Survey (CPS)[2] data on hours worked per week and payment of overtime, tips, and commission to identify individuals who work overtime but are not paid overtime. I assess whether the damages available for overtime violations affects (1) undercompensated overtime hours worked (where the overtime wage was not paid), (2) fully compensated overtime hours worked (where the overtime wage was paid), and (3) non-overtime hours worked. This provides evidence on whether employers respond to increased damages for overtime violations by paying the required overtime wage premium, or by cutting overtime hours worked. I also assess whether the damages available for these violations affects the number of workers in occupations that are exempt from the law.

Following the decrease in the expected penalty for violating the Massachusetts overtime law, the average number of undercompensated and fully compensated overtime hours worked increased. The average number of non-overtime hours worked decreased, but this decrease is never statistically distinguishable from zero. The estimated effect for undercompensated overtime hours worked is consistent with theoretical predictions that, after undercompensated overtime hours become less costly, employers substitute towards them and away from non-

[2] CPS data is provided from the Integrated Public Use Microdata Series. Sarah Flood, Miriam King, Renae Rodgers, Steven Ruggles and J. Robert Warren. *Integrated Public Use Microdata Series, Current Population Survey: Version 6.0* [dataset]. Minneapolis, MN: IPUMS, 2018. https://doi.org/10.18128/D030.V6.0.

overtime hours worked.[3] I do not find evidence that the penalties change prompted employers to reclassify workers as being exempt or nonexempt from the overtime law.

This paper makes a contribution to the literature by formalizing a model for labor and capital where labor is separated into three categories: non-overtime, fully compensated overtime, and undercompensated overtime. This is an extension of previous models, which typically have not modeled the employer's choice to undercompensate overtime hours. A second feature of the model is that the average expected penalty for overtime violations is increasing in the amount of undercompensated overtime hours worked. The model explains how employers decide how many overtime hours to demand and how many of those hours to undercompensate. The model also provides predictions about how the demand for different categories of labor change when the cost of one type of labor changes.

Evidence from Massachusetts provides some support for the model's predictions that a decrease in the penalty for unpaid overtime wages will increase the average number of undercompensated overtime hours worked. Finally, the empirical results have important implications for policymakers, who may wish to deter violations of overtime laws, but who may want to avoid a cut in overtime hours worked, which is likely to harm at least some workers. In particular, if policymakers are concerned about spreading hours more evenly among workers, a rise in the penalty for overtime violations can help achieve this goal. In contrast, if policymakers want to give employers and employees more flexibility in setting hours, they may wish to avoid increasing the penalty for overtime violations.

[3] This result may be caused by employers scheduling more productive workers to work more overtime hours, and scheduling less productive workers to work fewer hours. In this case, undercompensated overtime hours worked would increase and non-overtime hours worked would decrease. Total hours worked would also likely decrease as the more productive workers use fewer hours to complete tasks than the less productive workers.

The remainder of this paper is organized as follows. Section II provides details on overtime laws generally, focusing on the federal overtime law (the FLSA) and changes to the penalty for violating the Massachusetts overtime law. Section III discusses theoretical predictions about how overtime laws affect demand for overtime and non-overtime labor, and it introduces and analyzes a theoretical model of labor demand. Section IV discusses the data and the methodology used to assess how penalties for overtime violations affect the demand for labor and the classification of labor as being either exempt or nonexempt from overtime laws. Section V provides results and analysis, and Section VI concludes.

1.2 Background

In July of 2000, the Supreme Judicial Court of Massachusetts reduced the expected penalty for state overtime violations by ruling that enhanced damages for overtime violations are only available for intentional or willful violations of the state's overtime law.[4] Previously, Massachusetts law allowed employees to recover treble (triple) damages for overtime violations, with no indication that the violation had to be intentional or willful for enhanced damages to apply.[5] By limiting the recovery of employees from three times the amount of the underpayment to simply the amount of the underpayment (unless the employee could show that the failure to pay the overtime wage was intentional), this change reduced the expected penalty for overtime violations. I use this reduction in the expected penalty to examine the extent to which a decrease in the cost of undercompensated overtime hours affects demand for undercompensated overtime hours, fully compensated overtime hours, and non-overtime hours.

[4] *Goodrow v. Lane Bryant, Inc.*, 732 N.E.2d 289, 299 (Mass. 2000).
[5] M.G.L.A. 151 § 1B (2018).

Overtime laws require employers to pay a wage premium for every hour worked above a certain number of hours. The federal law that provides for overtime compensation is the Fair Labor Standards Act (FLSA), which provides that every hour worked above forty hours in a week must be compensated at 150% of the regular (straight) wage. Any employee who is covered by and nonexempt from the overtime law is entitled to this increased wage (the overtime wage) for all hours worked at one employer above forty hours in a week. If an employer fails to pay the overtime wage for overtime hours, the employee can file a claim against the employer and, if successful, recover damages equal to double the unpaid wages.[6]

Violations of overtime laws are common. Based on CPS data, among employees who are covered and nonexempt[7] from the overtime provisions of the FLSA, hold a single job, and work more than forty hours per week, only 29.1% report being paid overtime pay, tips, or commissions[8] in 2020.[9] Employees who are nonexempt from the overtime provisions of the FLSA are 84% more likely to report being paid overtime pay, tips, or commissions than exempt individuals (conditional on holding a single job and working more than forty hours in a week). From 1996 to 2020, the percentage of such nonexempt employees who work over forty hours in a week who reported receiving overtime pay, tips, or commission ranged from 26.2% to 35.5%.

[6] 29 U.S.C. § 216 (2018). The FLSA allowed for double recovery of damages since the overtime law was passed in 1938. Tidewater Optical Co. v. Wittkamp, 19 S.E.2d 897, 901 (Va. 1942).

[7] For people who are not covered by the FLSA or who are exempt from the overtime provisions of the FLSA, employers are not required to pay the overtime wages for hours worked in excess of forty hours in a week. In the remainder of the paper, unless otherwise indicated, I use the phrase "nonexempt" to refer to individuals who are covered by the FLSA and nonexempt from the overtime provisions of the FLSA. I use the phrase "exempt" to refer to individuals who are either not covered by the FLSA or who are exempt from the FLSA's overtime provisions. I will also use the same terminology in discussing Massachusetts' overtime law.

[8] The CPS data includes whether the respondent is paid overtime, tips, or commissions. However, it does not separate people who are paid overtime from people who are paid tips or commissions. Thus, there are likely individuals who answered this question in the affirmative but who were in fact not paid overtime. Therefore, the percentage cited above is likely to understate the actual incidence of overtime violations.

[9] Among the subset of this population that works more than forty-five hours per week, 28.4% report being paid overtime, tips, or commissions.

As Figure 1.1 illustrates, this percentage has decreased since 1996, but over the past decade the percentage has been steadily increasing.

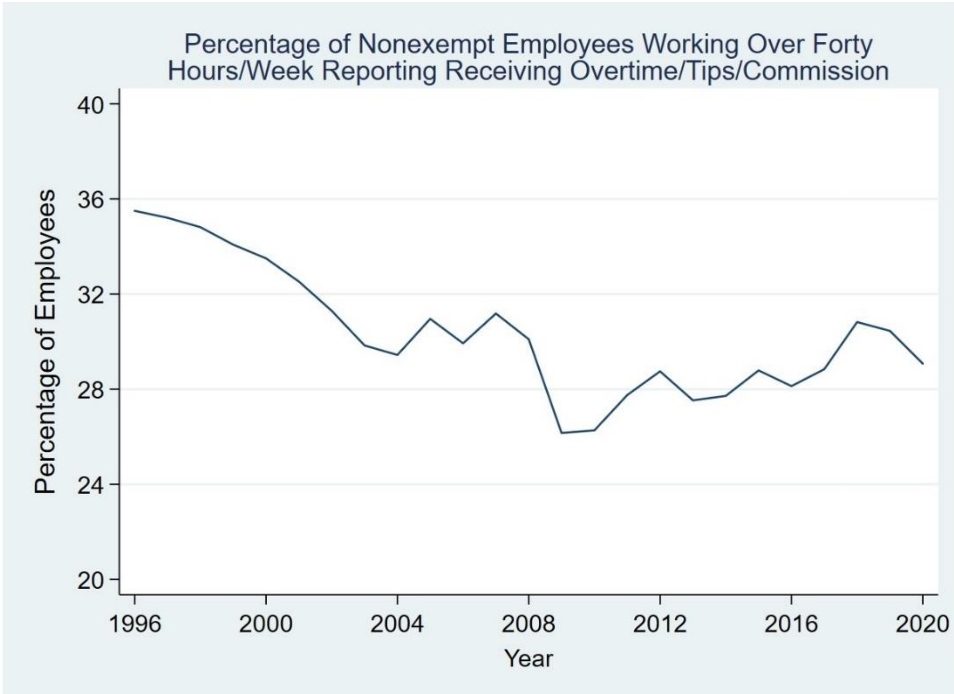

Figure 1.1: Probability of Reporting Receiving Overtime/Tips/Commission by Year

In addition to the federal law, most states have an overtime law, and these laws operate in a similar way as the FLSA: standard hours are typically forty hours per week,[10] and the overtime wage is 150% of the straight wage.[11] When state law differs from the federal law, it typically differs in one or both of the following aspects: (1) the law's coverage and exemptions; and (2) the penalties or damages for violating the law. As to the second point, some state laws allow only for single recovery of damages, some allow for double recovery, and some allow for treble recovery. Finally, since 2015, the District of Columbia has allowed for quadruple recovery of

[10] Notable exceptions here are Kansas (forty-six hours per week) and Minnesota (forty-eight hours per week). Additionally, many states also have standard daily hours or standard weekly workdays: these include California (eight hours per day and six days per week) and Colorado (twelve hours in a workday). For any hours worked beyond the standard hours, the overtime wage must be paid.

[11] The sole exception here is California, where the overtime wage is ordinarily 150% of the straight wage; however, California requires payment of 200% of the straight wage for hours worked above twelve hours in a workday, as well as for hours worked above eight hours on the seventh consecutive workday in a workweek.

unpaid overtime wages. In the past twenty years, eight states[12] and the District of Columbia have changed the damages that are recoverable for overtime violations.

When a plaintiff is covered and nonexempt by both the federal overtime law and a state overtime law, they have a choice to bring a claim under federal or state law (but not both). In addition to the damages available under the different laws, plaintiffs may consider the procedural requirements of bringing a claim under state law versus federal law, including whether plaintiffs are required to exhaust administrative remedies before bringing a claim. Under federal law, a plaintiff is not required to exhaust administrative remedies before filing a claim against his or her employer for unpaid overtime wages.[13] However, several states—including Massachusetts—require employees to exhaust administrative remedies before filing a claim. Finally, differences in filing fees for state and federal courts may cause plaintiffs to prefer one court over the other.

Like the overtime requirements of the FLSA, Massachusetts has an overtime law that requires employers to pay time-and-a-half for any hours worked above forty hours in a week. And since 1993, Massachusetts allowed employees to recover treble damages for overtime violations,[14] as well as certain other labor law violations.[15] The text of these laws did not in any way indicate that treble damages are only available for intentional or willful violations.[16]

From 1996 to 2004, two changes were made to the Massachusetts overtime law. Until 1998, an employer that failed to pay the required overtime rate to its employee was subject to a

[12] These are Arkansas, Connecticut, Maryland, Massachusetts, Missouri, New Mexico, Rhode Island, and Vermont.
[13] *Pineda v. JTCH Apartments, LLC*, 843 F.3d 1062, 1066 (5th Cir. 2016); *see also* 29 U.S.C. § 216 (2018); *Bogacki v. Buccaneers Ltd. Partnership*, 370 F. Supp. 2d 1201, 1205 (M.D. Fla. 2005).
[14] M.G.L.A. 151 § 1B (2018).
[15] Most prominently, at the time that *Goodrow* was decided, treble damages were also available for minimum wage violations, late payment or nonpayment of wages, improper expenditures of withholdings or deductions from wages, and improper withholding of tips. M.G.L.A. 149 § 150; *see also id.* §§ 148, 150C, 152A; M.G.L.A. 151 § 19.
[16] *See* M.G.L.A. 149 § 150; M.G.L.A. 151 § 1B. An intentional or willful violation is one where the employer was aware that they were violating the overtime law. In contrast, if the employer was not aware of the overtime law or mistakenly miscategorized the worker as nonexempt, then the violation would be considered unintentional.

fine of $50 to $200 or imprisonment for ten to ninety days (or both a fine and imprisonment). This is in addition to the damages that an employer would owe its employee for failure to pay the overtime rate. In August of 1998, the Massachusetts legislature changed the civil penalties for Massachusetts overtime violations. Starting on August 7, such employers were subject to a civil citation or order as provided in section 27C of chapter 149. This section authorized a maximum fine of $25,000 for any employer that pays a worker less than the overtime rate required by Massachusetts Statutes chapter 151 section 1A. The section also allows the attorney general to instead order that the infraction be rectified or that restitution be paid to the aggrieved party. Furthermore, first offenses carry a maximum fine of $15,000, and if the employer lacked specific intent to violate the overtime law, the maximum penalty for a first offense is $7,500. Thus, the 1998 change in the law significantly increased the maximum fine for an employer that violates the Massachusetts overtime law, but it removed the threat of imprisonment.

In July of 2000, the Supreme Judicial Court of Massachusetts ruled in *Goodrow v. Lane Bryant* that treble damages in overtime cases could only be awarded in cases where there was "heightened culpability" on the part of the employer (most prominently where the employer intentionally violated the law).[17] In July of 2008, a statute superseding the court's decision in *Goodrow* came into effect; this law clarified that the award of treble damages is mandatory when violations of the overtime law occur, regardless of whether the violation was intentional.[18]

After the *Goodrow* decision but before July of 2008, plaintiffs were required to show that an overtime violation was intentional to obtain treble damages. This likely made litigation more expensive for plaintiffs, and it reduced the expected recovery for plaintiffs and the expected penalty for employers who violate the overtime law. Although the *Goodrow* decision reduced the

[17] *Goodrow v. Lane Bryant, Inc.*, 732 N.E.2d 289, 299 (Mass. 2000).
[18] M.G.L.A. 151 § 20.

amount of damages available for (unintentional) violations of the state overtime law, federal law continued to provide for double damages for violations of the federal overtime law, with no distinction between intentional or unintentional violations. It is possible that the *Goodrow* decision made plaintiffs more likely to bring claims against their employer under the federal law, rather than the state law.[19] However, federal data on enforcement of the FLSA overtime law does not indicate that Massachusetts workers were more likely to bring claims under the federal law during the period during which the *Goodrow* decision was in effect.[20]

Several states changed penalties for overtime violations in recent years, but I focus on Massachusetts for three main reasons. First, Massachusetts had treble damages for any overtime violation since 1993, but these damages were struck down for unintentional violations in 2000 and reinstated in 2008.[21] Several states changed their penalties from single damages to double damages, but federal law has provided for double recovery for overtime violations since 1938. It is likely that a change in state law from single damages to double damages would have a diminished effect because the FLSA already allowed for double damages. Only four states[22] and the District of Columbia ever allowed for treble damages for overtime violations.

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[19] Plaintiffs who are covered and nonexempt from both the federal and the state overtime law have a choice about whether to file a claim under state law or under federal law. Because there is substantial overlap between the coverage and exemptions of the federal overtime law and the Massachusetts overtime law, it is likely that many plaintiffs had a choice about filing under state or federal law. 29 U.S.C. §§ 207, 213 (2018); M.G.L.A. 151 § 1A.

[20] To the contrary, in the three-and-a-half years prior to *Goodrow* being superseded, there were 409 claims brought against Massachusetts employers for overtime violations, compared to 694 claims brought against Massachusetts employers for overtime violations in the three-and-a-half years after *Goodrow* was superseded (an increase of 69.7%). In comparison, in all states, there were 27,538 overtime claims brought in the three-and-a-half years prior to *Goodrow* being superseded, compared to 35,062 overtime claims brought in the three-and-a-half years after *Goodrow* was superseded (an increase of 27.3%). Thus, relative to other states, the number of overtime claims in Massachusetts appears to have increased, rather than decreased, after *Goodrow* was superseded. *See Wage and Hour Compliance Action Data*, US DEPARTMENT OF LABOR, https://enforcedata.dol.gov/views/data_summary.php (last updated July 26, 2019).

[21] During this period, plaintiffs could only recover the amount of the underpayment (single recovery).

[22] These are Massachusetts, Missouri, New Mexico, and Rhode Island.

Second, the July 2000 change in Massachusetts' penalties occurred due to court action, as opposed to legislative action. Legislators may change laws for a variety of reasons, including in response to changes in demand for specific categories of labor.[23] Court decisions, on the other hand, are more likely to be exogenous to these labor outcomes. The reasoning of the *Goodrow* decision further indicates that the ruling was exogenous to labor market conditions, as the majority opinion relied on precedent from a previous case known as *Fontaine v. Ebtec Corp* to conclude that mandatory treble damages are unlawful in Massachusetts.[24] The court therefore reached its conclusion based on an application of precedent, rather than an analysis of the potential labor market impact of its decision. Thus, endogeneity is less likely to be a concern in evaluating the effects of the change in overtime violation penalties in Massachusetts, compared to the changes in overtime violation penalties in the other states that changed these penalties.

Third, the change in penalties for overtime violations that occurred in Massachusetts in 2000 happened independently of changes to other labor laws. The *Goodrow* decision did not

[23] For example, a state may increase penalties for violations of overtime laws in response to an increasing number of undercompensated overtime hours being worked. If this trend continues after the law changes and we examine the number of undercompensated overtime hours worked as a function of penalties, we would see a spurious positive correlation between the strength of penalties and the number of undercompensated overtime hours worked.

[24] *Fontaine v. Ebtec Corp.*, 613 N.E.2d 881, 889 (Mass. 1993). The reasoning of this decision in turn relied on language from a US Supreme Court decision known as *Trans World Airlines, Inc. v. Thurston*, 469 U.S. 111, 125 (1985). However, a careful reading of these cases demonstrates that both of these cases are inapplicable to the question considered in *Goodrow*. In *Fontaine*, the Supreme Judicial Court of Massachusetts noted that enhanced damages are "essentially punitive in nature," and the court concluded that an enhanced damages provision of a Massachusetts anti-discrimination statute only allowed for enhanced damages for knowing or reckless violations. *Fontaine*, 613 N.E.2d at 889; *see also* M.G.L.A. 151B § 9 (2018). However, this statute explicitly stated that the enhanced damages shall be awarded "if the court finds that the act or practice complained of was committed with knowledge, or reason to know, that such act or practice violated" the relevant provisions of the law. M.G.L.A. 151B § 9. Likewise, in *Trans World Airlines*, the US Supreme Court concluded that liquidated damages in the Age Discrimination in Employment Act (ADEA) are intended to be punitive in nature and that they are only available for willful violations of the ADEA. *Trans World Airlines*, 469 U.S. at 125. However, this conclusion was also based on the text of the ADEA, which stated that liquidated damages "shall be payable only in cases of willful violations." 29 U.S.C. § 626 (2018). The court in *Goodrow* read these cases incorrectly to mean that liquidated damages are only ever available for willful violations. However, because Massachusetts' overtime law did not indicate in any way that the treble damages are only available for intentional or willful violations, the court erred in concluding that a plaintiff must show that the violation was willful to recover liquidated damages. Thus, it is most likely that the change in penalties was both unexpected and exogenous to labor outcomes.

apply to treble damages for minimum wage or other labor law violations, and in fact mandatory treble damages for these laws were not struck down until the 2005 case of *Wiedmann v. The Bradford Group*.[25] In contrast, the legislative enactment of treble damages for overtime violations has typically occurred concurrently with other changes to labor laws. In particular, in all of the other contexts in which a state enacted mandatory treble or higher damages for overtime violations from 1999 to 2018, the state also adopted mandatory treble or higher damages for minimum wage violations, which may also impact labor demand. In sum, the experience of Massachusetts in 2000 provides a context for studying how enhanced penalties for overtime violations affects the demand for overtime labor, and this context is superior to the experiences of other states that have adopted enhanced damages through the legislative process.

1.3 Theory

This section examines from a theoretical basis how demand for undercompensated overtime hours, fully compensated overtime hours, and non-overtime hours change when the penalties for violating overtime laws change. This provides insights into how employers choose labor demand in light of overtime requirements, including when the penalty for violating the overtime law changes.

An increase in damages for overtime violations will operate as an increase in the expected cost of undercompensated overtime hours for two reasons. First, if the damages increase, then an employer will face a higher penalty when an employee brings a successful claim against the employer for unpaid overtime compensation. Second, the increase in damages will likely make employees more likely to bring overtime claims against their employer, as the

[25] 831 N.E.2d 304 (Mass. 2005).

benefit for doing so has increased. Thus, an increase in damages will cause the expected penalty for each overtime claim brought against the employer to increase, and it will cause the number of overtime claims brought against the employer to increase. This represents an increase in the cost of undercompensated overtime hours, and this will cause demand for undercompensated overtime hours to decrease. Employers may substitute towards fully compensated overtime hours, or they may substitute towards non-overtime hours (for example, by hiring more workers). Employers may also substitute towards capital, or they may reduce production. Finally, the employers may make a combination of these adjustments.

To analyze how a change in penalties for overtime violations will affect demand for different categories of labor, I adapt a model for labor demand introduced by Hart (1987). In Hart's model, firms choose the number of workers (N) and average hours (h) to minimize total labor costs. Per-period total labor costs C_L are given by:

$$C_L = (Pw(h) + Z + T)N, \tag{1}$$

where $w(h)$ is the total wage-cost per worker per period for h hours of work. This is multiplied by $P \geq 1$, where $1 - P$ is a payroll tax rate (thus $Pw(h)$ is the total per-hour cost that the employer pays). Z is the employer's per-period share of specific labor investments, defined as:

$$Z = (q + r)z, \tag{2}$$

where z are hiring and training costs, q is the separation rate, and r is a discount rate reflecting the opportunity cost of capital. Finally, T are per-period payroll tax contributions that depend on the number of workers, but do not depend on the number of hours worked. Output Q depends on the production function $Q = f(L)$, where L represents labor services, $f'(L) > 0$, and $f''(L) < 0$. L is related to h and N through the expression $L = g(h)N^{1-\alpha}$, with $g(0) = 0$, $g'(h) > 0$, $g''(h) < 0$, and $0 \leq \alpha < 1$.

Firms choose the optimum combination of h and N to minimize total labor costs, which is equivalent to finding a stationary point of the expression

$$J = C_L + \lambda(L - g(h)N^{1-\alpha}), \tag{3}$$

where λ is the Lagrangian multiplier. The first-order conditions $J_h = J_N = J_\lambda = 0$ are:

$$Pw'(h)N - \lambda g'(h)N^{1-\alpha} = 0 \tag{4}$$

$$Pw(h)+Z+T-\lambda(1-\alpha)g(h)N^{-\alpha} = 0 \tag{5}$$

$$L - g(h)N^{1-\alpha} = 0 \tag{6}$$

Dividing Equation (3) by Equation (4) yields:

$$\frac{w'(h)}{g'(h)} = \frac{w(h) + \frac{Z+T}{P}}{(1-\alpha)g(h)} \tag{7}$$

Equation (7) means that firms optimize the number of workers and average hours by equating the ratios of marginal cost to marginal product for hours and workers, respectively. Finally, the wage equation in Hart's model is given by:

$$w(h) = \begin{cases} w_s h_s + a w_s(h - h_s), & for\ h > h_s \\ w_s h, & for\ h \le h_s \end{cases} \tag{8}$$

where h_s is the standard (non-overtime) hours (forty hours per week in most jurisdictions in the United States), w_s is the standard (non-overtime or straight) wage, and $a > 1$ is the premium rate that firms pay to workers for overtime hours as required by the overtime law (1.5 in most jurisdictions in the United States).

Consider now a variation of Hart's model where the wage function also depends on the number of overtime hours that the firm chooses to compensate at the overtime wage, h_c. Thus, h_s are the standard hours worked, h_c are the fully compensated overtime hours worked, and h_u are the undercompensated overtime hours worked, which is given by $h_u = h - h_s - h_c$ when $h > h_s$ (and $h_u = 0$ otherwise). The expression for wages is therefore

$$w(h, h_c) = \begin{cases} w_s h_s + a w_s h_c + \varphi(h_u) w_s h_u, & for\ h > h_s \\ w_s h, & for\ h \leq h_s \end{cases} \tag{9}$$

The expression $\varphi(h_u)$ is the firm's expected cost of employing an individual to work h_u undercompensated overtime hours. $\varphi(h_u)w_s$ includes the standard wage that is normally paid to the employee for non-overtime hours, plus the expected penalty that depends on the probability of being prosecuted and the penalty multiplier that applies to overtime violations (for example, double damages under the federal overtime law). Therefore, $\varphi(h_u) > 1$. The probability of getting prosecuted will increase as the average number of undercompensated overtime hours worked increases, and therefore $\frac{d\varphi}{dh_u} > 0$. Finally, for low values of h_u, the cost of undercompensated overtime hours is lower than the cost of fully compensated overtime hours because there is a low probability of being prosecuted for small underpayments (the cost to the worker of filing a claim exceeds the expected recovery). Thus, $\varphi(h_u) < a$ for low values of h_u.

Because an undercompensated overtime hour is as productive as a fully compensated overtime hour, firms are indifferent between undercompensated and fully compensated overtime hours except to the extent that one type of overtime hour may be more expensive than the other. Therefore, for any given number of overtime hours worked, the firm will choose h_c (or, equivalently, h_u) to minimize the wage.[26] Thus the firm will choose h_c to minimize:

$$a w_s h_c + \varphi(h - h_s - h_c) w_s (h - h_s - h_c) \tag{10}$$

This is equivalent to choosing $h_u = h_u$* to minimize:

$$a w_s (h - h_s - h_u) + \varphi(h_u) w_s h_u \tag{11}$$

By the first-order condition, in an interior solution h_u* is implicitly given by:

[26] I refer to this choice, where the firm decides (for each possible quantity of total overtime hours) how many overtime hours to fully compensate and how many to undercompensate, as the firm's "first stage" choice.

$$h_u^* = \frac{a - \varphi(h_u^*)}{\frac{d\varphi}{dh_u}} \tag{12}$$

This must be greater than zero for some values of h_u because $\varphi(h_u) < a$ for low values of h_u, and $\frac{d\varphi}{dh_u} > 0$. The firm will continue to demand only undercompensated overtime hours until $\varphi(h_u) = a$, at which point all additional overtime hours will be fully compensated. There may also be a corner solution where the firm demands no fully compensated overtime hours, and therefore h_u* is equal to $h - h_s$ (or, in the case where $h \leq h_s$, h_u* will of course be zero). The total wage paid for overtime hours will be:

$$a w_s (h - h_s - h_u^*) + \varphi(h_u^*) w_s h_u^* \tag{13}$$

And therefore the average wage paid for each overtime hour will be:

$$a w_s \frac{h - h_s - h_u^*}{h - h_s} + \varphi(h_u^*) w_s \frac{h_u^*}{h - h_s} \tag{14}$$

The wage equation is then rewritten as:

$$w(h, h_c) = \begin{cases} w_s h_s + (a w_s \frac{h - h_s - h_u^*}{h - h_s} + \varphi(h_u^*) w_s \frac{h_u^*}{h - h_s})(h - h_s), & for\ h > h_s \\ w_s h, & for\ h \leq h_s \end{cases} \tag{15}$$

The average wage paid for each overtime hour in Equation (15) therefore replaces the parameter a in Hart's original model. The firm then solves for h and N as in the original model.[27]

Suppose that $\varphi(h_u)$ increases so that at every level of h_u, the expected penalty for employing h_u undercompensated overtime hours increases. Suppose also that $\frac{d\varphi}{dh_u}$ does not change, so that the increase in the penalty is the same for every undercompensated overtime hour worked. A firm that was initially not using overtime hours will not be affected by this change. However, if a firm had initially been using overtime hours, at least some of those hours were

[27] I refer to this choice, where the firm chooses h and N, as the firm's "second stage" choice.

undercompensated overtime hours (this follows from the fact that for low values of h_u, undercompensated overtime hours are less costly than fully compensated overtime hours). Returning to Equation (12) and differentiating with respect to $\varphi(h_u)$ yields:

$$\frac{\partial h_u^*}{\partial \varphi} = -\frac{1}{\frac{d\varphi}{dh_u^*}}, \tag{16}$$

which is negative because $\frac{d\varphi}{dh_u}$ is assumed to be positive. Thus, after an increase in $\varphi(h_u)$, the number of undercompensated overtime hours worked will decrease. At the first stage when the firm is choosing how many overtime hours to fully compensate, there are two cases: the firm will substitute away from undercompensated overtime hours and towards fully compensated overtime hours (case 1), or the firm will reduce the number of undercompensated overtime hours and leave the number of fully compensated overtime hours unchanged (case 2).

The increase in the cost of undercompensated overtime hours will raise the average cost of overtime hours. This is because the firm will use fewer of the less expensive undercompensated hours and more of the more expensive fully compensated hours (case 1 above), or the firm will continue to use only undercompensated overtime hours (but use fewer of them), which have become more expensive (case 2 above). Due to the higher average cost of overtime hours, the firm will substitute away from overtime hours (resulting in a fall in h) and towards workers (resulting in a rise in N). Therefore, following an increase in penalties for overtime violations, the model predicts that average undercompensated overtime hours will decrease, average fully compensated overtime hours will increase or remain unchanged, average total hours will decrease, and the number of workers will increase.[28]

[28] The exact amount by which firms substitute undercompensated overtime hours for fully compensated overtime hours and regular hours may vary by firm, sector, occupation, and several other factors. By Equation (7), the firm will choose N and h based on $g(h)$. Different firms may have different $g(h)$ functions, and so we may see the elasticities of the different kinds of labor vary from firm to firm.

1.4 Data and Methods

To examine how increased penalties for overtime violations affects labor demand, I use data from the Current Population Survey (CPS). CPS surveys individuals throughout the United States and asks them about usual weekly hours worked, hourly wages, weekly earnings, whether the individual works multiple jobs, and whether the individual is usually paid overtime, tips, or commissions. The CPS data includes the state that the individual lives in, and the industry and occupation of the individual, which allows me to determine whether the individual is covered by the overtime law and whether the individual falls into any exemptions in the law. Using this data, I am able to examine whether increased penalties for overtime violations affects labor outcomes for nonexempt individuals, in comparison to exempt individuals.

To conduct my analysis, I categorize workers by occupation, industry, and sector. The CPS data categorizes workers into 1,220 sets of occupation codes, 532 sets of industry codes, and 10 sector codes. Because I need to create fixed effects for each combination of occupation, industry, sector, and state, the categorization provided by CPS would result in a surplus of fixed effects that would prevent the analysis from running. I therefore mapped the 1,220 sets of occupation codes to 86 more general occupations; I also mapped the 532 sets of industry codes to 22 industries, and I combined two sector codes to reduce the number of sectors from 10 to 9.

Next, I classify each combination of occupation, industry, and sector as being covered or not covered by the Massachusetts overtime law, as well as covered or not covered by the federal overtime law. This allows me to determine which workers are affected by the change in the Massachusetts overtime law, and the extent to which they were affected by the change in the law. To do this, I reviewed the exemption and coverage details of the Massachusetts and federal overtime laws, and mapped combinations of occupation, industry, and sector as being covered or not covered by each law. When the coverage status of any such combination under either of the laws could not be determined, then these combinations were classified separately.

Finally, I ensure that the analysis is not contaminated by contemporaneous changes in overtime laws elsewhere in the country. I reviewed the federal overtime law and the overtime laws of all fifty states and the District of Columbia to identify any changes to overtime laws in each state during the period of analysis (1996 to 2004). I identified 16 states (along with the District of Columbia) that changed their general overtime laws in a way that may affect the outcome variables of interest, and I dropped these states from the analysis below.

Of the remaining 34 states, I searched for changes in the overtime law from 1996 to 2004 that affected only specific occupations, industries, or sectors. For example, some states made certain occupations or industries exempt from the state overtime law during this period. In these cases, I dropped the affected occupations, industries, or sectors for these states from the analysis below. Finally, I found that certain computer systems analysts, computer programmers, software engineers, and other similarly skilled workers were made exempt from the federal overtime law in 1996, and these workers were dropped from the analysis below. Dropping these affected states and affected occupations, industries, and sectors within specific states ensures that the analysis below is not contaminated by contemporaneous changes in overtime laws.

In my main analysis, I examine how the *Goodrow* decision affected overtime and non-overtime hours worked in Massachusetts, as well as the percent of people in occupations that are exempt within each industry. To examine how the change in penalties for overtime violations affects hours worked, I use both a difference-in-differences and a triple-differences framework. In the difference-in-differences framework, I examine how the outcome variables of interest—the number of hours worked for each category of labor—changed for nonexempt workers in

Massachusetts compared to exempt workers in the state in a nine-year period surrounding the change in the law. In the triple-differences framework, state provides the third difference, and I examine how hours worked changes for nonexempt workers in Massachusetts, relative to exempt workers in Massachusetts and both exempt and nonexempt workers in other states.

In both frameworks, I separate workers into three categories: (1) those who are covered by both the Massachusetts overtime law and the federal overtime law; (2) those who are covered by the Massachusetts overtime law but not the federal overtime law; and (3) those who are not covered by the Massachusetts overtime law.[29] This allows me to examine whether the change in damages affected these three groups differently. The econometric specification for each of the models using the difference-in-differences framework is:

$$\begin{gathered} Y_{i,t} = \sum_k \alpha_k * Occupation + \sum_t \beta_t * Year_t + \\ \delta * After_t + \gamma * Non - Exempt_i \times After_t + \lambda * X_{i,t} + \epsilon_{i,t} \end{gathered} \tag{17}$$

The unit of observation in the above regressions is individual i in year t. The outcome $Y_{i,t}$ represents the dependent variable of interest, for example the number of undercompensated overtime hours worked by individual i in year t. The coefficient δ represents the effect of being in the post-*Goodrow* period (after July of 2000). The coefficient γ represents the difference-in-differences (DD) estimator for the effect of decreased overtime penalties on hours worked for non-exempt workers after the *Goodrow* decision. The specification includes fixed effects for each combination of occupation, industry, and sector—which are denoted in Equations (17) and

[29] For coverage and exemption details for the Massachusetts law, see M.G.L.A. 151 § 1A. For coverage and exemption details for the federal overtime law, see 29 U.S.C. § 207 and 29 U.S.C. § 213. Because coverage and exemption is based almost entirely on the worker's occupation, industry, and sector, I am able to use occupation and industry information from CPS to identify whether workers belong to an occupation, industry, and sector that is covered and nonexempt from the Massachusetts and federal overtime laws.

(18) as simply Occupation—as well as fixed effects for each year. The variable $X_{i,t}$ is a vector of controls such as gender, race, and ethnicity, and $\varepsilon_{i,t}$ is the error term.

In the triple-differences framework, I expand the analysis to include workers from states outside of Massachusetts. The econometric specification is now:

$$\begin{gathered} Y_{i,t,s} = \sum_k \alpha_k * Occupation + \sum_t \beta_t * Year_t + \sum_s \sigma_s * State_s + \delta * After_t + \\ \sum_m \rho_m * After_t \times State_s + \sum_n \kappa_n * After_t \times Occupation + \sum_o \upsilon_o * State_s \times Occupation_i + \\ \gamma * After_t \times Massachusetts_s \times Non-Exempt_i + \lambda * X_{i,t,s} + \epsilon_{i,t,s} \end{gathered} \tag{18}$$

Now fixed effects are included for (1) the interaction between state and time (before and after *Goodrow*), (2) the interaction between state and occupation, and (3) the interaction between occupation and time (before and after *Goodrow*). The γ coefficient is now the coefficient in front of the state-time-occupation interaction, and it is the triple-differences estimator for being a non-exempt employee in Massachusetts after the *Goodrow* decision.

It is possible that for a given industry, the change in penalties would impact the number of workers within each occupation, as employers may attempt to classify workers as working in exempt occupations to avoid overtime requirements. Thus, I also assess how the change in overtime penalties affected the percent of workers who are in exempt occupations within an industry and sector, as opposed to nonexempt occupations in the industry and sector. The econometric specification for this analysis is as follows:

$$Percent\ Covered_{i,t} = \sum_i \alpha_i * Industry + \gamma * After_{i,t} + \lambda * X_{i,t} + \epsilon_{i,t} \tag{19}$$

I restrict analysis to Massachusetts workers, and the unit of observation is the combination of industry and sector, indexed by i, in year t. The coefficient δ represents the effect of being in the post-*Goodrow* period (after July of 2000), and the coefficient γ estimates the effect of being in this post-*Goodrow* period. There are fixed effects for each combination of industry and sector, which are denoted in Equations (19) and (20) as simply Industry. The variable $X_{i,t}$ is again a vector of various controls, and $\varepsilon_{i,t}$ is again the error term.

Finally, to more credibly identify the effect of *Goodrow* on the percent of workers covered by the overtime law, I use a difference-in-differences framework to assess how the composition of each industry-sector combination changed after *Goodrow* relative to the same industry-sector combinations in other states. The econometric specification here is:

$$\begin{gathered} Percent\ Covered_{i,t,s} = \sum_i \alpha_i * Industry + \sum_t \beta_t * Year_t + \\ \sum_s \sigma_s * State_s + \delta * After_t + \gamma * After_t \times Massachusetts_s + \lambda * X_{i,t} + \epsilon_{i,t} \end{gathered} \tag{20}$$

The specification now includes fixed effects for each state and year, and the coefficient γ now estimates the effect of being in the post-*Goodrow* period in Massachusetts. It is therefore the difference-in-differences estimator for the effect of *Goodrow* on the percent of workers within each industry and sector who covered by the Massachusetts overtime law.

1.5 Results

In this section I examine the effect of mandatory treble damages on undercompensated overtime hours worked to assess how employers respond to changes in penalties for overtime violations. An initial question to answer is whether the *Goodrow* decision affected the overall number of undercompensated overtime hours worked in Massachusetts. *Goodrow* only affected the cost of undercompensated overtime hours, and not fully compensated overtime hours or non-overtime hours. Thus, if *Goodrow* did not affect the demand for undercompensated overtime hours, then there would be no substitution effects on fully compensated overtime hours and regular hours, and therefore likely no impact on labor demand generally.

Figure 1.2 uses CPS data to plot the average number of undercompensated overtime hours worked by month for Massachusetts workers who are nonexempt from the Massachusetts overtime law. Figure 1.3 plots the fraction of total hours worked that are undercompensated overtime hours over time for the same population. In both figures, the sample excludes computer systems analysts, computer programmers, software engineers, and other similarly skilled workers (these workers were made exempt from the federal overtime law in 1996). The sample also excludes individuals who did not report how many hours they typically worked per week or who reported that their typical hours per week varies. Finally, the sample excludes individuals who reported being self-employed or an unpaid family member, or whose sector was unknown.

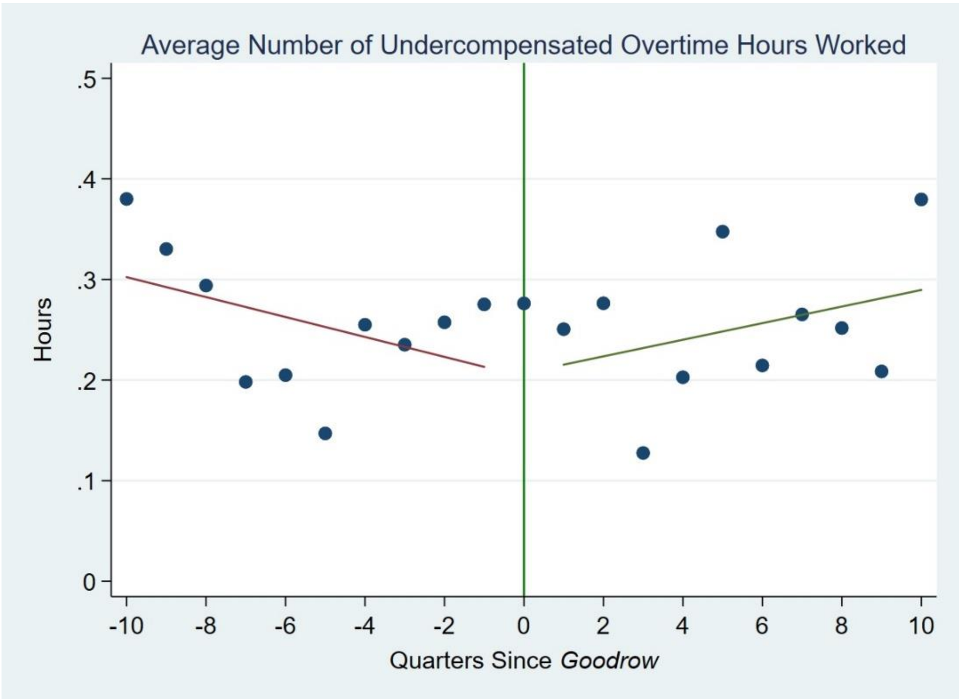

Figure 1.2: Average Number of Undercompensated Overtime Hours Worked

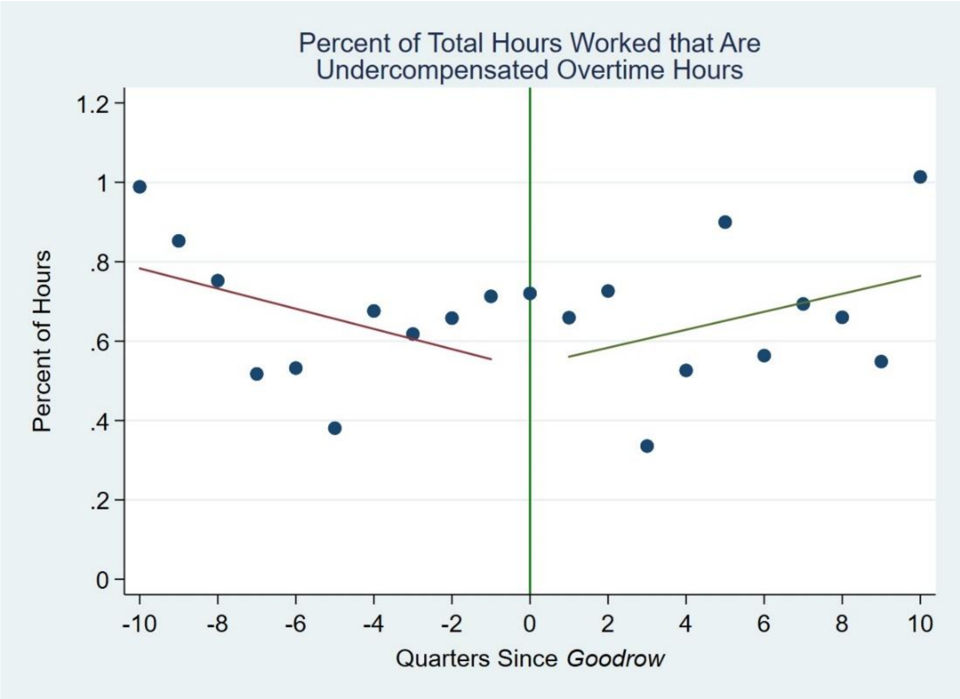

Figure 1.3: Fraction of Total Hours Worked that Are Undercompensated Overtime Hours

Both the number of overtime hours worked and the percentage of total hours worked that are undercompensated overtime hours appear to have increased somewhat after the court in *Goodrow* struck down mandatory treble damages, a result that economic theory would predict. However, the relationship above could be driven by outside forces, such as general trends in typical working hours or greater labor market mobility that allows employers to hire additional workers as opposed to having existing employees work overtime.

The next step is to examine how the gap in hours worked between covered workers changed after the *Goodrow* decision, relative to non-covered workers. In the subsequent analysis, there is a control group and two treatment groups. The control group includes workers who are not covered by the Massachusetts overtime law, while both treatment groups include workers who are covered by the Massachusetts overtime law. The first treatment group includes workers who are covered by the federal overtime law. For these workers, the amount of damages they could recover fell from treble damages to double damages after *Goodrow* (unless they could show that the failure to pay overtime was intentional). The second treatment group includes workers who are not covered by the federal overtime law. For these workers, the ordinary amount of damages they could recover fell from treble to single damages after *Goodrow*. The balance table for these three groups is displayed below:

Table 1.1: Balance Table

	Control	Treated 1	Treated 2
Observations	24,947	22,400	2,262
Average Age	40.9	38.0	41.2122
% Married	64.0%	53.8%	56.8%
% US Citizen	94.6%	90.5%	97.7%
% Female	53.8%	39.4%	36.9%
% Black	4.35%	5.26%	5.70%
% Asian/Pacific Islander	2.03%	3.15%	1.50%
% Hispanic	2.92%	5.15%	3.67%
% High School Diploma or Equivalent	95.4%	85.2%	91.8%
% College Degree	52.0%	23.8%	24.4%
% Union Member	17.2%	15.1%	62.2%
% Covered by Union	18.8%	16.1%	64.5%

The sample includes Massachusetts workers surveyed in the Current Population Survey from 1996 to 1999 (the year prior to the *Goodrow* decision). The control group includes workers who are not covered by the Massachusetts overtime law. The first treatment group includes workers who are covered by the Massachusetts overtime law but not the federal overtime law. The second treatment group includes workers who are covered by the Massachusetts overtime law but not the federal overtime law. Individuals whose coverage status under either overtime law cannot be determined are also dropped from the sample. Individuals are dropped if they are self-employed, if they work as an unpaid family member, or if their sector is unknown.

Some differences between the three groups are notable. First, while the number of observations in the control group and the first treated group are similar, there are far fewer individuals in the second treatment group. There is considerable overlap between the coverage details of the Massachusetts overtime law and the federal law, and therefore there are relatively few workers who are covered by the Massachusetts law but not the federal law. Given the relatively small sample size of the second treatment group, the estimated effects of *Goodrow* on the outcome variables of interest for this group should be interpreted with some caution.

Additionally, workers in the control group are more likely to be married than workers in the treatment groups, and they are more likely to be female. The workers in the control group are also more likely to be highly educated, with 52.0% of the workers in the control group having a college degree, compared to 23.8% and 24.4% of the workers from the first and second treatment groups. Finally, the second treatment group is far more likely to be a union member or covered by a union than the control group or the first treatment group.

Before beginning my main analysis, I first examine whether the 1998 change in penalties for Massachusetts overtime laws affected any of the outcome variables of interest. In 1998 the Massachusetts legislature changed a statute providing for penalties for overtime violations. The legislature increased the maximum fine for an employer that violates the Massachusetts overtime law, but it removed the threat of imprisonment. The 1998 penalty may therefore increase or decrease the deterrent effect of the overtime law. In Subsection A I assess whether this penalty change affected undercompensated overtime hours worked, fully compensated overtime hours worked, regular hours worked, and total hours worked. I also evaluate whether this change affected the percent of workers who are categorized into exempt and nonexempt occupations.

I then move on to the main analysis of the paper. Subsection B presents the empirical results for Massachusetts for undercompensated overtime hours worked, fully compensated overtime hours worked, regular hours worked, and total hours worked respectively. In Subsection C, I examine how increased damages affected the fraction of workers in each sector who are categorized into exempt and nonexempt occupations. Finally, Subsection D includes three robustness checks for the main analysis. First, I examine whether the main results in Subsection B change after allowing for lagged effects. Second, I assess whether the main results change when restricting analysis to for-profit private employers. Third, I examine whether the main results are being driven by the inclusion of the control for the 1998 change in penalties.

1.5.1 Effect of 1998 Penalty Change

This subsection examines the extent to which the 1998 penalty change—which substantially increased the maximum fine but removed the threat of imprisonment for overtime violations—affected any outcome variables of interest, including hours worked and the percent of workers covered by the overtime law. I begin with a difference-in-differences analysis. Table 1.2 below presents regression results for Equation (17) for (1) undercompensated overtime hours worked, (2) fully compensated overtime hours worked, (3) non-overtime hours worked, and (4) total hours worked. In each of the models, the treatment period is August 1998 and later.

Table 1.2: Effect of 1998 Penalty Change on Hours Worked (DD)

	Undercompensated OT Hours Worked	Fully Compensated OT Hours Worked	Non-Overtime Hours Worked	Total Hours Worked
Female	-1.43***	-0.521***	-2.35***	-4.30***
	(0.182)	(0.0823)	(0.425)	(0.498)
Black	-0.0566	-0.111	1.81***	1.64***
	(0.171)	(0.112)	(0.383)	(0.473)
Asian/Pacific Islander	-0.000373	-0.445***	2.04**	1.60*
	(0.409)	(0.0725)	(0.845)	(0.923)
Hispanic	-0.488	-0.235**	2.03***	1.30**
	(0.132)	(0.0939)	(0.522)	(0.570)
DD Estimator	0.0391	-0.104	0.284	0.218
	(0.165)	(0.110)	(0.243)	(0.331)
Year FE	✓	✓	✓	✓
Ind./Occ./Sector FE	✓	✓	✓	✓
N	15,838	15,838	15,838	15,838
R^2	0.2100	0.0866	0.2829	0.2921

Robust standard errors clustered by each combination of industry, occupation, and sector are used in each model. The sample includes Massachusetts workers surveyed in the Current Population Survey from 1996 to June 2000 (the month prior to the *Goodrow* decision). The sample excludes individuals who did not report how many hours they typically worked per week or who reported that their typical hours per week varies. The sample also excludes individuals who are self-employed, who work as an unpaid family member, or whose sector is unknown. Finally, the sample excludes individuals whose receipt of overtime, tips, or commissions is unknown.
Significance: * 0.10 ** 0.05 *** 0.01

The 1998 change in penalties is estimated to have increased the average number of undercompensated overtime hours worked, decreased the average number of fully compensated overtime hours worked, and increased the average number of non-overtime hours worked. The results suggest that the 1998 change in penalties may have encouraged overtime violations, possibly by removing the threat of imprisonment for overtime violations. However, each of the estimated effects are small and statistically indistinguishable from zero, suggesting that the 1998 change in penalties had little effect on each category of hours worked.

I next examine the estimated impact of the 1998 change in penalties on hours worked using a triple-differences framework. Table 1.3 below presents regression results for Equation (18) for each of the four categories of hours worked above.

Table 1.3: Effect of 1998 Penalty Change on Hours Worked (DDD)

	Undercompensated OT Hours Worked	Fully Compensated OT Hours Worked	Non-Overtime Hours Worked	Total Hours Worked
Female	-1.27***	-0.675***	-1.62***	-3.56***
	(0.0741)	(0.0418)	(0.130)	(0.194)
Black	-0.563***	-0.255***	0.806***	-0.0120
	(0.0721)	(0.0321)	(0.0844)	(0.128)
Asian/Pacific Islander	0.436***	-0.256***	0.698***	0.878***
	(0.161)	(0.0643)	(0.235)	(0.341)
Hispanic	-0.409***	-0.312***	1.17***	0.447**
	(0.0654)	(0.0353)	(0.156)	(0.190)
Effective Minimum Wage	0.105	0.0331	-0.195	-0.0572
	(0.156)	(0.0922)	(0.270)	(0.360)
Massachusetts	-0.274**	0.0869	-0.0815	-0.268
	(0.116)	(0.100)	(0.132)	(0.226)
After 1998 Penalties Change x Massachusetts	0.0683	-0.0816	-0.216	-0.230
	(0.229)	(0.225)	(0.172)	(0.385)
DDD Estimator	-0.417	3.17***	-0.887	1.87
	(0.954)	(0.286)	(0.644)	(1.21)
Year Fixed Effects	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓
Ind./Occ./Sector FE	✓	✓	✓	✓
Post-Change x State FE	✓	✓	✓	✓
Post-Change x Ind./Occ./Sector FE	✓	✓	✓	✓
State x Ind./Occ./Sector FE	✓	✓	✓	✓
N	148,211	148,211	148,211	148,211
R^2	0.1544	0.0489	0.2284	0.2435

Robust standard errors clustered by each combination of state, industry, occupation, and sector are used in each model. See note for Table 1.2 for sample details, except that the sample is no longer restricted to Massachusetts workers. Significance: * 0.10 ** 0.05 *** 0.01

Now the 1998 change in penalties is estimated to have decreased the average number of undercompensated overtime hours worked, increased the average number of fully compensated overtime hours worked, and decreased the average number of non-overtime hours worked—estimates that are the opposite sign from the difference-in-differences framework. Furthermore, the estimated effect for fully compensated overtime hours is large and statistically significant at the 1% significance level (though the remaining estimates continue to be statistically indistinguishable from zero). The triple-differences framework therefore suggests that the 1998 change in penalties did deter overtime violations, and employers appear to have substituted away from undercompensated overtime hours and towards fully compensated overtime hours.

One puzzling result is that the increase in fully compensated overtime hours is more than seven times the decrease in undercompensated overtime hours. One possible explanation is that workers in the affected occupations became more willing to work overtime because the employer started paying the appropriate overtime rate for overtime hours worked. Another possibility is that the change in penalties prompted employers to become more likely to categorize employees into occupations that are covered by the Massachusetts overtime law, and the employer paid the overtime wage for these hours. Tables 1.4 and 1.5 provide evidence on whether this occurred. Table 1.4 presents regression results for Equation (19), and Table 1.5 presents results for Equation (20). As before, the treatment period is August 1998 and later.

Table 1.4: Effect of 1998 Penalty Change on Proportion of Workers Covered by Overtime Law

	(1) Unweighted	(2) Unweighted	(3) Weighted	(4) Weighted
% Female	-	0.0179	-	-0.0236
		(0.110)		(0.0637)
% Black	-	0.0175	-	-0.102
		(0.174)		(0.127)
% Asian/Pacific Islander	-	0.0486	-	-0.103
		(0.262)		(0.241)
% Hispanic	-	0.0848	-	0.236**
		(0.181)		(0.112)
Effective Minimum Wage	-	-0.0165	-	-0.0154
		(0.0218)		(0.0114)
After 1998 Penalties Change	-0.00457	0.0000813	0.00344	0.00569
	(0.0123)	(0.0156)	(0.00665)	(0.00826)
Ind./Sector FE	✓	✓	✓	✓
N	243	243	243	243
R^2	0.8990	0.8994	0.9730	0.9737

Robust standard errors are used in each model. Observations are unweighted in specifications (1) and (2), while observations are weighted by the number of workers within each industry, sector, and year in specifications (3), and (4). The sample includes Massachusetts workers surveyed in the Current Population Survey from 1996 to June 2000 (the month prior to the *Goodrow* decision). The sample excludes individuals who are self-employed, who work as an unpaid family member, or whose sector is unknown. The sample also excludes individuals whose coverage status under the Massachusetts overtime law could not be determined. Combinations of industry, sector, and year with fewer than 10 observations are dropped. Significance: * 0.10 ** 0.05 *** 0.01

Table 1.5: Effect of 1998 Penalty Change on Proportion of Workers Covered by Overtime Law (DD)

	(1) Unweighted	(2) Unweighted	(3) Weighted	(4) Weighted
% Female	-	0.00299	-	0.0206***
		(0.00637)		(0.00589)
% Black	-	0.0994***	-	0.106***
		(0.00829)		(0.00755)
% Asian/Pacific Islander	-	0.0528**	-	-0.0161
		(0.0213)		(0.0199)
% Hispanic	-	0.0334***	-	0.0647***
		(0.00876)		(0.00793)
Effective Minimum Wage	-	-0.00108	-	0.00113
		(0.00630)		(0.00575)
Massachusetts	-0.0104*	0.00699	-0.0354***	-0.0166***
	(0.00557)	(0.00634)	(0.00480)	(0.00572)
DD Estimator	0.00934	0.00843	0.00830	0.00722
	(0.00630)	(0.00628)	(0.00546)	(0.00551)
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Ind./Sector FE	✓	✓	✓	✓
N	33,939	33,939	33,939	33,939
R^2	0.8820	0.8827	0.9241	0.9247

Robust standard errors are used in each model. Observations are unweighted in specifications (1) and (2), while observations are weighted by the number of workers within each industry, sector, state, and year in specifications (3), and (4). See note for Table 1.4 for sample details, except that the sample is no longer restricted to Massachusetts workers. Significance: * 0.10 ** 0.05 *** 0.01

In general, the 1998 penalty change is estimated to have increased the percentage of workers within each industry and sector who are covered by the Massachusetts overtime law. Only the unweighted difference-in-differences model without controls suggests that the impact is negative. The remaining seven specifications indicate that the change in penalties had a small positive impact that is statistically indistinguishable from zero. Specification (4) in Table 1.5 is the preferred specification, as it uses a more compelling identification strategy (difference-in-differences), observations are weighted by the number of workers within each industry and sector, and it includes controls for the gender and racial composition of each industry and sector, as well as the effective minimum wage. This specification estimates that the 1998 change in penalties caused the percent of workers in covered occupations to increase by 0.722%. All four of the triple-differences models provide similar estimated effects of the 1998 penalties change, which provides some confidence that they are displaying the genuine effect of the change.

Overall, the evidence suggests that the 1998 change in penalties may have affected the average number of hours worked and the percent of workers in covered occupations within each industry and sector. Therefore, to ensure that this change is not leading to biases in the main analysis, the models in the following subsections will control for the effect of the 1998 change.

1.5.2 Effect of Reduced Penalties on Overtime and Non-Overtime Hours Worked

This subsection examines the extent to which reduced penalties for overtime violations affects the number of overtime and non-overtime hours worked. I rely on variation in the effect of the Massachusetts Supreme Court’s decision in *Goodrow* to examine how reduced overtime penalties affects four categories of hours worked: (1) undercompensated overtime hours, (2) fully compensated overtime hours, (3) non-overtime hours, and (4) total hours. Table 1.6 below presents the regression results for Equation (17) for these four categories of labor.

Table 1.6: Effect of *Goodrow* on Hours Worked (Difference-in-Differences)

	Undercompensated OT Hours Worked	Fully Compensated OT Hours Worked	Non-Overtime Hours Worked	Total Hours Worked
Female	-1.37***	-0.473***	-2.38***	-4.22***
	(0.155)	(0.0668)	(0.326)	(0.384)
Black	-0.339***	-0.106	1.57***	1.12***
	(0.102)	(0.0872)	(0.239)	(0.295)
Asian/Pacific Islander	-0.274	-0.377***	1.11**	0.463
	(0.264)	(0.0520)	(0.542)	(0.623)
Hispanic	-0.448***	-0.187***	1.65***	1.01***
	(0.120)	(0.0511)	(0.353)	(0.381)
After 1998 Penalties	-0.0390	-0.118	0.302	0.145
Change x Covered Worker	(0.143)	(0.0889)	(0.215)	(0.281)
DD Estimator (Treble to	0.163	-0.00533	-0.268	-0.110
Double Damages)	(0.143)	(0.0705)	(0.249)	(0.294)
DD Estimator (Treble to	0.328	0.528***	-0.763*	0.0921
Single Damages)	(0.309)	(0.200)	(0.397)	(0.575)
Year FE	✓	✓	✓	✓
Ind./Occ./Sector FE	✓	✓	✓	✓
N	30,542	30,542	30,542	30,542
R^2	0.1920	0.0680	0.2594	0.2748

Robust standard errors clustered by each combination of industry, occupation, and sector are used in each model. The sample includes Massachusetts workers surveyed in the Current Population Survey from 1996 to 2004. The sample excludes individuals who did not report how many hours they typically worked per week or who reported that their typical hours per week varies. The sample also excludes individuals who are self-employed, who work as an unpaid family member, or whose sector is unknown. Finally, the sample excludes individuals whose receipt of overtime, tips, or commissions is unknown. Significance: * 0.10 ** 0.05 *** 0.01

The results for workers who are covered by both the Massachusetts overtime law and the federal overtime law are consistent with the model described in Section III. For these workers, after the decrease in penalties for overtime violations, the average number of undercompensated overtime hours worked increased, while the average number of fully compensated overtime hours and non-overtime hours worked decreased. This is consistent with economic theory, which predicts that employers would substitute away from fully compensated overtime and non-overtime hours and towards undercompensated overtime hours when the penalty for overtime violations decreases. However, the estimated effects are all small and statistically insignificant.

For workers who are covered by the Massachusetts overtime law but not the federal law, the average number of undercompensated and fully compensated overtime hours worked increased, while the average number of non-overtime hours worked decreased. The estimated increase in fully compensated overtime hours is relatively large and statistically significant at the 1% significance level. Meanwhile, the estimated decrease in non-overtime hours is even larger and significant at the 10% significance level. The results indicate that, for the individuals who were covered by the Massachusetts overtime law but not the federal law, the reduction in expected penalties caused workers to work fewer non-overtime hours and more overtime hours (both undercompensated and fully compensated). However, the results for workers who are exempt from the federal overtime law should be interpreted with caution, as this represents a much smaller sample than the population that is covered by the federal overtime law.

Table 1.7 below presents the regression results for the triple-differences framework shown in Equation (18) for the same four categories of labor. Now the data sample is expanded to include states other than Massachusetts, and fixed effects are included for each state and for each combination of occupation, industry, and sector within each state. There are also fixed effects for each state after the *Goodrow* decision, as well as fixed effects for each combination of occupation, industry, and sector after *Goodrow*. The DDD estimator captures the effect of being a worker in Massachusetts who is covered by the Massachusetts overtime law after *Goodrow* was decided, and this estimates the effect of *Goodrow* on each category of hours worked.

Table 1.7: Effect of *Goodrow* on Hours Worked (Triple-Differences)

	Undercompensated OT Hours Worked	Fully Compensated OT Hours Worked	Non-Overtime Hours Worked	Total Hours Worked
Female	-1.19***	-0.598***	-1.62***	-3.41***
	(0.0644)	(0.0340)	(0.115)	(0.168)
Black	-0.571***	-0.190***	0.953***	0.192*
	(0.0599)	(0.0236)	(0.0747)	(0.111)
Asian/Pacific Islander	0.189*	-0.255***	0.404*	0.338
	(0.112)	(0.0431)	(0.208)	(0.286)
Hispanic	-0.490***	-0.284***	1.17***	0.398**
	(0.0610)	(0.0268)	(0.145)	(0.175)
Effective Minimum Wage	-0.0390	0.0255	-0.266*	-0.280
	(0.105)	(0.0557)	(0.137)	(0.193)
After 1998 Penalties	-0.135	-0.300***	0.145	-0.290
Change x Covered Worker	(0.133)	(0.0993)	(0.158)	(0.263)
Massachusetts	-0.139	0.103	-0.0467	-0.0827
	(0.106)	(0.0818)	(0.182)	(0.224)
Post-*Goodrow* x	0.151	-0.138	0.312	0.325
Massachusetts	(0.196)	(0.107)	(0.337)	(0.395)
DDD Estimator (Treble to	0.360**	0.240**	-0.244	0.356
Double Damages)	(0.171)	(0.0944)	(0.357)	(0.467)
Year Fixed Effects	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓
Ind./Occ./Sector FE	✓	✓	✓	✓
Post-*Goodrow* x State FE	✓	✓	✓	✓
Post-*Goodrow* x Ind./Occ./Sector FE	✓	✓	✓	✓
State x Ind./Occ./Sector FE	✓	✓	✓	✓
N	304,103	304,103	304,103	304,103
R^2	0.1470	0.0427	0.2190	0.2348

Robust standard errors clustered by each combination of state, industry, occupation, and sector are used in each model. See note for Table 1.6 for sample details, except that the sample is no longer restricted to Massachusetts workers and individuals are dropped if their combination of state, occupation, industry, and sector occurs fewer than 1,000 times. Significance: * 0.10 ** 0.05 *** 0.01

Consistent with the model described in Section III, after the decrease in penalties for overtime violations, the average number of undercompensated overtime hours worked increased for workers who are covered by both the Massachusetts and the federal overtime law. The estimated effect is more than double the effect estimated using the difference-in-differences framework and is now significant at the 95% significance level. Furthermore, there is now a positive and statistically significant estimated effect on the number of fully compensated overtime hours worked, a result that is inconsistent with the model described in Section III but consistent with effect estimated using the difference-in-differences framework for workers who are covered by the Massachusetts law but not the federal law. *Goodrow* has a negative estimated effect on the average number of non-overtime hours worked, which is consistent with economic predictions and the results from the difference-in-differences framework.

Figure 1.4 below displays the time series of undercompensated overtime hours worked. Figure 1.4(a) shows the average number of undercompensated overtime hours worked for four groups of individuals: (1) Massachusetts workers who work in occupations that are covered by the Massachusetts overtime law (covered occupations); (2) Massachusetts workers who work in non-covered occupations; (3) non-Massachusetts workers who work in covered occupations; and (4) non-Massachusetts workers who work in non-covered occupations. Figure 1.4(b) displays the gap in undercompensated overtime hours worked for Massachusetts workers and for non-Massachusetts workers, as well as the difference in these gaps between Massachusetts and non-Massachusetts workers (the difference-in-differences). The change in the difference-in-differences variable after the 1998 change in penalties and the 2000 *Goodrow* decision can be interpreted as the triple-differences estimator for the effect of these changes in penalties. Figures 1.5 and 1.6 are analogous to Figure 1.4, but these figures refer to fully compensated overtime hours worked and non-overtime hours worked respectively.

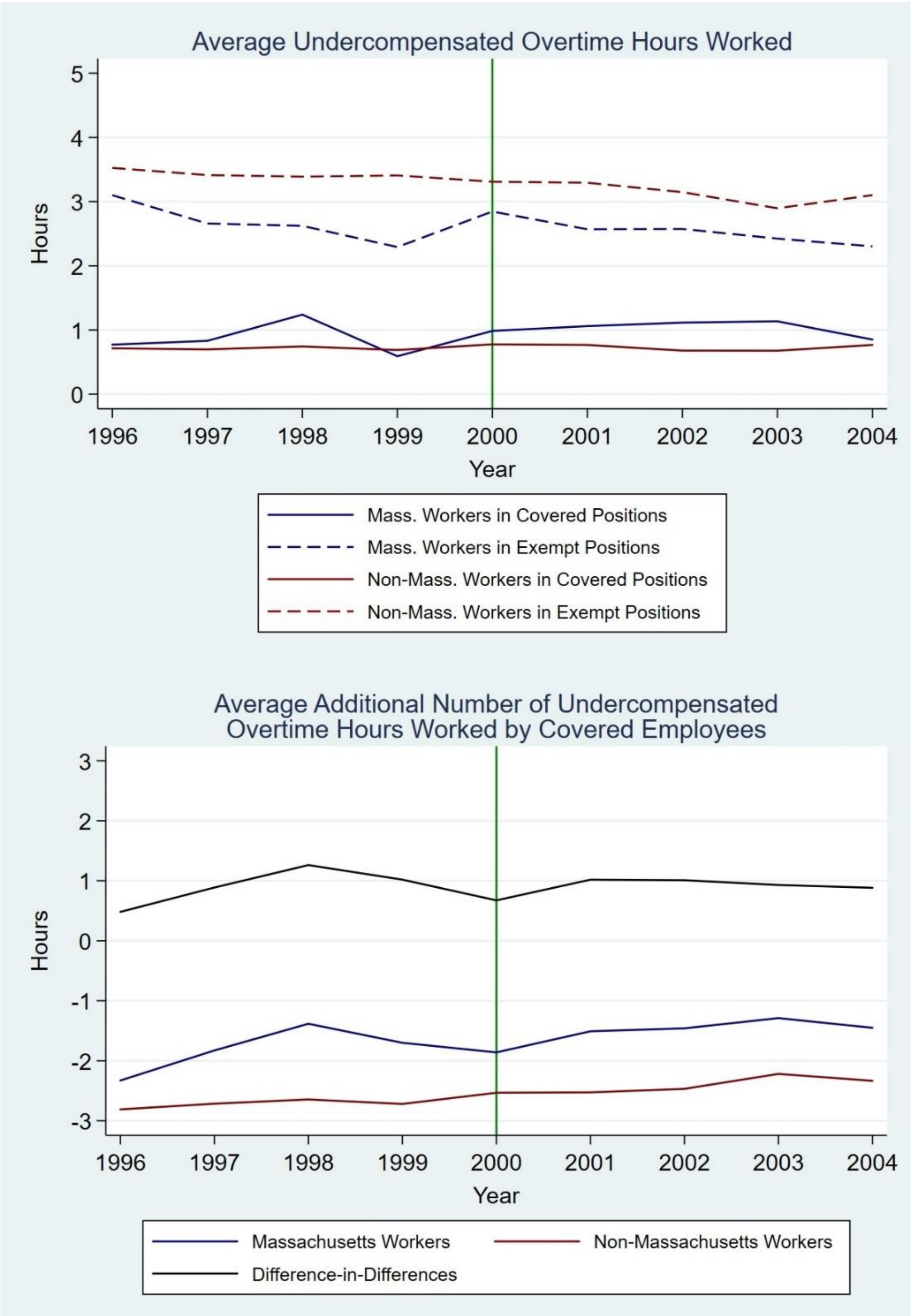

Figure 1.4: Time Series of Undercompensated Overtime Hours Worked

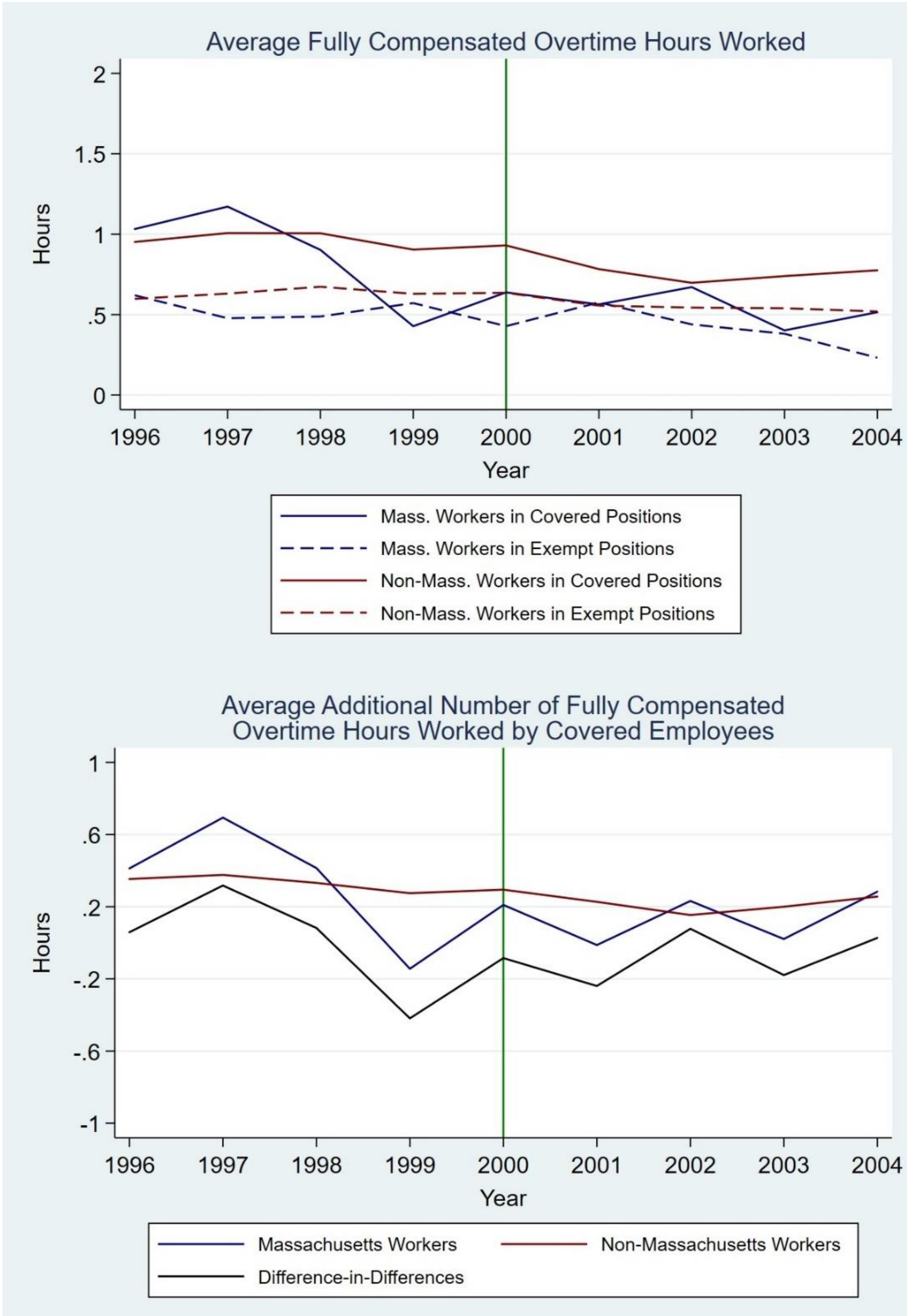

Figure 1.5: Time Series of Fully Compensated Overtime Hours Worked

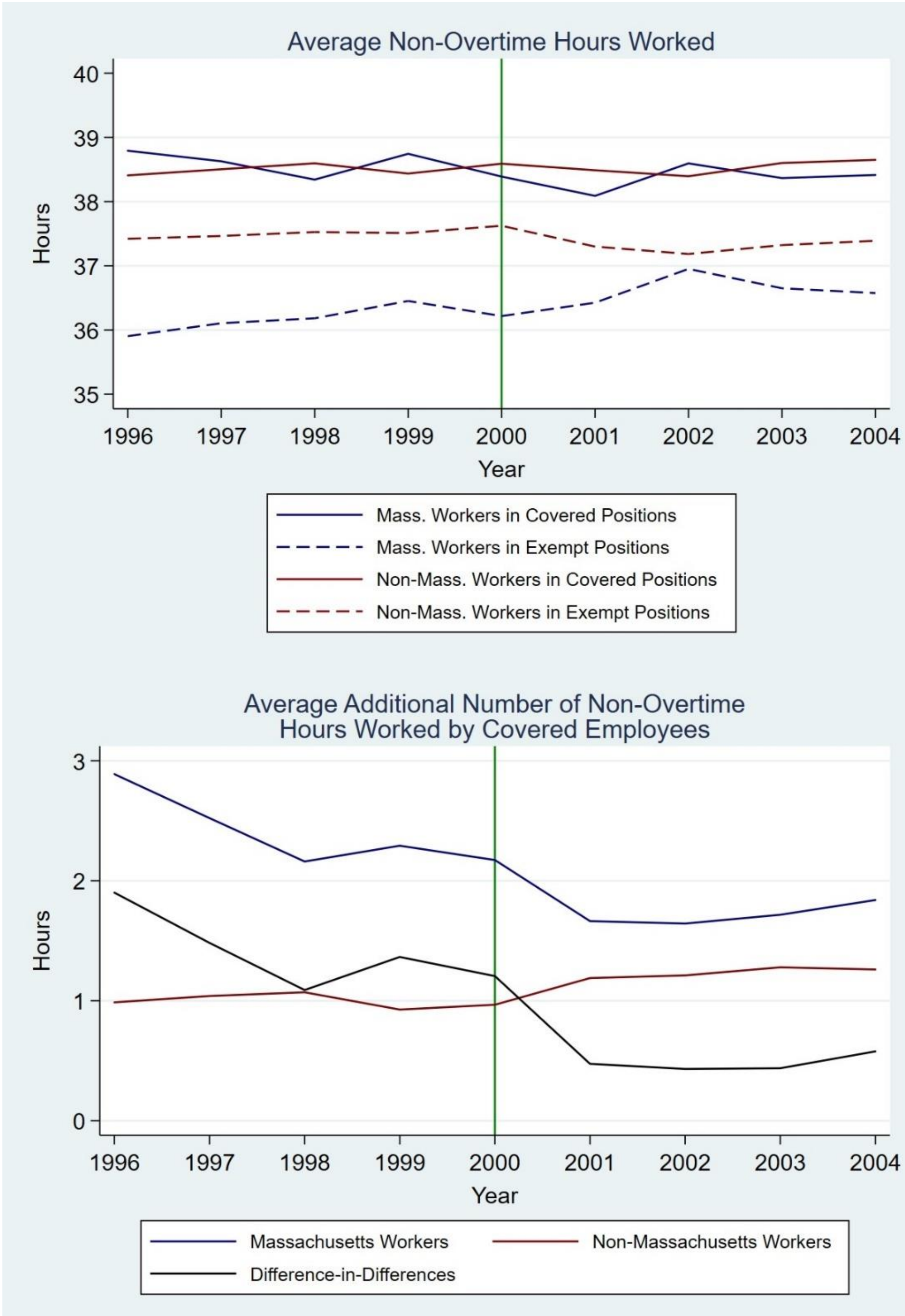

Figure 1.6: Time Series of Non-Overtime Hours Worked

The difference-in-differences trend in Figure 1.4 provides some evidence that the 1998 change in penalties led to a slight decrease in undercompensated overtime hours worked, while *Goodrow*'s reduction in expected penalties led to a slight increase in undercompensated overtime hours worked. Additionally, the difference-in-differences trend in Figure 1.5 suggests that the

1998 change in penalties led a reduction in fully compensated overtime hours worked, while *Goodrow* led to an increase in fully compensated overtime hours worked. Finally, the difference-in-differences trend in Figure 1.6 suggests the opposite effect for non-overtime hours worked, indicating that the 1998 change in penalties increased non-overtime hours worked while the *Goodrow* decision reduced non-overtime hours worked. All of these estimated effects are consistent with the estimated effects shown in Table 1.7.

In sum, both the difference-in-differences framework and the triple-differences framework provide support for economic theory's prediction that reducing the penalty for overtime violations would lead employers to substitute away from non-overtime hours and towards undercompensated overtime hours. However, the triple-differences framework indicates that employers also substitute towards fully compensated overtime hours after the penalty for overtime violations decreases. This behavior is not fully consistent with the model described in Section III, but possible explanations for this result are provided in Section VI.

1.5.3 Effect of Reduced Penalties on Percent of Workers that Are Covered

In this subsection I examine how reduced penalties for overtime violations affects the percent of workers in each industry that are covered by the overtime law. Employers can undercompensate workers for overtime hours worked by categorizing them as being in occupations that are not covered by the overtime law. When the penalties for undercompensating overtime hours decrease, employers may become more likely to miscategorize these workers into occupations that are not covered by the law. This could occur for two reasons. First, an employer may believe that an employee is nonexempt but classifies the employee into an exempt occupation because the employer is willing to risk paying the now lower penalty. Second, the employer may believe that an employee is exempt, but when penalties are higher, the employer will err on the side of caution and classify the employee into a nonexempt occupation. Both explanations would cause the fraction of exempt workers in each industry to increase when the penalty for intentional overtime violations decreases.

Table 1.8 below presents the regression results for Equation (19), modeling the percent of workers within each combination of industry, sector, and year against an indicator variable for whether the observation was in August of 2000 or later. Three of the four specifications suggest that being in the post-*Goodrow* period increases the percent of workers within each industry and sector who are covered by the overtime law. Specification (4) is preferred, as it includes controls for the percent of female employees, Black employees, Asian/Pacific Islander employees, and Hispanic employees within each industry-sector combination. The observations are also weighted to reflect the number of workers within each combination of industry, sector, and year.

Table 1.8: Effect of *Goodrow* on Proportion of Workers Covered by Overtime Law

	(1)	(2)	(3)	(4)
% Female	-	0.0697	-	0.0256
		(0.0709)		(0.0468)
% Black	-	0.308**	-	0.176**
		(0.129)		(0.0836)
% Asian/Pacific Islander	-	0.180	-	0.0590
		(0.240)		(0.150)
% Hispanic	-	-0.0749	-	0.0243
		(0.143)		(0.0864)
Effective Minimum Wage	-	-0.0117	-	-0.0142
		(0.0157)		(0.0105)
After 1998 Penalties Change	-0.00312	-0.00260	0.00294	0.00660
x Covered Worker	(0.0131)	(0.0148)	(0.00702)	(0.00784)
Post-*Goodrow*	0.00338	0.0151	-0.00548	0.0100
	(0.0116)	(0.0188)	(0.00566)	(0.0142)
Ind./Sector FE	✓	✓	✓	✓
N	454	454	454	454
R^2	0.8503	0.8565	0.9668	0.9674

Robust standard errors are used in each model. Observations are unweighted in specifications (1) and (2), while observations are weighted by the number of workers within each industry, sector, and year in specifications (3), and (4). The sample includes Massachusetts workers surveyed in the Current Population Survey from 1996 to 2004. The sample excludes individuals who are self-employed, who work as an unpaid family member, or whose sector is unknown. The sample also excludes individuals whose coverage status under the Massachusetts overtime law could not be determined. Combinations of industry, sector, and year with fewer than 10 observations are dropped. Significance: * 0.10 ** 0.05 *** 0.01

Specification (4) indicates that being in the post-*Goodrow* period increases the percent of workers in occupations that are covered by the overtime law by 1.00%. This estimated effect is of a similar magnitude as the estimated effect from Specification (2), which suggests that being in the post-Goodrow period increases the percent of workers in covered occupations by 1.51%. However, the coefficient estimates for being in the post-*Goodrow* period for all of the specifications are statistically indistinguishable from zero.

Table 1.9 below presents the regression results for Equation (20). Equation (20) expands the analysis in Table 1.8 to states other than Massachusetts, and it includes fixed effects for each state and year. The DD estimator is the effect of being in Massachusetts in the post-*Goodrow* period on the percent of workers that are classified in covered occupations.

Table 1.9: Effect of *Goodrow* on Proportion of Workers Covered by Overtime Law (Difference-in-Differences)

	(1) Unweighted	(2) Unweighted	(3) Weighted	(4) Weighted
% Female	-	0.0175***	-	0.0249***
		(0.00456)		(0.00425)
% Black	-	0.107***	-	0.102***
		(0.00579)		(0.00537)
% Asian/Pacific Islander	-	0.0176	-	-0.0359***
		(0.0132)		(0.0126)
% Hispanic	-	0.0556***	-	0.0832***
		(0.00596)		(0.00536)
Effective Minimum Wage	-	0.00607***	-	0.000662
		(0.00231)		(0.00212)
After 1998 Penalties Change	0.00398	0.00368	0.00366	0.00299
x Covered Worker	(0.00311)	(0.00309)	(0.00261)	(0.00260)
Massachusetts	-0.00896*	0.00752	-0.0292***	-0.0119***
	(0.00495)	(0.00517)	(0.00418)	(0.00443)
DD Estimator	0.00298	-0.00455	0.0000714	-0.000442
	(0.00543)	(0.00592)	(0.00490)	(0.00537)
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Ind./Sector FE	✓	✓	✓	✓
N	70,961	70,961	70,961	70,961
R^2	0.8762	0.8771	0.9191	0.9198

Robust standard errors are used in each model. Observations are unweighted in specifications (1) and (2), while observations are weighted by the number of workers within each industry, sector, state, and year in specifications (3), and (4). See note for Table 1.8 for sample details, except that the sample is no longer restricted to Massachusetts workers. Significance: * 0.10 ** 0.05 *** 0.01

Specification (4) is again preferred, and it indicates that being in Massachusetts in the post-*Goodrow* period actually decreased the percent of workers in covered occupations by 0.0442%. The estimated effects from the other models range from a reduction of 0.455% to an increase of 0.298%. None of the results are statistically significant at any standard significance level, suggesting that the effect of *Goodrow* on the percent of workers covered by the Massachusetts overtime law was relatively small.

Figure 1.7 below displays the time series of the percentage of workers in occupations covered by the Massachusetts overtime law. Figure 1.7 displays the trend of this variable from 1996 to 2004 for Massachusetts workers, and for non-Massachusetts workers. There is a notable decrease in the percentage of Massachusetts workers in occupations that are covered by the Massachusetts overtime law, relative to non-Massachusetts workers. However, this drop does not occur until 2002, and there is a contemporaneous (though smaller) drop in the percent of non-Massachusetts workers in occupations that would be covered by the Massachusetts law.

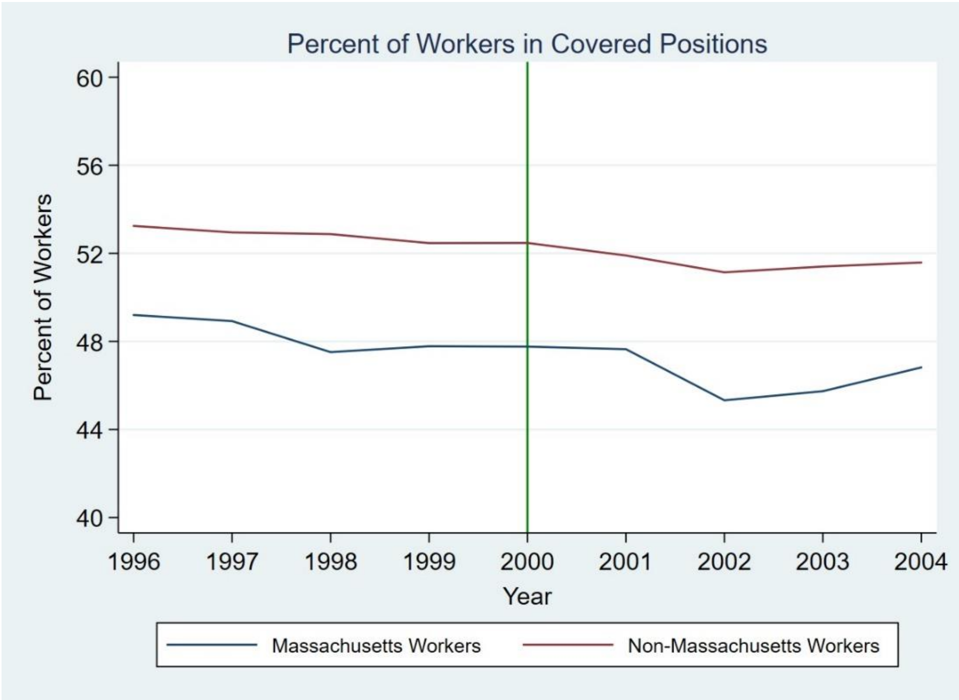

Figure 1.7: Time Series of Percent of Workers in Covered Occupations

Overall, there is no clear impact of *Goodrow* on the percent of workers within each industry and sector who are covered by the overtime law from any of the specifications above. Thus, if the *Goodrow* decision prompted employers to reclassify workers to be covered or not covered by the Massachusetts overtime law, the effect was modest at best.

1.5.4 Robustness Checks

This subsection examines the robustness of the main results from Subsection B. First, I evaluate whether the estimated effect of *Goodrow* on hours worked changes after allowing for lagged effects. Second, I assess whether the estimated effect of *Goodrow* on hours worked changes when restricting analysis to for-profit private employers. Third, I examine whether the main results are being driven by the inclusion of a control for the 1998 penalties change.

Table 1.10 below presents the regression results for the triple-differences framework shown in Equation (18) for each of the four categories of labor, except that an additional variable is added. The lagged DDD estimator takes the value of 1 for individuals in Massachusetts who are covered by the Massachusetts overtime law and who are surveyed in August of 2001 or later. Otherwise, it takes the value of 0. Thus, the DDD estimator reflects the immediate effect of *Goodrow* on hours worked, and the sum of DDD estimator and lagged DDD estimator reflects the effect of *Goodrow* on hours worked at least one year after *Goodrow* was decided.

Table 1.10: Effect of *Goodrow* on Hours Worked (Triple-Differences with One-Year Lag)

	Undercompensated OT Hours Worked	Fully Compensated OT Hours Worked	Non-Overtime Hours Worked	Total Hours Worked
Female	-1.19***	-0.598***	-1.62***	-3.41***
	(0.0644)	(0.0340)	(0.115)	(0.168)
Black	-0.571***	-0.190***	0.953***	0.192*
	(0.0599)	(0.0236)	(0.0747)	(0.111)
Asian/Pacific Islander	0.189*	-0.255***	0.404*	0.338
	(0.112)	(0.0431)	(0.208)	(0.286)
Hispanic	-0.490***	-0.284***	1.17***	0.398**
	(0.0610)	(0.0268)	(0.145)	(0.175)
Effective Minimum Wage	-0.0407	0.0301	-0.278**	-0.289
	(0.105)	(0.0562)	(0.138)	(0.194)
After 1998 Penalties Change x	-0.135	-0.300***	0.144	-0.291
Covered Worker	(0.133)	(0.0994)	(0.158)	(0.263)
Massachusetts	-0.138	0.100	-0.0389	-0.0768
	(0.106)	(0.0824)	(0.184)	(0.226)
Post-*Goodrow* x Massachusetts	0.153	-0.143	0.325	0.336
	(0.197)	(0.107)	(0.338)	(0.395)
DDD Estimator (Treble to	0.307	0.387***	-0.620	0.0733
Double Damages)	(0.219)	(0.128)	(0.567)	(0.682)
Lagged DDD Estimator (Treble	0.0728	-0.199	0.509	0.384
to Double Damages)	(0.185)	(0.128)	(0.361)	(0.355)
Year Fixed Effects	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓
Ind./Occ./Sector FE	✓	✓	✓	✓
Post-*Goodrow* x State FE	✓	✓	✓	✓
Post-*Goodrow* x Ind./Occ./Sector FE	✓	✓	✓	✓
State x Ind./Occ./Sector FE	✓	✓	✓	✓
N	304,103	304,103	304,103	304,103
R^2	0.1470	0.0427	0.2190	0.2348

Robust standard errors clustered by each combination of state, industry, occupation, and sector are used in each model. See note for Table 1.7 for sample details. Significance: * 0.10 ** 0.05 *** 0.01

The results in Table 1.10 are qualitatively similar to the results in Table 1.7. Table 1.10 indicates that *Goodrow* increased the average number of undercompensated overtime hours worked by 0.307 hours per week within one year of the decision, and by 0.379 hours per week more than one year after the decision. The results for fully compensated overtime and non-overtime hours worked in Table 1.10 are also similar to the corresponding results in Table 1.7. The lagged DDD estimator is the opposite sign of the DDD estimator for fully compensated overtime and non-overtime hours worked, suggesting that *Goodrow* led to a significant immediate change in these variables followed by a reduced effect in the long-term.

Table 1.11 below presents the regression results for the triple-differences framework shown in Equation (18) for each of the four categories of labor, with the sample now limited to individuals who reported working for a for-profit private organization. The results in Table 1.11 for overtime hours worked are nearly identical to the corresponding results in Table 1.7. Notably, Table 1.11 estimates that the *Goodrow* decision led to a decrease in non-overtime hours worked of only 0.0847 hours per week, roughly a third of the estimated decrease of 0.244 hours per week from Table 1.7. However, in both cases the estimated decrease is statistically indistinguishable from zero, and the similarity of results between Tables 1.7 and 1.11 for undercompensated and fully compensated overtime hours worked suggests that restricting attention to for-profit private employers does not significantly alter the results.

Table 1.11: Effect of *Goodrow* on Hours Worked (Triple-Differences For-Profit Private)

	Undercompensated OT Hours Worked	Fully Compensated OT Hours Worked	Non-Overtime Hours Worked	Total Hours Worked
Female	-1.20***	-0.674***	-1.73***	-3.61***
	(0.0708)	(0.0355)	(0.127)	(0.186)
Black	-0.458***	-0.206***	1.03***	0.361***
	(0.0554)	(0.0257)	(0.0792)	(0.112)
Asian/Pacific Islander	0.359***	-0.279***	0.867***	0.948***
	(0.113)	(0.0455)	(0.165)	(0.245)
Hispanic	-0.423***	-0.304***	1.30***	0.573***
	(0.0603)	(0.0270)	(0.151)	(0.180)
Effective Minimum Wage	-0.0106	0.0197	-0.279*	-0.270
	(0.0736)	(0.0683)	(0.148)	(0.178)
After 1998 Penalties Change x Covered Worker	-0.147	-0.292***	0.149	-0.290
	(0.132)	(0.0992)	(0.159)	(0.265)
Massachusetts	-0.0932	0.0917	-0.0609	-0.0624
	(0.0979)	(0.0848)	(0.193)	(0.229)
Post-*Goodrow* x Massachusetts	0.0428	-0.119	0.168	0.0913
	(0.181)	(0.125)	(0.377)	(0.425)
DDD Estimator (Treble to Double Damages)	0.378**	0.241**	-0.0847	0.534
	(0.181)	(0.0984)	(0.371)	(0.484)
Year Fixed Effects	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓
Ind./Occ./Sector FE	✓	✓	✓	✓
Post-*Goodrow* x State FE	✓	✓	✓	✓
Post-*Goodrow* x Ind./Occ./Sector FE	✓	✓	✓	✓
State x Ind./Occ./Sector FE	✓	✓	✓	✓
N	266,925	266,925	266,925	266,925
R^2	0.1537	0.0405	0.2347	0.2540

Robust standard errors clustered by each combination of state, industry, occupation, and sector are used in each model. See note for Table 1.7 for sample details, except that the sample is further limited to individuals who work for a for-profit private employer. Significance: * 0.10 ** 0.05 *** 0.01

Table 1.12 below presents the regression results for the triple-differences framework shown in Equation (18) for each of the four categories of labor. Unlike the specifications from Subsection B, the control for the 1998 penalties change is now omitted.

Table 1.12: Effect of *Goodrow* on Hours Worked (No Control for 1998 Change)

	Undercompensated OT Hours Worked	Fully Compensated OT Hours Worked	Non-Overtime Hours Worked	Total Hours Worked
Female	-1.19***	-0.598***	-1.62***	-3.41***
	(0.0644)	(0.0340)	(0.115)	(0.168)
Black	-0.571***	-0.190***	0.953***	0.192*
	(0.0599)	(0.0236)	(0.0747)	(0.111)
Asian/Pacific Islander	0.189*	-0.255***	0.404*	0.338
	(0.112)	(0.0431)	(0.208)	(0.286)
Hispanic	-0.490***	-0.284***	1.17***	0.398**
	(0.0610)	(0.0268)	(0.145)	(0.175)
Effective Minimum Wage	-0.0376	0.0268	-0.268**	-0.276
	(0.105)	(0.0558)	(0.136)	(-0.276)
Massachusetts	-0.196**	-0.0235	0.0145	-0.205
	(0.0841)	(0.0551)	(0.172)	(0.205)
Post-*Goodrow* x Massachusetts	0.150	-0.142	0.313	0.321
	(0.197)	(0.107)	(0.337)	(0.395)
DDD Estimator (Treble to Double Damages)	0.284*	0.0712	-0.163	0.193
	(0.163)	(0.109)	(0.342)	(0.425)
Year Fixed Effects	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓
Ind./Occ./Sector FE	✓	✓	✓	✓
Post-*Goodrow* x State FE	✓	✓	✓	✓
Post-*Goodrow* x Ind./Occ./Sector FE	✓	✓	✓	✓
State x Ind./Occ./Sector FE	✓	✓	✓	✓
N	304,103	304,103	304,103	304,103
R^2	0.1470	0.0427	0.2190	0.2348

Robust standard errors clustered by each combination of state, industry, occupation, and sector are used in each model. See note for Table 1.7 for sample details. Significance: * 0.10 ** 0.05 *** 0.01

The magnitude of *Goodrow*'s estimated effect on each category of hours worked is now smaller than in the main results, and the estimated effect of Goodrow on fully compensated overtime hours worked is no longer statistically significant at any standard level of significance. However, the direction of all of the results is the same as in Table 1.7. The estimated effect of *Goodrow* on undercompensated overtime hours worked is still significant at the 10% significance level, and the estimated effect is approximately 79% of the magnitude of the result in Subsection B. Thus, all three robustness checks largely support the main finding that *Goodrow*'s reduction of penalties for overtime violations led employers to substitute away from non-overtime hours and towards both undercompensated and fully compensated overtime hours.

1.6 Discussion

This paper examined how damages for overtime violations affects labor demand and the classification of labor as either exempt or nonexempt from the overtime law. Based on a credibly exogenous reduction in penalties for overtime violations in Massachusetts in 2000, undercompensated overtime hours worked increased by as much as 23 minutes per week for workers covered by the Massachusetts and federal overtime law following the reduction in penalties for state overtime violations. The average number of fully compensated overtime hours worked may have increased by as much as 14 minutes per week, while the average number of non-overtime hours worked may have decreased by as much as 15 minutes per week for these workers. However, the estimated effect of the penalty reduction on non-overtime hours worked is not statistically distinguishable from zero at any standard significance level. I find little evidence that the reduction in penalties led employers to reclassify workers as being exempt from the Massachusetts overtime law, indicating that the estimated effects of the penalty reduction on hours worked are not contaminated by changing classifications of workers.

The results for undercompensated overtime hours worked and non-overtime hours worked are both consistent with economic theory, which predicts that a reduction in penalties should lead to a decrease in non-overtime hours worked and an increase in undercompensated overtime hours worked. However, the estimated increase in fully compensated overtime hours worked is not consistent with economic theory, which predicts that a reduction in penalties should lead employers to substitute away from fully compensated overtime hours and towards undercompensated overtime hours.

One possible explanation for this result is that employers have heterogeneous reactions to legal constraints. It is possible that, following a reduction in penalties for overtime violations, a subset of employers (Type A employers) substitute away from non-overtime and fully compensated overtime hours and towards undercompensated overtime hours. This causes non-overtime and fully compensated overtime hours worked to decrease, and undercompensated overtime hours worked to increase. There are some workers at other employers (Type B employers) who wish to work more hours and request to work overtime hours. Because workers at Type A employers are working more overtime hours, to remain competitive in the labor market the Type B employers also allow their employees to work overtime, and the Type B employers in fact pay the required overtime wage for these hours. This causes fully compensated overtime hours worked to increase overall, despite the drop in fully compensated overtime hours worked by workers at Type A employers.

Another possibility is that *Goodrow*'s reduction of damages for overtime violations led to increased overtime hours coupled with partial compliance with the overtime law. In recent decades researchers have investigated partial compliance with minimum wage laws, with researchers finding evidence of partial minimum wage compliance in South Africa (Bhorat et al. 2015). Researchers have suggested that a minimum wage law may cause a discriminatory employer to pay one favored group of workers the minimum wage, while paying a non-favored group a sub-minimum wage (Chang & Walia 2007). Likewise, after *Goodrow* employers may have scheduled workers to work more overtime hours, and the employers paid the required overtime wage for some of these workers but not for others. This may be the result of discriminatory preferences, or it may simply reflect a belief that the employer is less likely to be caught and punished if the employer complies with the overtime law for at least some workers. Future research should search for evidence on these hypotheses and assess what differences might exist among employers and among workers that may explain these findings.

Future research should also examine whether other penalties or procedural features may affect compliance with overtime laws. Galvin (2016) examined four features which may impact compliance with minimum wage laws: (1) treble damages, (2) civil/criminal penalties, (3) small claims process, and (4) post-judgment penalties. He found that only treble damages had a statistically significant impact on the incidence of minimum wage violations. However, future research should examine whether these or other features of overtime laws impact compliance with the overtime law or demand for overtime labor. Galvin (2016) also found that in the minimum wage context, state enforcement of the law affects compliance with the law. Future research should assess the extent to which increased enforcement of overtime laws drives employers' decisions to comply with the law.

This main results of this paper suggest that penalties for violating overtime laws may impact demand for overtime and non-overtime labor. Policymakers who are considering increasing damages or penalties for overtime violations should keep in mind that, as with any policy change, there are winners and losers. Workers who see an increase in hours as a result of the change (especially any workers who see an increase in fully compensated overtime hours worked) are the likely winners of such a change. Additionally, workers who continue to work overtime at the undercompensated wage and who bring a claim for enhanced damages against their employer will benefit from an increase in penalties. On the other hand, employees who see a cut in overtime hours worked and a reduction in total earnings will lose out from the change. Policymakers should take account of all of these anticipated costs and benefits—and which groups are likely to win and lose—when crafting changes to labor laws.

Chapter 2

Accountability Measures Matter: School Bond Measures, Home Prices, and Teacher Labor Market Outcomes in California

2.1 Introduction

In California, school bond measures are an important component in financing school construction and maintenance.[30] Bond measures in California can be approved at both the state and local level; at the state level, a simple majority of voters is required to approve a state general obligation bond, whereas at the local level, a supermajority of those residing within a school district is generally required to approve a bond measure.[31] Since 2001, voters can authorize a local bond measure with either 55% of the vote or a two-thirds supermajority; bond measures approved with a 55% threshold require greater accountability and transparency measures than those approved using the two-thirds threshold. In recent years, local bond funding

[30] Lunna Lopes & Iwunze Ugo, *Bonds for K-12 School Facilities in California*, PUBLIC POLICY INSTITUTE OF CALIFORNIA (May 2017), https://www.ppic.org/publication/bonds-for-k-12-school-facilities-in-california/.
[31] *Bond Measure*, EDSOURCE, https://edsource.org/glossary/bond-measure (last visited Aug. 30, 2021).

has greatly outpaced state bonds; from 2004 to 2016, state bonds provided $16.8 billion in K-12 school facilities funding, while local bond initiatives provided $91.1 billion in local funding.[32]

School bond measures have received increasing interest among academic researchers in recent years. Prior literature has examined the relationship between school bonds and student performance, finding that the passage of school bonds results in improved test scores (Hong & Zimmer 2016), especially for low-socioeconomic-status students (Rauscher 2020). Other research has found that passing school bond measures leads to large immediate increases in home prices, in part due to improvements in student achievement (Cellini et al. 2010). But there has been a general dearth of research examining how bond measures affect teacher labor market outcomes, nor has there been significant examination of the differences between bond measures approved using a 55% passage threshold, and those using a two-thirds passage threshold.

This paper examines two related questions. First, I investigate whether accountability measures for school bonds affect the hedonic value of the bond-financed capital improvements.[33] Second, I examine whether the passage of school bonds influences labor market outcomes, and whether the effect varies based on whether the bond measure was approved alongside enhanced accountability requirements. I examine the effect of bond measures on teacher salaries, the average education level of teachers, and average years of experience of teachers.

I first use a dynamic regression discontinuity (dynamic RD) approach based on Cellini et al. (2010)[34] to compare school districts where a bond measure passed with just above the required 55% of the vote with districts where a bond measure failed with slightly less than 55%

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[32] Lopes & Ugo, *supra* note 30.
[33] Much of the recent literature on the effect of school bond measures on home prices has drawn heavily upon Rosen (1974), Brueckner (1979), and Barrow & Rouse (2004). This line of research argues that the value of the public goods in an area, including public school expenditures, is reflected in the prices of homes in the area.
[34] This method is itself based on the standard regression discontinuity design (RDD) approach first introduced by Thistlethwaite & Campbell (1960).

of the vote. The dynamic RD approach has been favored over OLS because it may allow for more credible casual identification of the effect of school bonds on various outcomes. I then use OLS to estimate the effect of school bonds on these outcomes. There are at least two advantages of OLS over the dynamic RD approach. First, in OLS bond measures of different magnitudes can have different effects on the outcome of interest. Specifically, when the independent variable is the per-student amount of bonds passed, then OLS will estimate the effect of an additional dollar of school bonds passed, rather than simply the effect of a bond measure being approved. Second, OLS can treat bonds passed using a 55% passage separately from bonds passed using a two-thirds passage threshold, revealing any differential effects of these two types of bond measures.

This paper fits within the newly emerging literature that uses a regression discontinuity design to examine the impact of school bond measures on outcomes of interest. Rather than analyzing the effect of school bonds in general on home prices, I focus on differences between the effect of school bonds requiring a 55% passage threshold and those requiring a two-thirds passage threshold. This provides evidence on whether the accountability requirements associated with bond measures with a 55% passage threshold influence the hedonic value of the bonds to district residents, as measured by home values in the district. Ostensibly the revenue raised from these bond measures are for capital improvements, but the passage of these bonds may make revenue available for other purposes, such as teacher salaries or teacher hiring. I therefore also examine the impact of school bonds on labor market outcomes for teachers, which provides evidence on whether school bonds may lead to additional spending on items other than the programs that the bond measures are designed to fund.

Based on the dynamic RD approach, I find that bond measure passage increases teacher salaries and average teacher experience, but there is less evidence that bond measure passage affects home prices. Based on the OLS results, I find that home prices increase after voters approve school bonds that include enhanced accountability measures, but home prices decrease in the absence of the enhanced accountability requirements. Moreover, school bonds that are passed with enhanced accountability requirements are correlated with increases in teachers' average salary and average education level, while bonds that are passed without these accountability requirements are correlated with reductions in teacher salaries and the average education level of teachers. The results suggest that the enhanced accountability requirements for bonds passed using a 55% passage threshold influence the way the bonds are spent, to the apparent benefit of property owners and educators.

This paper makes at least two important contributions to the literature. First, the dynamic RD approach has been preferred to OLS in estimating the causal effect of school bond measures on home prices. But I provide evidence that OLS may be preferred to the dynamic RD approach because OLS allows for bond measures of different magnitudes to have different effects, and OLS allows for a better comparison of the effects of bond measures with different accountability requirements. The fact that the dynamic RD results are qualitatively similar to an analogous OLS model, but a more sophisticated OLS model provides qualitatively different results, suggests that the marginal value of more sophisticated modeling choices may exceed the marginal value of a more credible identification strategy in the context of school bond measures.

Second, in a break from the existing literature on the effect of school bonds on home prices and other outcomes, I focus on the differential effects of bond measures requiring 55% of the vote to pass and those requiring a two-thirds supermajority to pass. This provides important evidence on whether accountability and transparency requirements do make a difference in how money is spent. The finding that the additional accountability measures do result in higher home prices, higher teacher salaries, and higher teacher education levels has important implications for policymakers, who may seek to encourage additional accountability requirements in other self-governance processes to improve outcomes for taxpayers.

The remainder of this paper is organized as follows. Section II provides additional background on the structure of school bond measures in California and the role these measures play in financing school facilities in California. Section III describes the data and methods that I use to evaluate the relationship between school bond measures, home prices, and teacher labor market outcomes. Section IV provides the results, and Section V offers concluding remarks.

2.2 Background

To fully appreciate the important role that school bond measures play in local school funding in California, it is important to understand the history of local public school finance in the state. Prior to the 1970's, California's system of financing schools was based primarily on local property taxes.[35] While this system put California at the top of the country in terms of per pupil spending, this system gave an advantage to more affluent areas that had higher property values, and a disadvantage to less wealthy communities where property tax rates had to be significantly higher to fund schools at the same level as the wealthier communities.[36] In a line of 1970's cases known as *Serrano v. Priest*, the California Supreme Court found the state's property-tax-based school funding system unconstitutional and ordered the California

[35] *Landmark US Cases Related to Equality of Opportunity in K-12 Education*, STANFORD EQUALITY OF OPPORTUNITY AND EDUCATION, https://edeq.stanford.edu/sections/landmark-us-cases-related-equality-opportunity-education (last visited Aug. 30, 2021).
[36] Becky Jeffries, *California Public School Funding: A Brief History*, UKIAH DAILY JOURNAL, June 17, 2017, https://www.ukiahdailyjournal.com/2017/06/17/california-public-school-funding-a-brief-history/.

Legislature to equalize funding among school districts.[37] The core ruling in *Serrano* has survived multiple challenges since the 1970's and remains settled law. Today roughly 58% of California K-12 funding comes from state funds; 21% comes from local property taxes; 12% comes from other local sources; and the remainder comes from federal funding sources and the state lottery.[38]

With local property taxes no longer the primary source of school funding in California, school bond measures represent the primary source of revenue for construction and maintenance operations.[39] While these bond measures can be approved at both the state and local levels, the number of local bonds proposed has increased since 2000 and now greatly outpaces state bonds. Between 2004 and 2016, state bond measures provided $16.8 billion in funding for K-12 school facilities, whereas local bond measures provided $91.1 billion in funding.

One reason local bond measures have been more popular since 2000 is that California voters approved Proposition 39 that year, which lowered the share of votes needed to pass local bond measures from a two-thirds supermajority to a 55% supermajority. A district can use a 55% passage threshold only if three requirements are met.[40] First, the bonds can only be used for construction, rehabilitation, equipping of school facilities, or the acquisition or lease of real property for school facilities. Second, the measure must include a specific list of projects to be funded, along with certification that the school board considered safety, class size reduction, and information technology needs in developing the list. Third, the school board must conduct annual, independent audits to ensure that the bonds have been used only for the projects listed in

[37] *California*, EDUCATION LAW CENTER, https://edlawcenter.org/litigation/states/california.html (last visited Aug. 30, 2021).
[38] *Who Pays: Where California's Public School Funds Come From*, ED 100, https://ed100.org/lessons/whopays (last updated Jan. 2021).
[39] Lopes & Ugo, *supra* note 30.
[40] *Proposition 39*, LEGISLATIVE ANALYST'S OFFICE, https://lao.ca.gov/ballot/2000/39_11_2000.html (last accessed May 5, 2022).

the measure. Some districts continue to require a two-thirds supermajority for bond measure passage, possibly to bypass the additional accountability requirements.

School bond measures are popular. Of the 1,018 local bond initiatives on the ballot between 2004 and 2016, voters passed 83% of them, and since 1999 at least 60% of Californians said they would vote yes on a local bond for school construction projects.[41] The most recent statewide bond measure approved by voters was Proposition 51 in 2016, which authorized $7 billion in general obligation bonds for public school construction and modernization.

Figure 2.1 below illustrates the variation that exists in the amount of school bonds approved by districts across California. The panel on the left displays the histogram of the average amount of per-student school bonds approved by each school district from 1996 to 2020, excluding the 24.5% of districts in which no school bonds were approved during this period.[42] The panel on the right displays the histogram for the natural logarithm of this average, again omitting those districts that did not approve any school bonds during this period.

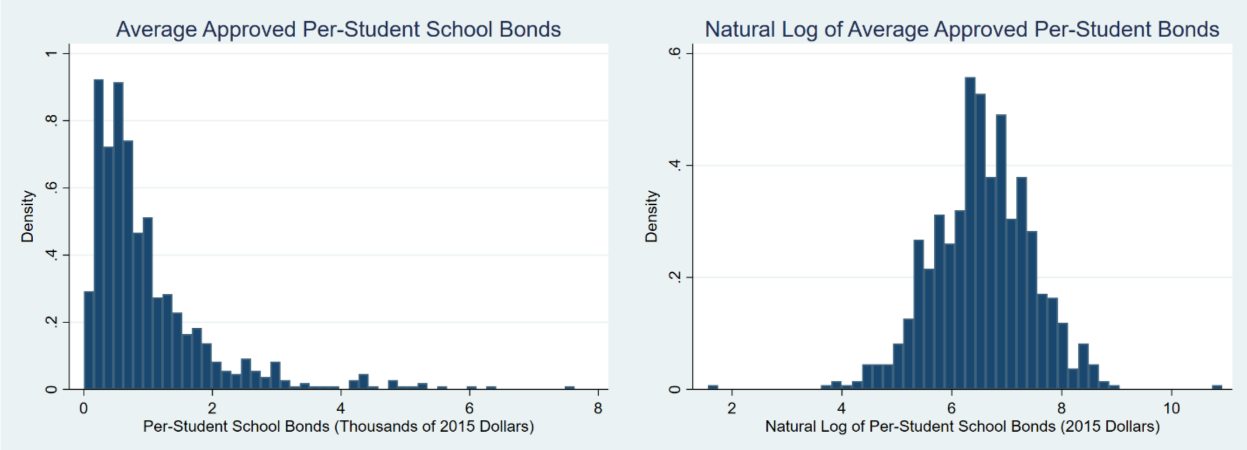

Figure 2.1: Histogram of Per-Student School Bonds Approved (1996 to 2020)

[41] Lopes & Ugo, *supra* note 30.
[42] Additionally, one district in which the average amount of per-student school bonds was $55,000 is omitted. This district is Minarets Joint Union High School District, a district that had 51 students in 1999 and which approved a bond measure in an amount not exceeding $11,920,000 the same year.

The average district approved roughly $992 per student in 2022 dollars in bonds each year, with the median district approving $630 per student in bonds each year. Overall, local bond measures provided more than $18.8 billion in funding for K-12 facilities in 2016 alone. Total state and local funding for public schools in California is approximately $13,400,[43] so roughly 7.4% of school funding comes from local bond measures for the average district.

These bond measures therefore provide a sizeable amount of local variation in school facility funding, even in light of the *Serrano* decision. To the extent that the *Serrano* cases sought to equalize educational outcomes between low-income and high-income school districts, school bond measures may undermine this policy by contributing to unequal educational outcomes between the districts that propose and approve bond measures and those that do not. And Proposition 39 might be seen as further encouraging these bond measures, as long as they contain enhanced accountability requirements. The role that these bond measures—and the enhanced accountability requirements associated with them—may play in funding teacher salaries and employment is therefore an important and understudied area of research.

2.3 Data and Methods

This paper uses data from three primary sources to evaluate how the passage of school bond measures affects home prices and labor market outcomes for teachers. First, I obtained data on school bond measure elections from the California Election Data Archive (CEDA).[44] This data contains complete ballot measure election results for California school district elections,

[43] Melanie Hanson, *U.S. Public Education Spending Statistics*, EDUCATION DATA INITIATIVE, https://educationdata.org/public-education-spending-statistics (last updated Mar. 15, 2022).
[44] *County, City, School District & Ballot Measure Election Results*, CALIFORNIA SECRETARY OF STATE, https://www.sos.ca.gov/elections/county-city-school-district-ballot-measure-election-results (last visited Aug. 30, 2021).

including the date of the election, the text of the ballot measure, the number of votes for and against the measure, and what percent of the vote the ballot needed to pass (the passage threshold). From the text of the ballot measures, I parsed out the total bond amount on the ballot.

I use the parsed bond amount for each school bond to determine the total amount of school bonds that were on the ballot for each school district in each year, as well as the amount of these bonds that had a 50% passage threshold, the amount that had a 55% passage threshold, and the amount that had a two-thirds passage threshold. I also determine the total amount of bonds that passed and the total amount that failed, including disaggregated by passage threshold.

Second, I obtained data on teacher compensation and employment[45] from the California Department of Education. Using the compensation and employment data, I am able to calculate three variables of interest for each school district and year: (1) average teacher salary, (2) average education level of teachers (column), and (3) average years of teaching experience for teachers (step). I also use student enrollment data to calculate the per-student amount of bonds that were on the ballot for each election, and the per-student amount of bonds that passed.

Third, I obtained data on typical home values from the online real estate marketplace Zillow.[46] I use Zillow's data from the ZHVI Single-Family Home Time Series to collect the typical home price for each school district in California for each year from 2000 to 2020. Using data from the California Department of Education, I identify all of the zip codes containing an active school for each school district in each year. I then calculate the average typical home price across all of the zip codes containing a school in the district. I use this average price as the

[45] *Certificated Salaries & Benefits*, CALIFORNIA DEPARTMENT OF EDUCATION, https://www.cde.ca.gov/ds/fd/cs/ (last updated December 26, 2019). I also obtain student enrollment data from the California Department of Education. *Enrollment by School*, CALIFORNIA DEPARTMENT OF EDUCATION, https://www.cde.ca.gov/ds/sd/sd/filesenr.asp (last updated April 15, 2020).

[46] *Housing Data*, ZILLOW, https://www.zillow.com/research/data/ (last visited Aug. 30, 2021).

average home price for each district in each year. Finally, I obtained data on unemployment rates for each county and year from the California Employment Development Department (EDD).[47]

My research framework is as follows. First, consistent with the prior literature on school bonds, I examine the effect of school bond passage on typical home prices using a dynamic regression discontinuity (dynamic RD) approach. The advantage of the dynamic RD approach is that it provides credible identification of the effect of school bond passage on home prices. However, the causal identification is based on bond measures for which the percent of voters voting in favor of the bond measure was close to[48] the passage threshold. It is thus based on a smaller number of observations than an OLS framework would be based on.

I restrict attention to proposed bond measures that had a passage threshold of 55%, and I control for the proposal of other bond measures up to eight years in the past. The econometric specification using this RDD approach is:

$$\begin{aligned} Y_{i,t+m} &= \tau * I\{Vote\,\%_t \geq 55\%\} + \beta_1 * (Vote\,\%_t - 55\%) + \beta_2 * I\{Vote\,\%_t \geq 55\%\} * (Vote\,\%_t - 55\%) + \\ &\sum_{j=0}^{j=8} \psi_{t+m-j} * I\{\geq 1\ Other\ Bond\ Measure\ on\ Ballot_{t+m-j}\} + \sum_k \eta_k * County_k + \sum_t \phi_t * Year_t + \epsilon_{i,t} \end{aligned} \tag{1}$$

There are two sets of regressions, with the variable Y representing the outcome of interest (either typical home price or the natural logarithm of typical home price). The unit of observation is school district i in year t. For each outcome, I run eleven regressions indexed by m: one for the current value of Y, along with two years of lags and eight years of leads. Thus m takes integer values from -2 to 8. The running variable is the percent of the vote received by bond measures with a 55% passage threshold in school district i in year t. The discontinuity occurs at 55%, where the bond measure passes. Each β coefficient estimates the effect of the running variable on

[47] *Employment Development Department*, LABOR MARKET INFORMATION BY COUNTY, https://www.labormarketinfo.edd.ca.gov/geography/lmi-by-county.html (last visited Aug. 30, 2021).
[48] As explained below, I use a bandwidth around the passage threshold of twenty percentage points (45% to 65%).

the dependent variable below and above the 55% cutoff, while τ represents the estimated effect of voters approving a bond measure with a 55% passage threshold. Fixed effects are included for each county and year, and ε is the error term.

As noted by many recent analyses of school bond measures, many school districts that fail to pass a bond measure will try again in a future year. Therefore, a standard RD approach will misestimate the effect of bond measures on outcomes of interest. Thus, in this dynamic RD approach I control for whether any other bond measure (other than the bond measure being examined) was proposed each year in the previous eight years.

I use a one-degree polynomial for the running variable; this follows Gelman & Imbens (2018), who recommend against using high-order polynomials for the running variable. I use a Epanechnikov kernel to construct the local-polynomial estimators. To construct the dynamic RD point estimator, I use a bandwidth of 20 percentage points, with 10 percentage points below the 55% cutoff and 10 percentage points above the cutoff.[49]

Next, I examine the effect of school bond passage on typical home prices using OLS. I regress each home price outcome against a set of dummy variables that code for bond passage in each year for each district. The econometric specification is:

$$Y_{i,t} = \sum_{j=-2}^{j=8} \gamma_{t-j} * I\{Bond\ Measure\ Passed_{t-j}\} + \sum_{k} \eta_k * District_k + \sum_{t} \phi_t * Year_t + \lambda * X_{i,t} + \epsilon_{i,t} \qquad (2)$$

Each dummy variable (one for the current year, along with two years of leads and eight years of lags) is equal to 1 if there was at least one bond measure passed in that school district in that year, and 0 otherwise. The γ coefficients represent the estimated effect of bond passage on the

[49] The relatively large bandwidths are necessary given the relatively low number of bond measures in the dataset.

outcome Y. Fixed effects are included for each district and year, X controls for the unemployment rate in the county containing the district, and ε is again the error term.

I then make two adjustments to Equation (2). First, rather than using a dummy variable that codes for bond passage as the independent variable of interest, I use the per-student bond amount approved. Second, I separate the bond amount by passage threshold, which allows for bonds passed with a 55% passage threshold to have a different effect than bonds passed with a two-thirds passage threshold. The econometric specification is now:

$$Y_{i,t} = \sum_{j=-2}^{j=8} \alpha_{t-j} * 50\%\ Bonds\ Passed_{i,t-j} + \sum_{j=-2}^{j=8} \beta_{t-j} * 55\%\ Bonds\ Passed_{i,t-j} + \sum_{j=-2}^{j=8} \gamma_{t-j} * 2/3\ Bonds\ Passed_{i,t-j} + \sum_{i} \eta_i * District_i + \sum_{t} \phi_t * Year_t + \lambda * X_{i,t} + \epsilon_{i,t} \tag{3}$$

Now the α coefficients represent the effect of per-student bonds passed using a 50% passage threshold on the outcome of interest. The β coefficients represent the effect of bonds passed using a 55% passage threshold, while the γ coefficients represent the effect of bonds passed using a two-thirds passage threshold.

After examining how the approval of school bonds affects home prices, I use the same approach to assess how the approval of school bonds affects three labor market outcomes for teachers. These outcomes are teacher salaries, average teacher education level, and average years of teaching experience. I begin with the dynamic RD approach using Equation (1) before moving on to the OLS models given in Equations (2) and (3).

A higher education level in a district signals one or both of the following. First, the district may be hiring more highly educated teachers. This may occur if the district specifically prioritizes these teachers because the district perceives that these teachers are more qualified and capable of teaching students. Alternatively, it may reflect greater selectivity in hiring teachers.

For example, if the district receives significantly more applications per opening, the district may prioritize applicants based on education level, even if the district is not explicitly seeking more highly educated teachers. Second, it may indicate that the district is encouraging existing teachers to obtain more units of education to improve their teaching ability. Likewise, greater average years of experience in a district reflects either that the district is hiring teachers with more teaching experience, or teachers are staying employed with the district longer.

2.4 Results

Before examining the relationship between school bonds, school board elections, and teacher labor market outcomes, I provide some context on these variables. I first document some empirical patterns in proposed and approved school bonds. Figure 2.2 below displays the number of school districts that had school bond measures proposed and approved, and the total school bonds proposed and approved from 1996 to 2020:

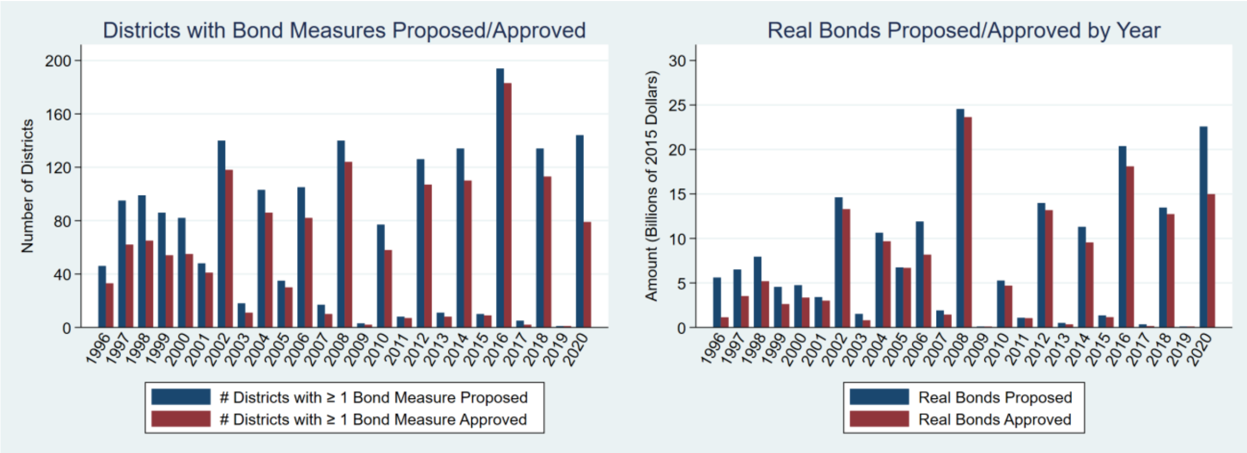

Figure 2.2: Bond Measures and School Bonds Proposed and Approved

Bond measures are far more popular in even-numbered years than odd-numbered years, especially since 2002. On average from 2011 to 2020 there were seven districts with bond measures each odd-numbered year, compared to 146 districts each even-numbered year. The number of districts with bond measures was highest in 2016 when there were 194 districts with bond measures. On average there are 74 districts with at least one bond measure in a given year (out of an average of 956 districts in the dataset). An average of 58 districts approved a bond measure each year, which is roughly 78% of the districts with a bond measure on the ballot. Roughly 81% of proposed school bonds are approved by voters, with a high of $23.6 billion (2015 dollars) in school bonds approved in 2008. On average, roughly $7.8 billion in real school bonds are on the ballot each year, with an average of $6.3 billion being approved by voters.

Figure 2.3 below displays the total school bonds proposed and the total school bonds approved by year, disaggregated by the measure's passage threshold. Prior to 2001, all school bonds required a two-thirds supermajority of approval to pass. In November of 2000, California voters approved Proposition 39, which reduced the passage threshold from two-thirds to 55% if the district agreed to enhanced transparency and accountability requirements related to the bonds. Since then, almost all bonds proposed and approved have used a 55% passage threshold.

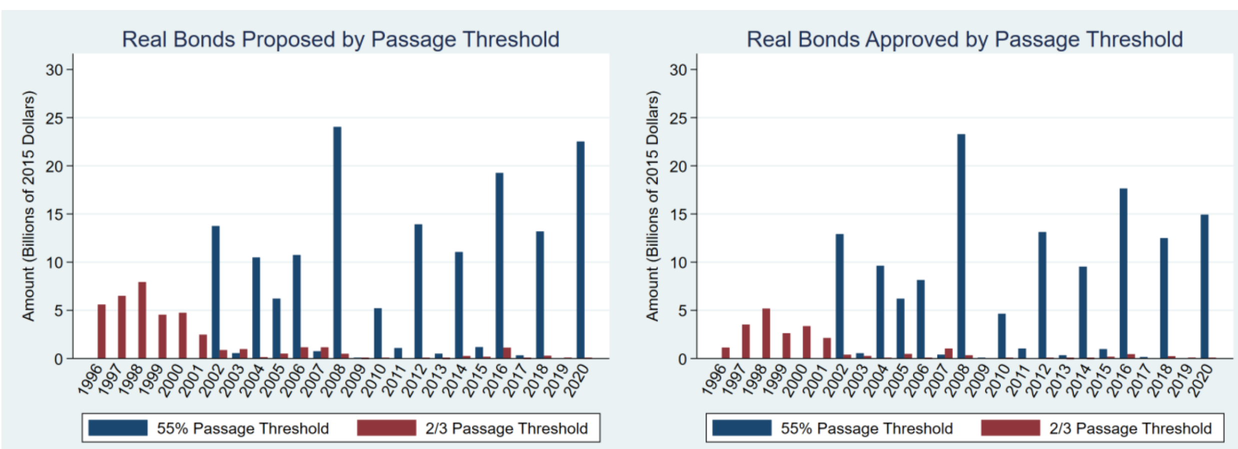

Figure 2.3: School Bonds Proposed and Approved by Passage Threshold

Comparing Figures 2.2 and 2.3, the amount of bonds proposed and approved mostly tracks the amount of districts with bond measures on the ballot and the amount of districts approving a bond measure. The inflation-adjusted amount of approved bonds has mostly increased from 1996 to 2008, fell drastically in 2010, and has been on a generally upward trajectory since then.

Next, I document trends in home prices by school district. Figure 2.4 below displays the average real home price of California school districts by year. The average home price for a district is calculated based on Zillow's typical home price in each zip code that contains an active school in the district. The average home price is the average of these typical home prices, and the average real home price is adjusted for inflation using 2015 dollars.

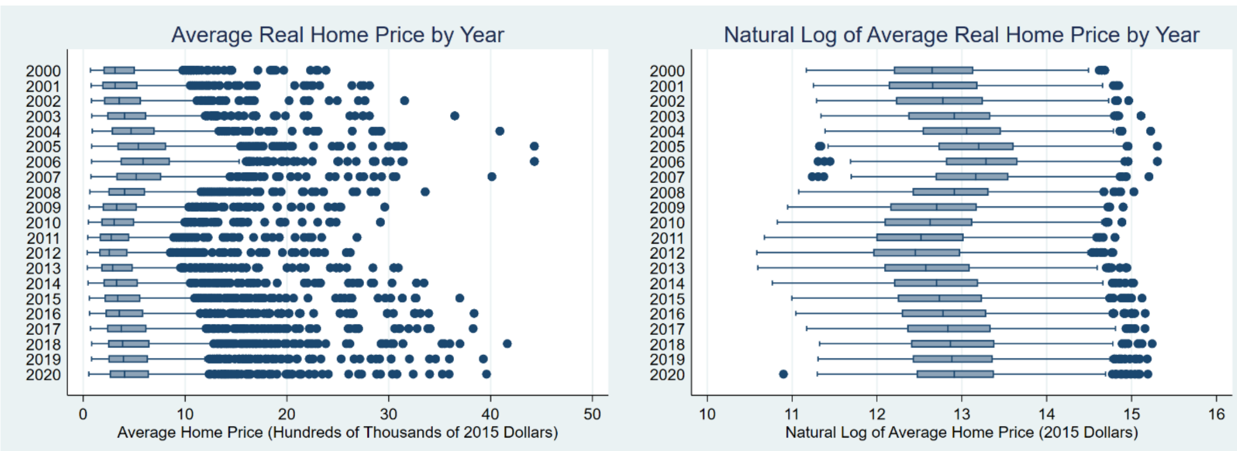

Figure 2.4: Typical Home Price by Year

The average typical home price has increased from $412,000 (2015 dollars) in 2000 to $689,000 in 2006, before falling to $365,000 in 2012. Since then the average typical home price has increased to $571,000. But the distribution of home prices is clearly right-skewed, with many outlier districts with typical home prices above $2 million. Taking the natural logarithm of home prices thus provides a more readable illustration of the evolution of home prices over time. Home prices clearly peaked in 2006, fell until 2012, and have since been recovering.

In assessing the impact of approved school bond measures and school bonds on home prices, some potential confounding factors come to mind. First, it seems likely that the same economic conditions that influence home prices are influencing the proposal and approval of school bond measures. Real approved school bonds peaked in 2008, which is the year that saw the largest drop in real home prices during the period. Voters may have supported the bond measures that year as a stimulus in response to the economic downturn that began in 2008. And if local economic conditions also impact bond measure approval, then controlling for year would not eliminate the confounding effect of economic conditions. If this is the case, then OLS would likely underestimate the effect of school bond approval on home prices, and a more credible causal identification strategy (like the dynamic RD approach) would be advantageous over OLS.

Third, I document patterns in teacher labor market outcomes. Figure 2.5 below shows the evolution of salary, teacher education (column), and years of experience over time.

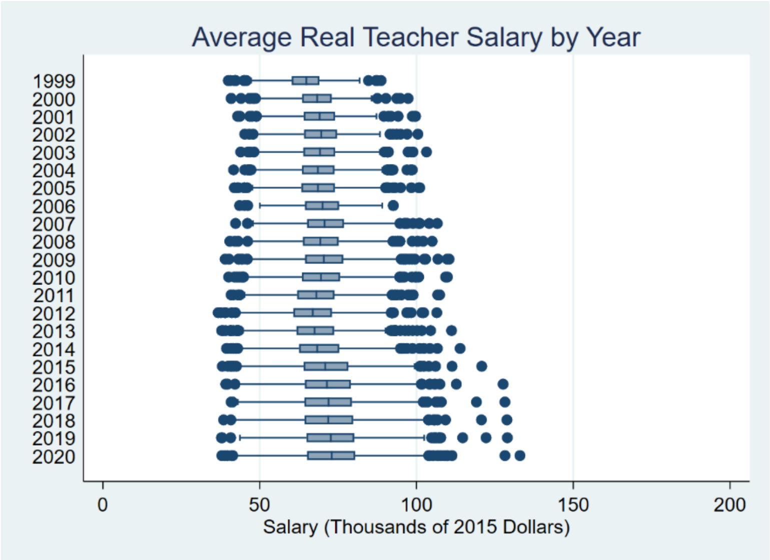

Figure 2.5(a): Real Teacher Salary by Year

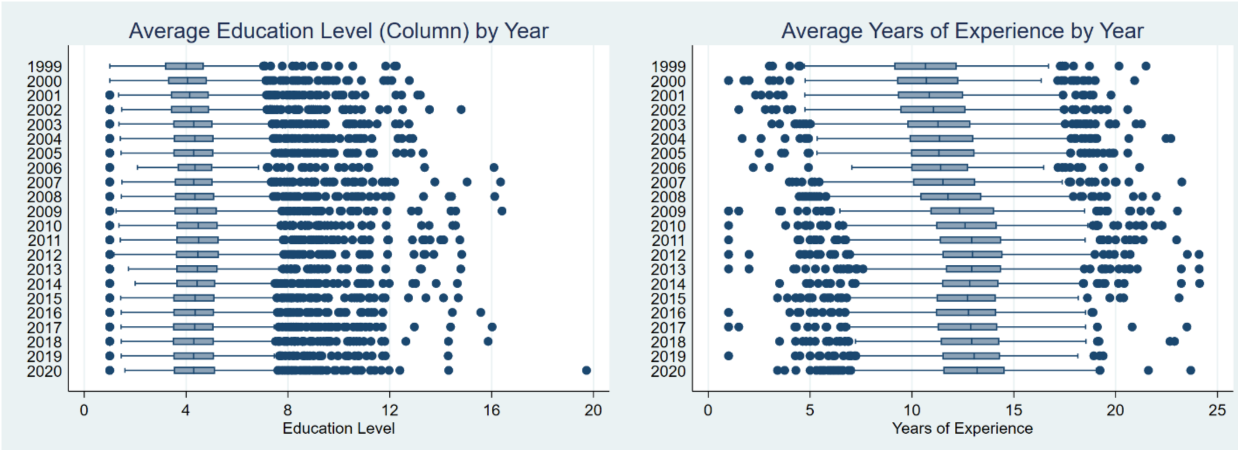

Figure 2.5(b): Teacher Education and Experience by Year

Real salaries have mostly increased over the period, climbing from $64,500 (2015 dollars) per year in 1999 to $73,000 per year in 2020. Meanwhile, average teacher column (measuring the education level of teachers) ranged from a low of 4.06 in 1999 to a high of 4.71 in 2011, while average teacher experience varied from a low of 10.7 years of experience in 1999 to a high of 12.9 years of experience in 2020. Real teacher salaries reached relative low points from 2011 to 2013 in the wake of the Great Recession. But this was also a period where there were significant bond-financed projects under construction, driven in large part by the $23.6 billion in school bonds that were approved in 2008. A naïve observer might therefore conclude that bond measures reduce teacher salaries. But a robust analysis that controls for macroeconomic-related changes in average teacher salary from year to year might produce different inferences about the effect of school bond measures on teacher salaries.

The three labor market outcomes above appear to have less variation over time, relative to approved school bonds and home prices. But there is still evidence that average salaries decreased surrounding the Great Recession and its aftermath. In regressing teacher salaries against approved school bonds, including year fixed effects may control for statewide economic conditions that may affect this variable. But local economic conditions that may affect all of these variables may still result in bias in the coefficient estimates. Thus, a dynamic RD approach may provide more credible causal identification of the effect of school bonds on these labor market outcomes.

2.4.1 Home Prices

In this section I examine the effect of school bond measures on home prices. The analysis builds upon Cellini et al. (2010), who use a dynamic RD approach to find that home prices increase following the passage of school bond measures. I begin with a dynamic RD approach before moving on to two OLS frameworks. In the first OLS framework, the treatment variable is whether a bond measure was approved. In the second OLS framework, the treatment variable is the real dollar value of school bonds that were approved.

This analysis differs from Cellini et al. because I restrict attention to bond measures with a 55% passage threshold. Thus, all of these bond measures have enhanced accountability measures compared to those with a two-thirds passage threshold. Because bond measures with a two-thirds passage threshold became rare after 2001, the sample size of these measures is too low to conduct meaningful analysis using the dynamic RD framework. In contrast, Cellini et al. primarily focus on bond measures with a two-thirds passage threshold, because districts did not gain the ability to use a 55% passage threshold for bond measures until 2001.

Table 2.1 below plots the RDD estimates for the effect of bond measure passage on typical home price, and the natural logarithm of typical home price. Figure 2.6 below plots the two dependent variables against the percentage of the electorate voting for the school bond measure, with the discontinuity at the 55% passage threshold representing the estimated effect of passage. Figure 2.6(a) plots typical home price one year to eight years after the election, while

Figure 2.6(b) plots the same graphs for the natural logarithm of typical home price. Finally, Figure 2.7 plots the coefficient estimates from Table 2.1 to show the effect of bond passage on home prices over time.

Table 2.1: Effect of Bond Measure Passage on Home Prices (Dynamic RD)

	Dependent variable:	
	Typical Home Price ($1000s)	Natural Log of Typical Home Price
Bond Passed—2 Year Lead	-88.1*	-0.0876
	(53.4)	(0.0721)
Bond Passed—1 Year Lead	-86.2	-0.0772
	(54.4)	(0.0690)
Bond Passed	-95.6*	-0.0807
	(56.3)	(0.0654)
Bond Passed—1 Year Lag	-53.9	-0.0570
	(72.6)	(0.0837)
Bond Passed—2 Year Lag	-14.1	-0.0336
	(73.2)	(0.0846)
Bond Passed—3 Year Lag	-97.8	-0.120
	(82.8)	(0.0972)
Bond Passed—4 Year Lag	-104	-0.123
	(85.1)	(0.0982)
Bond Passed—5 Year Lag	-71.5	-0.110
	(87.0)	(0.0973)
Bond Passed—6 Year Lag	-61.6	-0.0924
	(91.3)	(0.0985)
Bond Passed—7 Year Lag	-78.2	-0.0819
	(80.5)	(0.111)
Bond Passed—8 Year Lag	-69.7	-0.0557
	(82.2)	(0.103)
Mean Value	565	13.0
Avg. Obs. to the Left of 55%	123	123
Avg. Obs. to the Right of 55%	755	755

This table displays coefficient estimates for the effect of bond measure passage on home prices from the dynamic RD framework. Eleven separate equations are estimated (one for each lead, the current value, and the lag of the dependent variable). County and year fixed effects are included, and the controls are indicator variables for whether at least one other school bond measure was proposed each year in the previous eight years. Significance: * 0.10 ** 0.05 *** 0.01

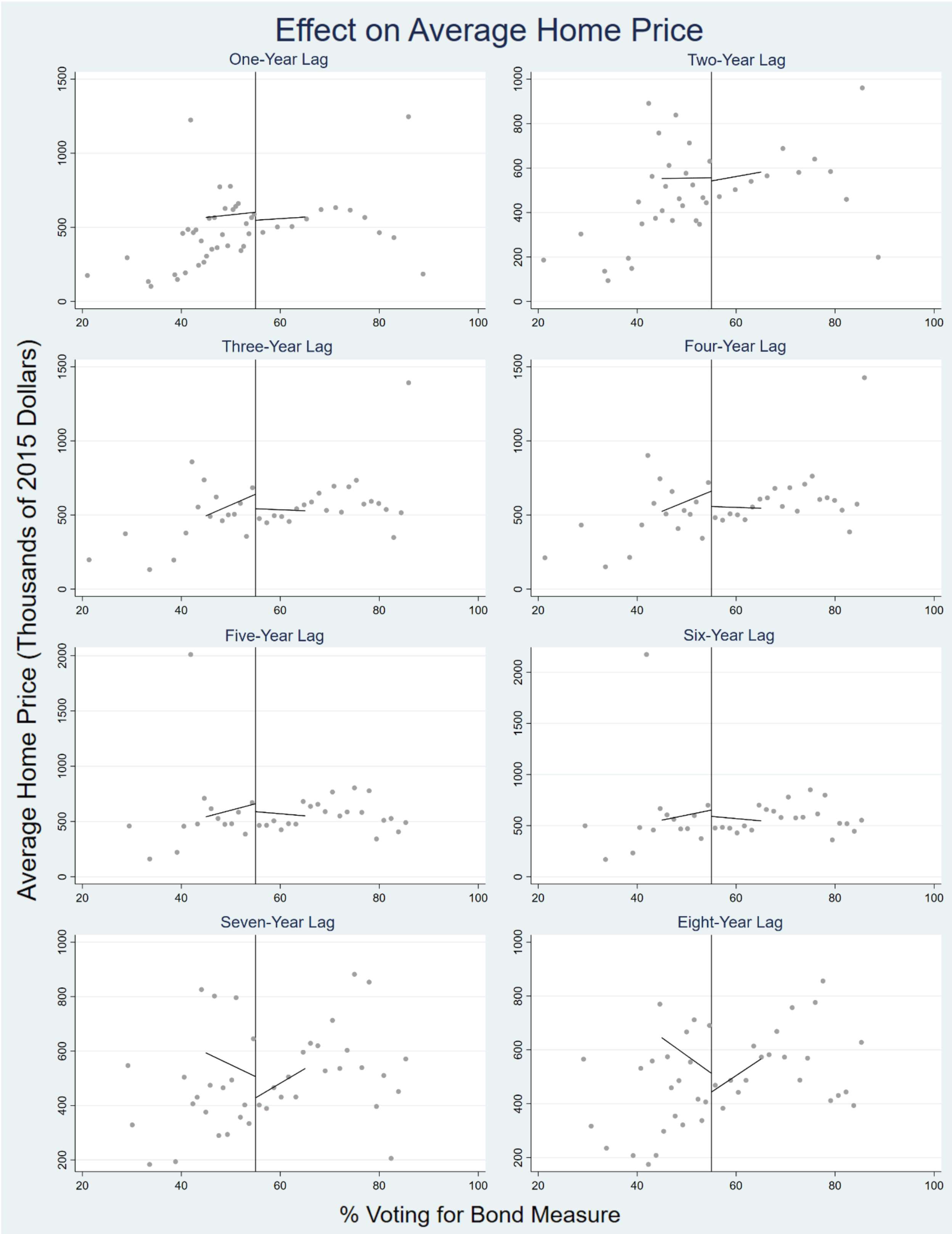

Figure 2.6(a): Typical Home Price (Dynamic RD)

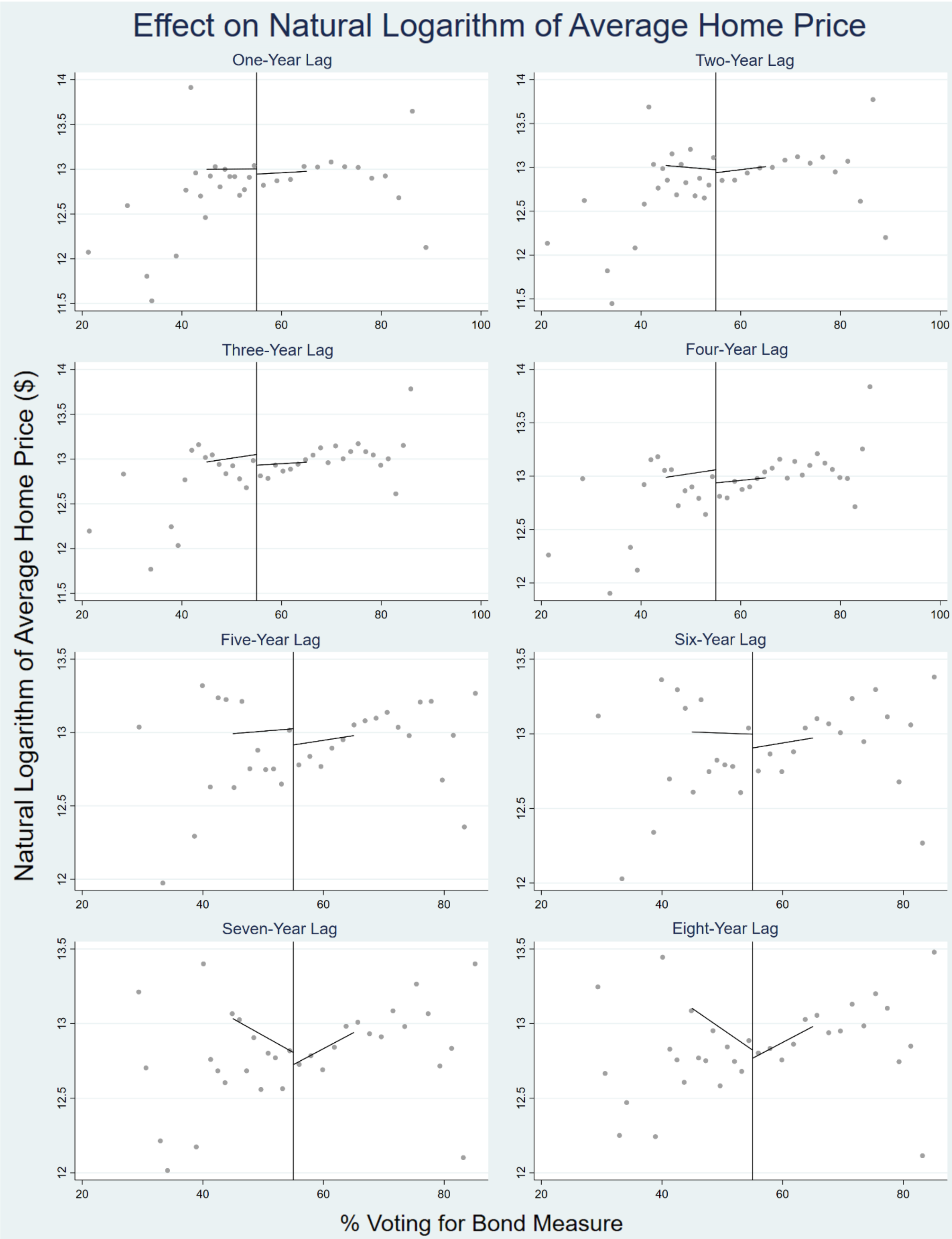

Figure 2.6(b): Natural Log of Typical Home Price (Dynamic RD)

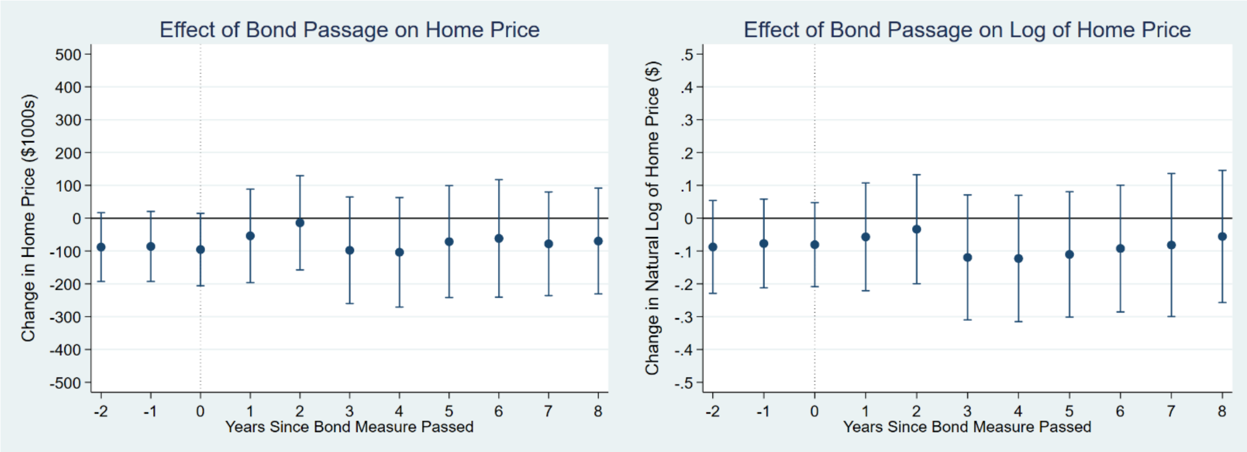

Figure 2.7: Effect of Bond Passage on Home Prices (Dynamic RD)

Overall, the dynamic RD approach does not provide clear evidence that bond measure passage affects home prices within eight years of passage. Based on the coefficient estimates from the first specification (using typical home price as the dependent variable), bond measure passage leads to a decrease in home prices of $68,800 on average in the eight years after passage. Based on the second specification, bond measure passage leads to a decrease in home prices of 8.03%. All of the coefficient estimates in both specifications are negative, but none are statistically different from zero. Moreover, the average of the coefficient estimates in the two years before passage are of similar magnitude to the coefficient estimates after passage. And looking to Figure 2.6, the effect of bond measure passage is unclear. So the dynamic RD approach provides only ambiguous evidence that passage of a bond measure decreases home prices.

Next, I use OLS to evaluate how approval of school bonds affects home prices. Table 2.2 displays the OLS estimates for the effect of bond measure passage on typical home price and the natural logarithm of typical home price, using models based on Equation (2). Figure 2.8 plots the coefficient estimates from Table 2.2 to show the effect of bond passage on home prices over time.

Table 2.2: Effect of Bond Measure Passage on Home Prices (OLS)

	Dependent variable:	
	Typical Home Price ($1000s)	Natural Log of Typical Home Price
Bond Passed—2 Year Lead	0.289	0.00416
	(4.09)	(0.00916)
Bond Passed—1 Year Lead	-1.66	0.00695
	(3.88)	(0.00898)
Bond Passed	3.05	0.00803
	(6.48)	(0.00943)
Bond Passed—1 Year Lag	-3.20	0.00613
	(5.94)	(0.00773)
Bond Passed—2 Year Lag	1.72	0.0121
	(6.88)	(0.00758)
Bond Passed—3 Year Lag	5.99	0.0152*
	(9.20)	(0.00768)
Bond Passed—4 Year Lag	8.30	0.0106
	(11.2)	(0.00807)
Bond Passed—5 Year Lag	8.26	0.00613
	(8.76)	(0.00692)
Bond Passed—6 Year Lag	5.37	0.000328
	(7.48)	(0.00640)
Bond Passed—7 Year Lag	4.58	-0.00723
	(5.56)	(0.00591)
Bond Passed—8 Year Lag	5.53	-0.0111**
	(5.69)	(0.00524)
Mean Value	504	12.9
District Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
N	12,251	12,251
Adjusted R^2	0.9584	0.9733

This table displays coefficient estimates for the effect of bond measure passage on home prices from the OLS framework. County and year fixed effects are included, and the unemployment rate for the county containing district i in year t is included as a control. Significance: * 0.10 ** 0.05 *** 0.01

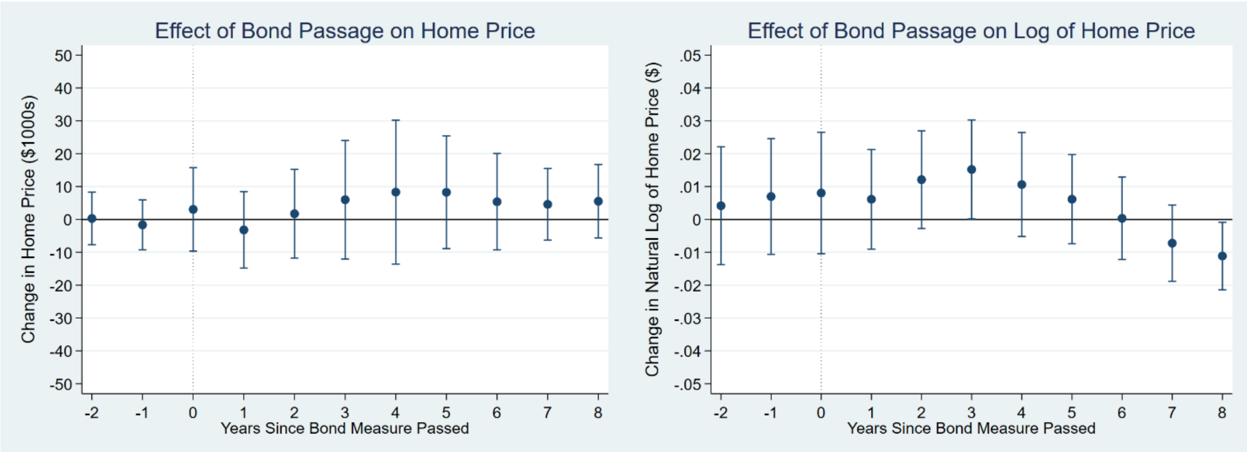

Figure 2.8: Effect of Bond Passage on Home Prices (OLS)

Under the OLS specifications, the effect of bond passage on home prices remains unclear. The first specification suggests that bond measure passage is correlated with an increase in home prices of $4,570 on average in the eight years after passage. In the second specification, it appears that home prices initially increase by roughly 0.846% on average in the six years after passage, but home prices then decrease by 0.721% and 1.11% seven and eight years after passage, respectively. But only the increase three years after passage and the decrease eight years after passage are statistically significant at any standard significance level (10% and 5% respectively). So like the dynamic RD approach, the OLS models provide only ambiguous evidence that school bonds have any effect on home prices within eight years of passage.

Finally, I estimate two models based on Equation (3). In these models, the dependent variable is the per-student amount of school bonds passed (along with two years of leads and eight years of lags). The bonds are separated by passage threshold, so that bonds passed using a 55% passage threshold can have different coefficient estimates from those passed using a two-thirds passage threshold. Table 2.3 displays the OLS estimates for the effect of per-student bonds passed on home prices, using models based on Equation (3). Figure 2.9 plots the coefficient estimates from Table 2.3 to show the effect of bond passage on home prices over time.

Now there is some evidence that home prices increase following passage of school bonds under a 55% passage threshold, home prices decrease following passage of bonds under a two-thirds passage threshold. Under the first specification, a $1000-per-student increase in school bonds passed using a 55% passage threshold is followed by an increase in home prices of $843 on average over the following eight years. Meanwhile, a $1000-per-student increase in school bonds passed using a two-thirds passage threshold is followed by a decrease in home prices of

$369 on average over the following eight years. However, these results are not statistically significant under any standard significance level.

Table 2.3: Effect of Bonds Passed ($1000s Per Student) on Home Prices

	Dependent variable:			
	Typical Home Price ($1000s)		Natural Log of Typical Home Price	
	55% Threshold	2/3 Threshold	55% Threshold	2/3 Threshold
Bonds Passed—2 Year Lead	0.885	-0.213	0.000938**	-0.00113
	(0.632)	(0.503)	(0.000450)	(0.00132)
Bonds Passed—1 Year Lead	0.940	0.170	0.000843	-0.00101
	(0.709)	(0.430)	(0.000512)	(0.00155)
Bonds Passed	1.53	-0.126	0.000855	-0.00132
	(0.936)	(0.447)	(0.000593)	(0.00162)
Bonds Passed—1 Year Lag	0.411	-0.503	0.000565	-0.00152
	(0.585)	(0.586)	(0.000553)	(0.00159)
Bonds Passed—2 Year Lag	0.628	-0.666	0.000684	-0.000363
	(0.656)	(0.862)	(0.000583)	(0.00131)
Bonds Passed—3 Year Lag	0.650	-0.904	0.000982*	-0.00117
	(0.847)	(0.864)	(0.000559)	(0.00139)
Bonds Passed—4 Year Lag	1.03	-0.0614	0.00122*	-0.00285**
	(1.11)	(0.656)	(0.000675)	(0.00125)
Bonds Passed—5 Year Lag	0.876	-0.229	0.00160**	-0.00348***
	(0.819)	(0.809)	(0.000612)	(0.00129)
Bonds Passed—6 Year Lag	0.992	-0.373	0.00191***	-0.00393***
	(0.907)	(1.15)	(0.000665)	(0.00116)
Bonds Passed—7 Year Lag	0.852	0.0827	0.00178***	-0.00451***
	(0.765)	(0.867)	(0.000631)	(0.00123)
Bonds Passed—8 Year Lag	1.30	-0.297	0.00185***	-0.00497***
	(0.786)	(1.02)	(0.000633)	(0.00139)
Mean Value	504		12.9	
District Fixed Effects	✓		✓	
Year Fixed Effects	✓		✓	
N	11,850		11,850	
Adjusted R^2	0.9593		0.9742	

This table displays coefficient estimates for the effect of school bonds passed on home prices, with school bonds separated by passage threshold. County and year fixed effects are included, and the unemployment rate for the county containing district i in year t is included as a control. Significance: * 0.10 ** 0.05 *** 0.01

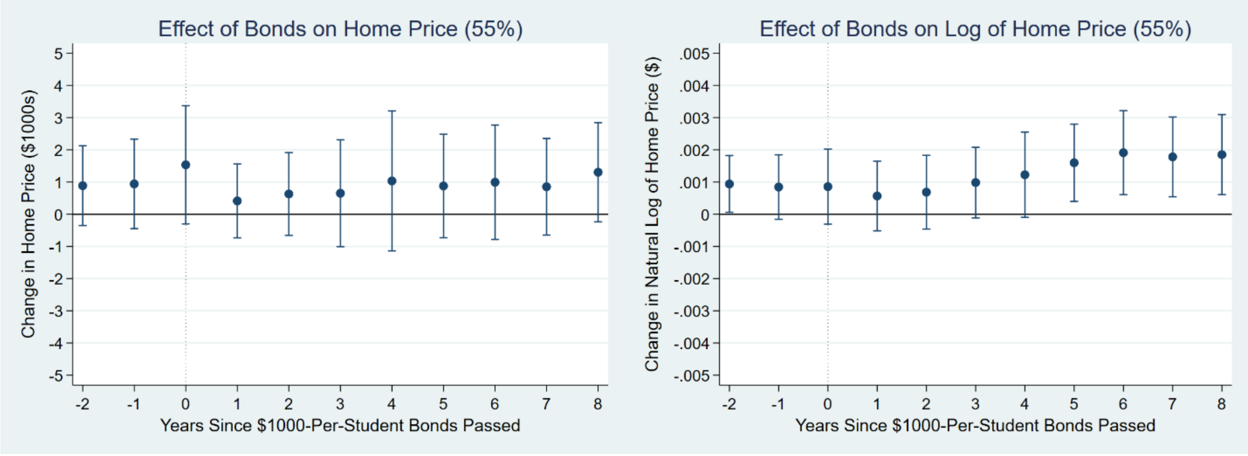

Figure 2.9(a): Effect of School Bonds on Home Prices (55%)

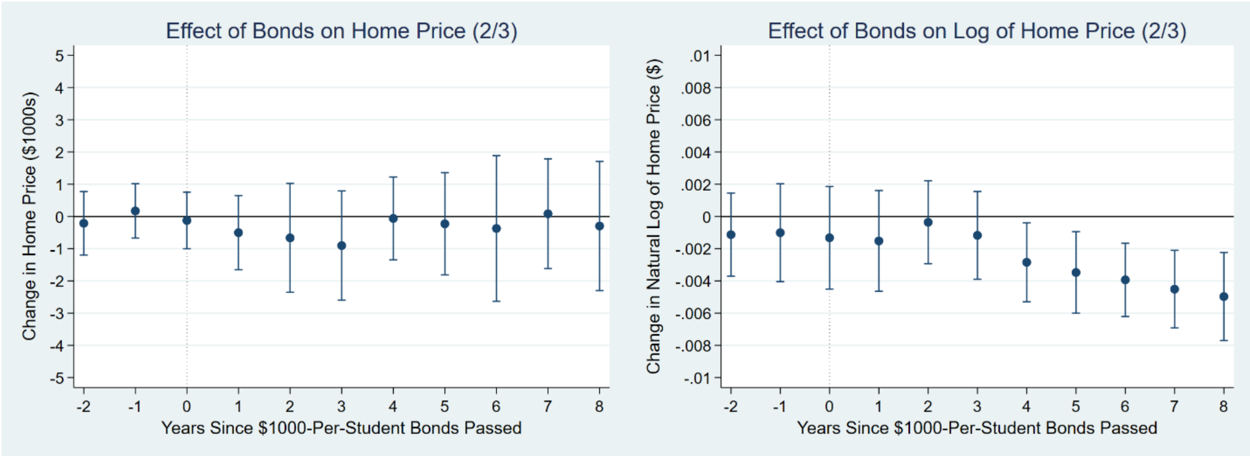

Figure 2.9(b): Effect of School Bonds on Home Prices (2/3)

Looking to the second specification, a \$1000-per-student increase in school bonds passed using a 55% passage threshold is followed by an increase in home prices of 0.132% on average over the following eight years. Meanwhile, a \$1000-per-student increase in school bonds passed using a two-thirds passage threshold is followed by a decrease in home prices of 0.284% on average over the following eight years. Six of the estimates for bonds passed using a 55% passage threshold are statistically significant at the 10% significance level or better, and five of the estimates for bonds passed using a two-thirds passage threshold are statistically significant at the 5% significance level or better.

However, in the third specification (regressing log home prices against bond measures passed using a 55% passage threshold), the coefficient estimate on the two-year lead is positive and statistically significant at the 5% significance level. To be sure, this may be the result of statistical noise. None of the coefficient estimates for the other eight leads in Table 2.3 are statistically significant at any level of significance, and it is certainly possible that one out of eight leads will have a statistically significant coefficient estimate when the true effect on the lead is actually zero. But it could also be a sign that home price increases precede approval of school bonds. For example, when wealthy residents move into a neighborhood, home prices may increase while those new residents are more likely to vote to approve bond measures than their predecessors in the neighborhood. Alternatively, as home prices rise for other reasons, residents in an area may have a higher sense of wealth, and they may be more likely to approve bond measures. Given the positive and statistically significant estimated effect of bond passage on the two-year lead of log home prices, some caution is warranted in interpreting the positive estimated effects of bond passage on the current and future values of log home prices.

One puzzling result is that Table 2.2 suggests that bond measure passage is followed by a reduction in home prices after several years, while Table 2.3 suggests that school bonds passed using a 55% passage threshold (the most common category of bond measures) are followed by an increase in home prices. One explanation for this is that the negative effect estimated in Table 2.2 is being driven by the passage of bond measures with a two-thirds passage threshold, and that even though this is a relatively small share of bond measures, it is enough to drive the apparently conflicting results. And in fact when Table 2.2 is re-estimated using models that separate the two types of bond measures, the results are more consistent with Table 2.3. Table 2.4 below displays the regression results for these models (regressing home prices and log home prices against indicator variables coding for bond passage, separated by passage threshold).

Table 2.4: Effect of Bond Measure Passage on Home Prices (Separated by Passage Threshold)

	Dependent variable:			
	Typical Home Price ($1000s)		Natural Log of Typical Home Price	
	55% Threshold	2/3 Threshold	55% Threshold	2/3 Threshold
Bonds Passed—2 Year Lead	0.992	1.29	0.00773	-0.00329
	(4.74)	(14.6)	(0.00929)	(0.0324)
Bonds Passed—1 Year Lead	-0.943	6.06	0.0105	0.0209
	(4.43)	(15.6)	(0.00901)	(0.0318)
Bonds Passed	3.98	-3.89	0.0114	0.0234
	(7.41)	(14.0)	(0.00969)	(0.0287)
Bonds Passed—1 Year Lag	-1.86	-12.8	0.00953	0.0230
	(6.36)	(15.1)	(0.00822)	(0.0339)
Bonds Passed—2 Year Lag	3.15	-8.49	0.0147*	0.0389
	(7.63)	(19.2)	(0.00823)	(0.0304)
Bonds Passed—3 Year Lag	9.32	-3.11	0.0175*	0.0314
	(12.0)	(19.2)	(0.0100)	(0.0312)
Bonds Passed—4 Year Lag	13.1	-5.59	0.0169	-0.0107
	(13.7)	(14.9)	(0.0105)	(0.0300)
Bonds Passed—5 Year Lag	14.0	-1.94	0.0151	-0.0180
	(12.6)	(13.8)	(0.0102)	(0.0254)
Bonds Passed—6 Year Lag	10.5	-0.908	0.0132	-0.0278
	(11.7)	(11.7)	(0.0104)	(0.0205)
Bonds Passed—7 Year Lag	8.49	2.31	0.00929	-0.0343*
	(10.1)	(10.2)	(0.00999)	(0.0188)
Bonds Passed—8 Year Lag	13.4	0.0877	0.0129	-0.0427**
	(11.4)	(10.7)	(0.00969)	(0.0164)
Mean Value	504		12.9	
District Fixed Effects	✓		✓	
Year Fixed Effects	✓		✓	
N	12,251		12,251	
Adjusted R^2	0.9587		0.9735	

See note for Table 2.2 for details. Now bond measures are separated by passage threshold, with coefficient estimates for each type of bond measure reported separately. Significance: * 0.10 ** 0.05 *** 0.01

Now the coefficient estimates for the bond measures passed using a 55% passage threshold are mostly positive, while the coefficient estimates for the bond measures passed using a two-thirds passage threshold are mostly negative. So it does appear that it was the bond measures passed using a two-thirds passage threshold that were driving the results from Table 2.2. The results suggest that bond measures that are approved alongside enhanced accountability requirements increase home prices, while bond measures that are passed with fewer accountability measures decrease home prices. This contrasts with the findings in Cellini et al., who find that bond measure passage increases home prices. The bond measures in that analysis are predominantly those with a two-thirds passage threshold, but in this analysis those measures appear to reduce home prices.

A separating equilibrium could explain these apparently conflicting results. Prior to 2001, a two-thirds passage threshold was used for all bond measures, and on average the bond measures increased home prices. But after 2001, districts could choose to use a lower passage threshold in exchange for enhanced accountability requirements. When districts gained that choice, school boards chose to use a 55% passage threshold for bond measures used to finance projects that are likely to improve home prices, and a two-thirds passage threshold for bond measures for projects that are less likely to improve home prices. This raises the question of whether bond measures passed with a two-thirds passage threshold are used to finance things other than school facilities, such as increases in teacher salaries or the hiring of teachers with different levels of education or experience. That is the subject of inquiry in the next section.

2.4.2 Labor Market Outcomes

In this section I examine whether teacher labor market outcomes change following approval of school bond measures. This provides evidence about whether labor market outcomes could explain changes in home prices following approval of school bond measures. I begin again with a dynamic RD approach before moving on to two OLS frameworks. Table 2.5 displays the dynamic RD estimates for the effect of bond measure passage on three labor market outcomes: (1) average teacher salary, (2) average teacher education level (also known as “column”), and (3) average years of experience for teachers (also known as “step”). Figure 2.10 plots the coefficient estimates from Table 2.5 to show the effect of bond passage on these outcomes over time. Detailed dynamic RD graphs are included in the Appendix as Figures 2.A1 through 2.A3.

Table 2.5: Effect of Bond Measure Passage on Teacher Labor Market Outcomes (Dynamic RD)

	Dependent variable:		
	Average Salary ($1000s)	Average Education Level (Column)	Average Experience Level (Step)
Bond Passed—2 Year Lead	1.16	0.126	0.852**
	(1.29)	(0.283)	(0.386)
Bond Passed—1 Year Lead	1.71	0.279	0.621
	(1.15)	(0.256)	(0.443)
Bond Passed	1.31	0.206	0.854**
	(1.26)	(0.251)	(0.386)
Bond Passed—1 Year Lag	1.85	0.173	1.60***
	(1.32)	(0.312)	(0.397)
Bond Passed—2 Year Lag	1.49	0.154	1.61***
	(1.27)	(0.324)	(0.394)
Bond Passed—3 Year Lag	1.98	0.242	1.35***
	(1.26)	(0.277)	(0.413)
Bond Passed—4 Year Lag	1.83	-0.0372	1.32***
	(1.23)	(0.330)	(0.386)
Bond Passed—5 Year Lag	3.49***	1.17***	0.951**
	(1.34)	(0.285)	(0.428)
Bond Passed—6 Year Lag	3.34**	0.228	0.216
	(1.42)	(0.283)	(0.420)
Bond Passed—7 Year Lag	1.55	-0.0479	0.0527
	(1.53)	(0.285)	(0.436)
Bond Passed—8 Year Lag	-0.802	0.114	-0.416
	(1.51)	(0.271)	(0.507)
Mean Value	72.0	4.69	12.4
Avg. Obs. to the Left of 55%	118	118	118
Avg. Obs. to the Right of 55%	694	694	694

See note for Table 2.1 for details. Now the dependent variables are average teacher salaries, average education level (column), and average years of teacher experience. Significance: * 0.10 ** 0.05 *** 0.01

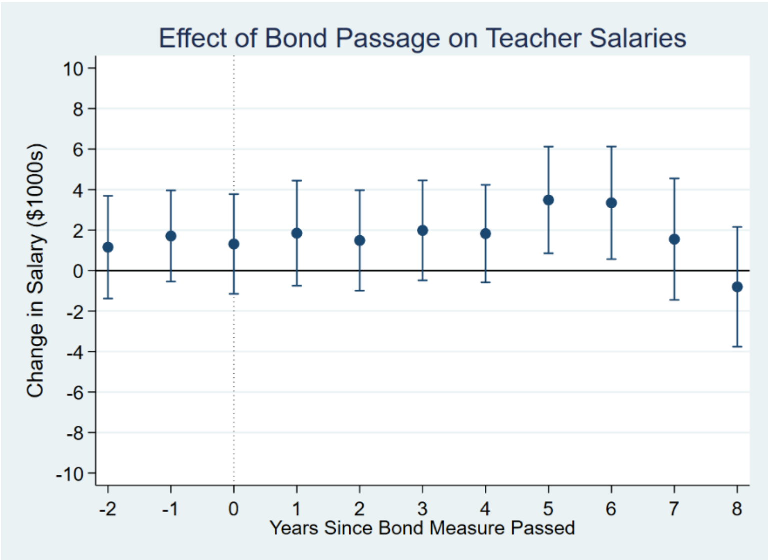

Figure 2.10(a): Effect of Bond Measures on Teacher Salary (Dynamic RD)

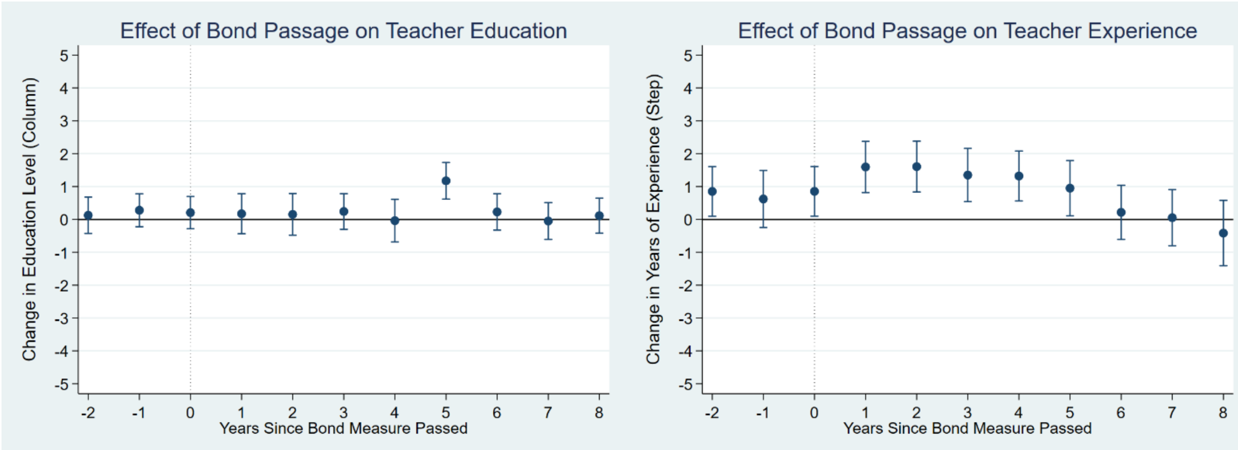

Figure 2.10(b): Effect of Bond Measures on Teacher Education and Experience (Dynamic RD)

The results provide some evidence that the passage of a bond measure leads to an increase in salaries and average years of teacher experience. Bond measure passage is correlated with an increase in average salaries of $1,840 on average over the following eight years, and two of those years have coefficient estimates that are statistically significant at the 5% significance level or better. Bond measure passage is also correlated with an increase in average years of experience for teachers of 0.834 years on average over the following eight years, and five of those years have coefficient estimates that are statistically significant at the 5% significance level or better. However, the coefficient estimate for the two-year lead is positive and statistically significant at the 5% significance level, suggesting that the coefficient estimates from the dynamic RD approach may not solely reflect the causal effect of bond measure passage. And while bond measure passage is correlated with an increase in average education level of 1.17 units five years after passage (statistically significant at the 1% significance level), the other coefficient estimates are much closer to zero. Overall, the dynamic RD approach does provide some evidence that when a school bond measure is passed, average salaries and the average experience level of teachers increase over the following eight years.

Next, I use OLS to evaluate how approval of school bonds affects labor market outcomes. Table 2.6 below displays the OLS estimates for the effect of bond measure passage on labor market outcomes, using models based on Equation (2). Figure 2.11 plots the coefficient estimates from Table 2.6 to show the effect of bond passage on each labor market outcome over time.

Table 2.6: Effect of Bond Measure Passage on Teacher Labor Market Outcomes (OLS)

	Dependent variable:		
	Average Salary ($1000s)	Average Education Level (Column)	Average Experience Level (Step)
Bond Passed—2 Year Lead	-0.389**	-0.0734**	-0.125*
	(0.185)	(0.0287)	(0.0708)
Bond Passed—1 Year Lead	-0.243	-0.0199	-0.119
	(0.220)	(0.0367)	(0.0781)
Bond Passed	-0.183	0.0144	-0.104
	(0.231)	(0.0359)	(0.0844)
Bond Passed—1 Year Lag	-0.109	0.0589	-0.120
	(0.225)	(0.0507)	(0.0909)
Bond Passed—2 Year Lag	-0.111	0.0205	-0.122
	(0.225)	(0.0496)	(0.0881)
Bond Passed—3 Year Lag	0.0755	0.0476	-0.0941
	(0.211)	(0.0539)	(0.0846)
Bond Passed—4 Year Lag	0.0907	0.0460	-0.0368
	(0.241)	(0.0530)	(0.0919)
Bond Passed—5 Year Lag	0.193	0.0523	0.0193
	(0.255)	(0.0459)	(0.0888)
Bond Passed—6 Year Lag	0.166	0.0111	0.00660
	(0.239)	(0.0542)	(0.0923)
Bond Passed—7 Year Lag	-0.103	-0.00450	-0.0853
	(0.196)	(0.0466)	(0.0702)
Bond Passed—8 Year Lag	0.0452	0.00257	0.00297
	(0.177)	(0.0468)	(0.0728)
Mean Value	69.6	4.51	12.1
District Fixed Effects	✓	✓	✓
Year Fixed Effects	✓	✓	✓
N	10,880	10,880	10,880
Adjusted R^2	0.9131	0.8095	0.6887

See note for Table 2.2 for details. Now the dependent variables are average teacher salaries, average education level (column), and average years of teacher experience. Significance: * 0.10 ** 0.05 *** 0.01

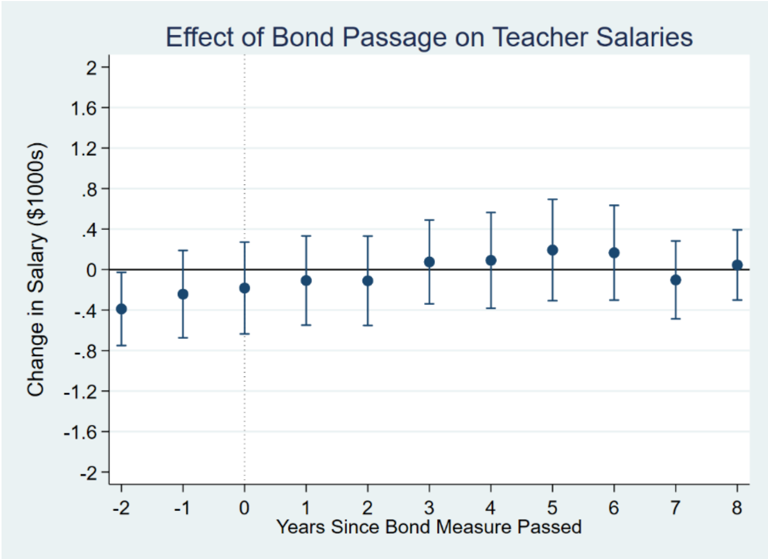

Figure 2.11(a): Effect of Bond Measures on Teacher Salary (OLS)

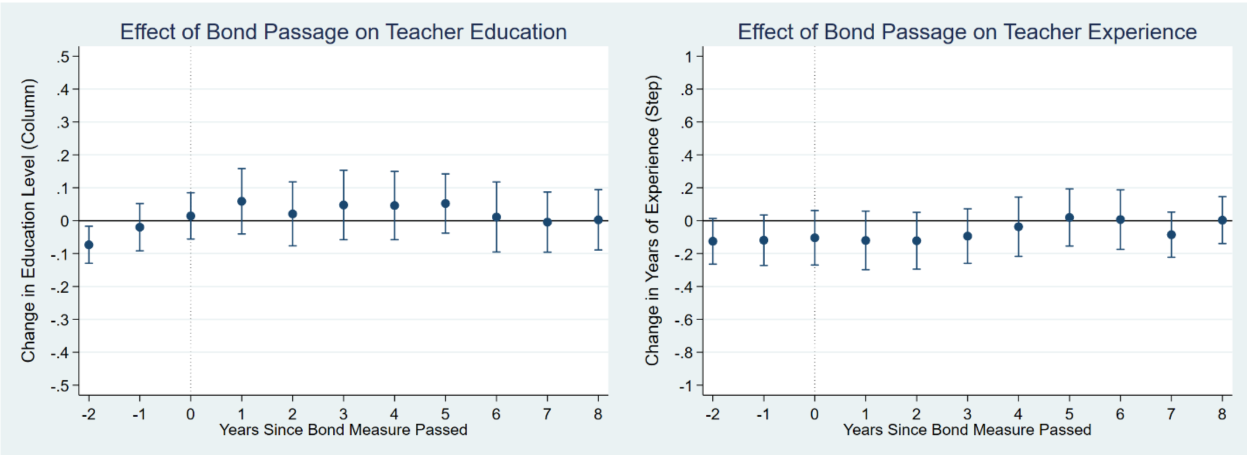

Figure 2.11(b): Effect of Bond Measures on Teacher Education and Experience (OLS)

None of the coefficient estimates in the OLS framework are statistically significant at any standard significance level, except for the coefficient estimates for three of the lead variables. Figure 2.11 appears to show that bond passage was followed by an increase in the average salaries of teachers over the eight years following passage, but the estimated increase was only approximately $350 per year on average, relative to the two years prior to bond passage. The estimated changes for the other outcomes are minimal, suggesting that passage of bond measures has little if any impact on labor market outcomes for teachers.

Finally, I estimate three additional models based on Equation (3). In these models, the dependent variable is the per-student amount of school bonds passed (along with two years of leads and eight years of lags). Bonds that were passed using a 55% passage threshold are separated from bonds that were passed using a two-thirds passage threshold, as the two types of bonds may have differential effects on labor market outcomes. Table 2.7 below displays the OLS estimates for the effect of per-student bonds passed on these labor market outcomes. Table 2.7(a) displays the effect of bonds passed using a 55% passage threshold, while Table 2.7(b) displays the effect of bonds passed using a two-thirds passage threshold. Figure 2.12 plots the coefficient estimates from Table 2.7 to show the effect of bond passage on labor market outcomes over time.

Table 2.7(a): Effect of Bonds Passed ($1000s Per Student) on Teacher Labor Market Outcomes (55%)

	Dependent variable:		
	Average Salary ($1000s)	Average Education Level (Column)	Average Experience Level (Step)
Bond Passed—2 Year Lead	0.00231	-0.00119	-0.000467
	(0.0141)	(0.00240)	(0.00586)
Bond Passed—1 Year Lead	-0.00176	0.00196	0.000278
	(0.0184)	(0.00450)	(0.00566)
Bond Passed	0.0187	0.00330	0.00968
	(0.0191)	(0.00367)	(0.00762)
Bond Passed—1 Year Lag	0.00717	0.00327	0.00293
	(0.0188)	(0.00607)	(0.00865)
Bond Passed—2 Year Lag	0.0156	0.00332	0.00665
	(0.0175)	(0.00603)	(0.00859)
Bond Passed—3 Year Lag	0.0280	0.00743	0.00994
	(0.0181)	(0.00446)	(0.00739)
Bond Passed—4 Year Lag	0.0327*	0.00733	0.00589
	(0.0181)	(0.00487)	(0.00708)
Bond Passed—5 Year Lag	0.0244	0.00775	0.00522
	(0.0189)	(0.00499)	(0.00751)
Bond Passed—6 Year Lag	0.0260	0.00495*	0.00403
	(0.0185)	(0.00275)	(0.00689)
Bond Passed—7 Year Lag	0.0170	0.00380	-0.00247
	(0.0154)	(0.00337)	(0.00620)
Bond Passed—8 Year Lag	0.0327**	0.00462*	-0.00000734
	(0.0157)	(0.00261)	(0.00443)
Mean Value	69.6	4.51	12.1
District Fixed Effects	✓	✓	✓
Year Fixed Effects	✓	✓	✓
N	10,620	10,620	10,620
Adjusted R^2	0.9145	0.8090	0.6874

This table displays coefficient estimates for the effect of school bonds passed using a 55% passage threshold on labor market outcomes. County and year fixed effects are included, and the unemployment rate for the county containing district i in year t is included as a control. Significance: * 0.10 ** 0.05 *** 0.01

Table 2.7(b): Effect of Bonds Passed ($1000s Per Student) on Teacher Labor Market Outcomes (2/3)

	Dependent variable:		
	Average Salary ($1000s)	Average Education Level (Column)	Average Experience Level (Step)
Bond Passed—2 Year Lead	-0.0521*	-0.00385	0.0101
	(0.0285)	(0.00493)	(0.0174)
Bond Passed—1 Year Lead	-0.0294	0.000434	0.0245*
	(0.0294)	(0.00329)	(0.0136)
Bond Passed	-0.0235	0.000316	0.0121
	(0.0292)	(0.00411)	(0.0197)
Bond Passed—1 Year Lag	-0.0549	-0.00589	0.00107
	(0.0444)	(0.00505)	(0.0205)
Bond Passed—2 Year Lag	-0.0719**	-0.00517	-0.00771
	(0.0306)	(0.00474)	(0.0128)
Bond Passed—3 Year Lag	-0.0670**	-0.000911	0.00202
	(0.0275)	(0.00428)	(0.00899)
Bond Passed—4 Year Lag	-0.0722***	0.00595	0.000387
	(0.0247)	(0.00739)	(0.00801)
Bond Passed—5 Year Lag	-0.0585*	-0.00550	0.00696
	(0.0324)	(0.00569)	(0.0104)
Bond Passed—6 Year Lag	-0.0535**	-0.00937	0.0114
	(0.0231)	(0.00791)	(0.0104)
Bond Passed—7 Year Lag	-0.0737***	-0.00977**	0.000335
	(0.0271)	(0.00435)	(0.00492)
Bond Passed—8 Year Lag	-0.0378	-0.00339	0.00649
	(0.0300)	(0.00239)	(0.00894)
Mean Value	69.6	4.51	12.1
District Fixed Effects	✓	✓	✓
Year Fixed Effects	✓	✓	✓
N	10,620	10,620	10,620
Adjusted R^2	0.9145	0.8090	0.6874

See note for Table 2.7(a) for details. The coefficient estimates are for the effect of school bonds passed using a two-thirds passage threshold. Significance: * 0.10 ** 0.05 *** 0.01

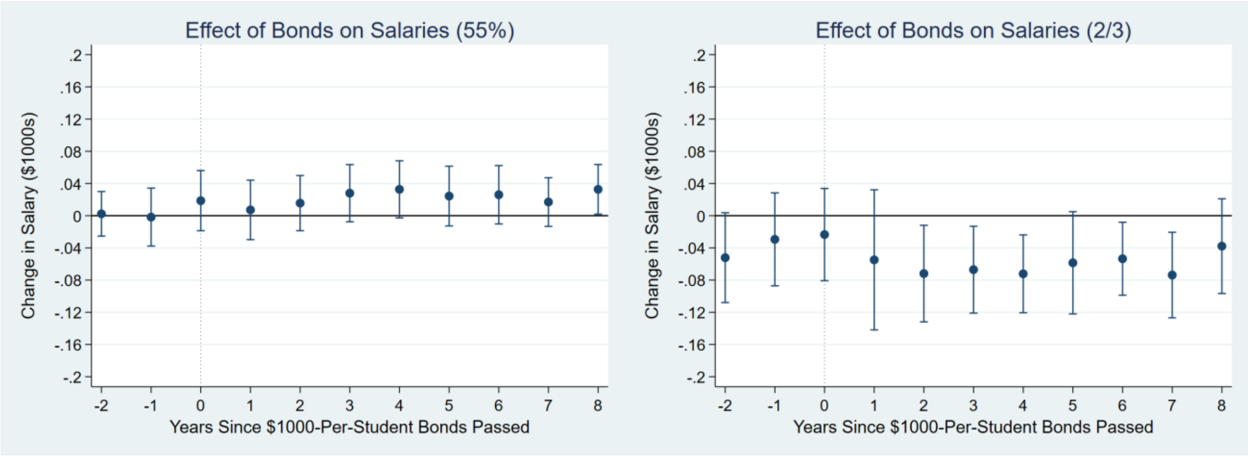

Figure 2.12(a): Effect of School Bonds on Teacher Salary

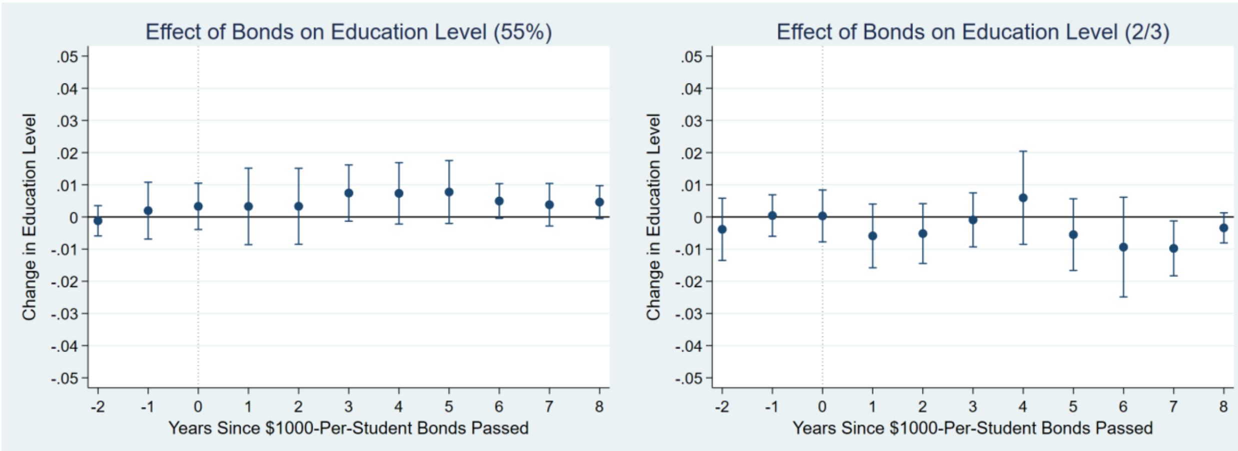

Figure 2.12(b): Effect of School Bonds on Teacher Education

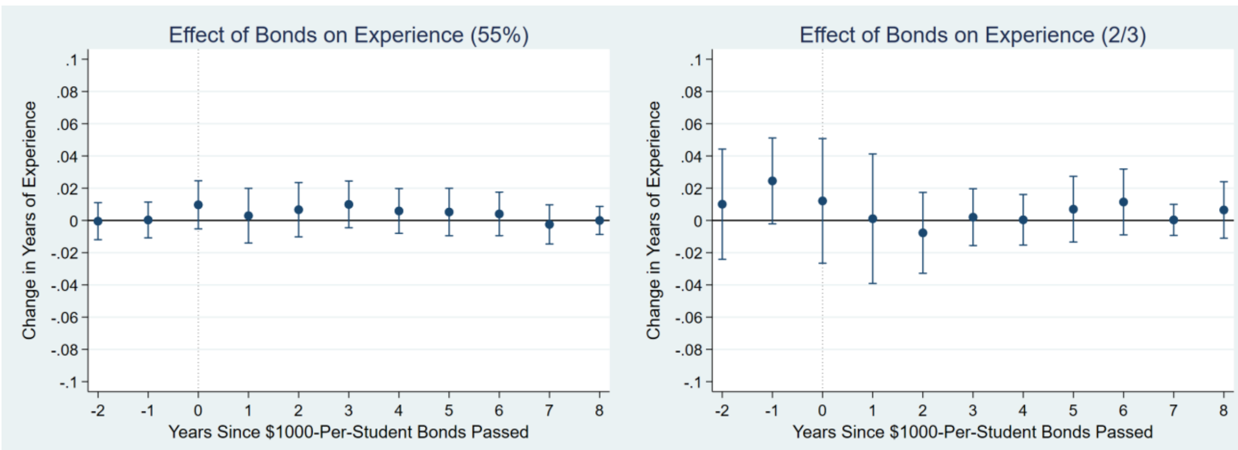

Figure 2.12(c): Effect of School Bonds on Teacher Experience

The results now suggest that the passage of school bonds using a 55% passage threshold is followed by small but precisely estimated increases in teacher salaries and average teacher education level. Passage of $1000-per-student in these school bonds is correlated with an increase in teacher salaries of only $23 per year on average over the following eight years. The coefficients for two of the years are statistically significant at the 10% level or better. The same increase in these school bonds is correlated with an increase in average education level of 0.0053 units on average over the following eight years (roughly 0.12% of the average education level). There are two coefficients that are statistically significant at the 10% level. There is also some evidence that these bond measures are correlated with brief increases in the average experience level of teachers, though none of these effects are statistically distinguishable from zero.

In contrast, the passage of school bonds using a two-thirds passage threshold is followed by decreases in average salaries and average education level, though these decreases are again small. Passage of $1000-per-student in these school bonds is correlated with a decrease in teacher salaries of $61 per year on average over the following eight years. The coefficients for six of the years are statistically significant at the 10% level or better. The same increase in these school bonds is correlated with a decrease in average education level of 0.0043 units on average over the following eight years (roughly 0.094% of the average education level). However, only one of these coefficients is statistically significant (at the 5% significance level).

The results here are much smaller than the results from the dynamic RD approach, but are in generally the same direction. In both methods, passage of bond measures under a 55% passage threshold appears to increase teacher salaries. But based on the OLS results, the same increase in bond measures passed under a two-thirds passage threshold reduces teacher salaries slightly. The apparently conflicting results between the two types of bond measures suggests that greater accountability requirements for school bonds may result in better labor market outcomes for teachers. The exact mechanism for this effect is unclear. It is possible that the enhanced accountability requirements help ensure that districts engage in more competitive bidding for the capital projects funded by the bonds, which results in more money available for teacher salaries. And while the OLS results suggest that the overall effect for teachers is small, the findings do suggest that accountability measures do make some difference in how schools spend money.

Several possible confounders in the above relationships deserve mention. While the dynamic RD approach provides reasonably credible identification of the effect of bond measures on teacher labor market outcomes, the OLS results may be influenced by confounding factors. First, as noted in the previous section, the arrival of new residents into a neighborhood may cause both home prices to increase and school bond measures to be proposed and approved. If these new residents are also in favor of increasing teacher salaries or hiring more educated or more experienced teachers, the OLS estimates of the effect of bond measures on teacher labor market outcomes would be confounded by this contemporaneous change. Thus the OLS estimates should be interpreted as reflecting the correlation between bond measure passage and labor market outcomes, rather than a causal effect. Nevertheless, the OLS results indicate that the passage threshold used for school bond measures does affect teacher labor market outcomes, suggesting that the enhanced accountability measures associated with bond measures passed with a 55% passage threshold do have an effect on how the money is spent.

2.5 Discussion

Much of the recent literature on school bond measures has relied on a dynamic RD approach to measure the effects of the bond measures on various outcomes. But the existing literature has given little attention to the fact that California bond measures differ in the accountability requirements that are required for the bonds. Compared to OLS, the dynamic RD approach may provide more robust causal identification of the effect of bond measures on home prices and labor market outcomes. But with multiple categories of bond measures in California, combined with the fact that different bond measures authorize different amounts of bonds, an OLS framework may allow for better estimation of the effect of bond measures on various outcomes than a dynamic RD approach.

The dynamic RD approach suggests that the passage of bond measures has little effect on home prices, and OLS provides similar results when the independent variable of interest is passage of a bond measure. But when using an OLS specification that accounts for the amount of bonds authorized by the measure, OLS indicates that bonds passed using a 55% passage threshold lead to increases in home prices, while bonds passed using a two-thirds passage threshold lead to reductions in home prices. Both the dynamic RD approach and OLS indicate that bond measures lead to increases in teacher salaries. Furthermore, OLS indicates that bond measures passed with a 55% passage threshold lead to increases in teacher salaries and the average education level of teachers, whereas bond measures passed with a two-thirds passage threshold lead to decreases in both salaries and the average education level. However, the estimated effects on salaries and education level from OLS are both small.

The differential effects of these two kinds of school bonds suggest that enhanced accountability requirements do make a difference in how money is spent. Bonds that are subject to additional accountability requirements appear to increase home prices, teacher salaries, and the average education level of teachers. Bonds that are not subject to these additional accountability requirements appear to have the opposite effect. An explanation for this is that the enhanced accountability requirements accompanying bond measures passed using a 55% passage threshold may promote more competitive bidding for school facilities projects, which may reduce costs and improve the quality of the facilities. The improved school facilities may result in higher property values and therefore higher home prices, and the reduced costs of facilities may result in more money being available for teacher salaries and hiring teachers with more education. Further research on the possible mechanisms that explain this paper’s findings will help illuminate exactly how accountability requirements impact school bond spending.

2.6 Appendix

This appendix includes four graphs related to the effect of bond measure passage on teacher labor market outcomes. Figure 2.A1 displays average real teacher salaries against the percentage of the electorate voting for the school bond measure, with the discontinuity at the 55% passage threshold representing the estimated effect of passage. Figures 2.A2 through 2.A3 display average education level and average years of experience against the percentage of the electorate voting for the bond measure.

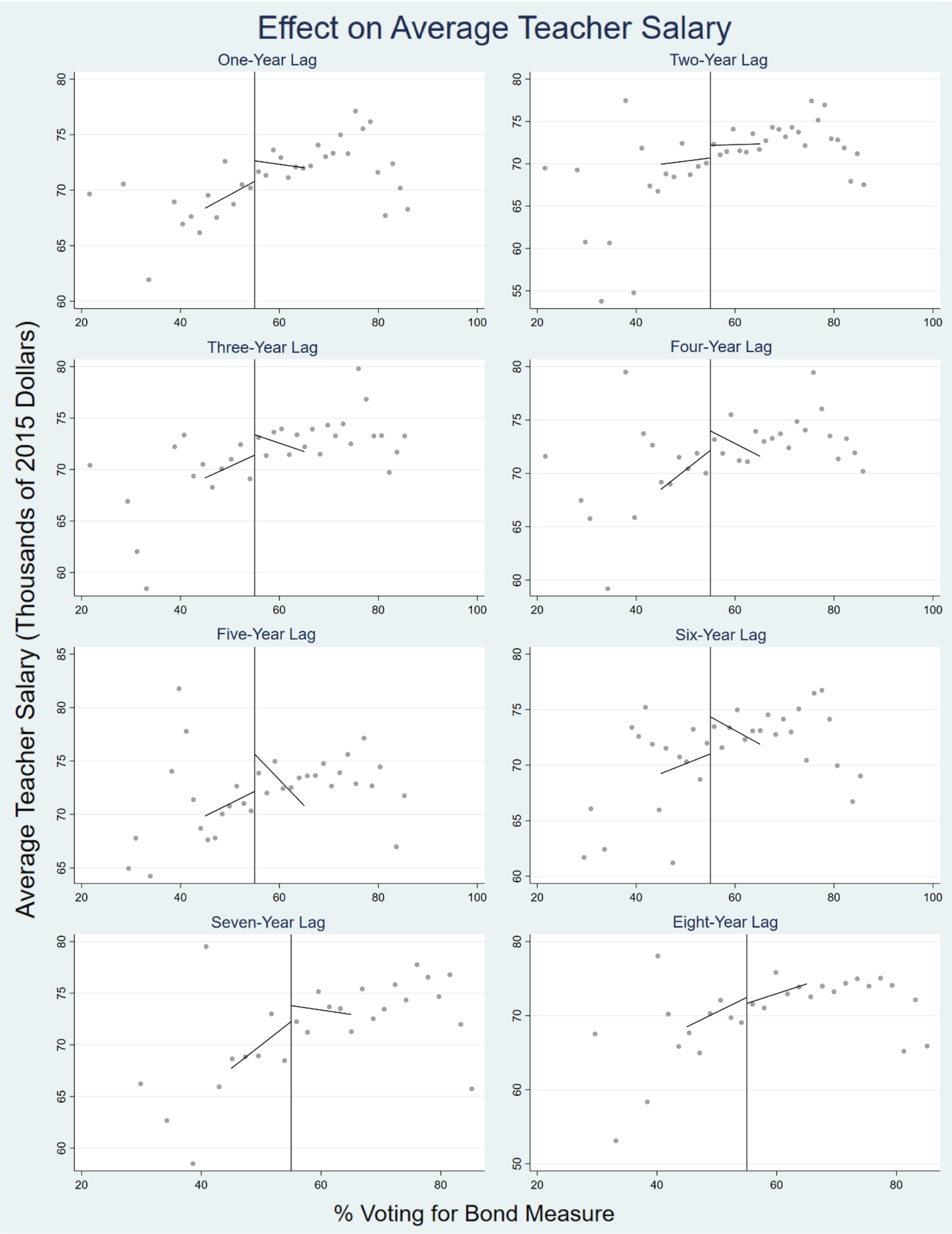

Figure 2.A1: Average Teacher Salary (Dynamic RD)

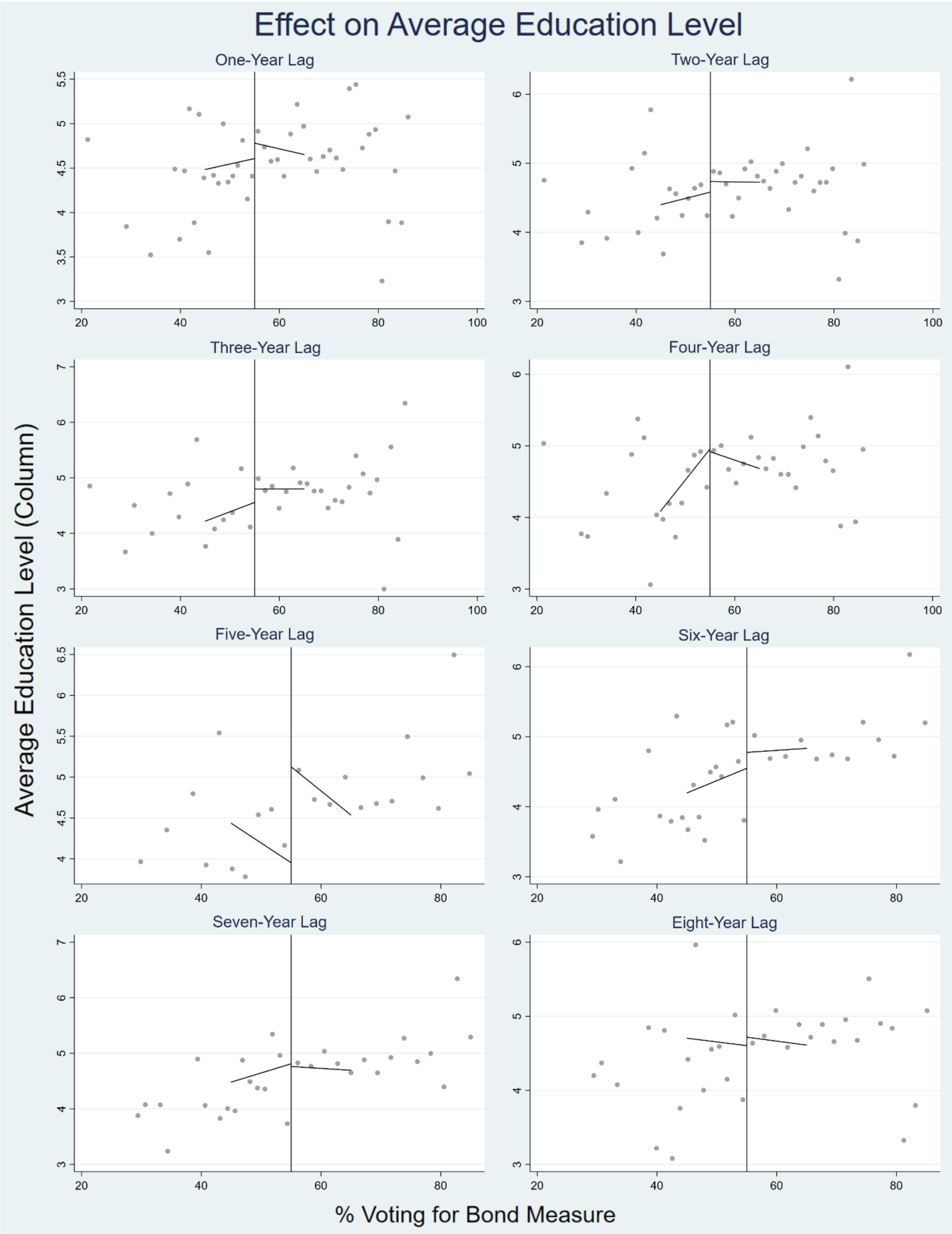

Figure 2.A2: Average Education Level (Dynamic RD)

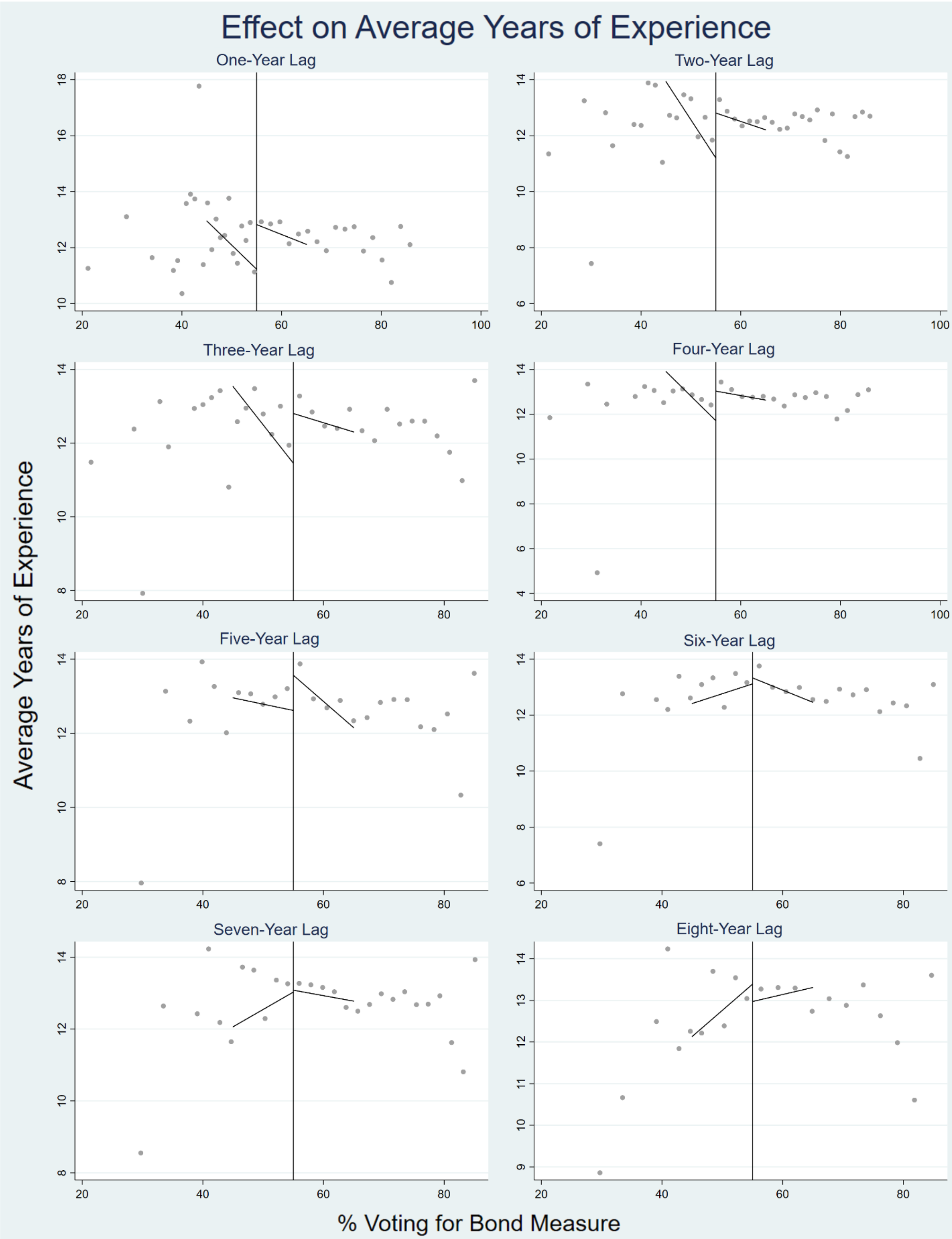

Figure 2.A3: Average Years of Experience (Dynamic RD)

Chapter 3

Increased State Training Funding May Improve Policing Outcomes: Evidence from California

3.1 Introduction

In the wake of several high-profile incidents of police use of force, especially against communities of color, many advocates have called for reform in police training programs. In particular, advocates have argued that police in the United States should receive more hours of training.[50] In the United States, training requirements are enacted at the state level, and there is no federal minimum amount of training that an officer must receive. On average, US police must receive a minimum of 648 hours of training, which is among the lowest in the developed world. In comparison, police officers in Canada, England, and Germany must be trained for at least 1,040 hours, 2,250 hours, and 4,050 hours respectively. Even within the US, police training requirements are significantly lower than in other occupations. Cosmetologists and plumbers, for

[50] *Not Enough Training*, THE INSTITUTE FOR CRIMINAL JUSTICE TRAINING REFORM, https://www.trainingreform.org/not-enough-training (last visited Sept. 26, 2021).

example, must receive 3,000 hours and 3,500 hours of training respectively, and advocates have pointed to these comparatively high standards to argue for stronger police training standards.

Advocates have also called for reform in the type of training received by officers.[51] While nearly a third of police training focuses on combat tactics, officers on average spend only ten hours of training on mental health intervention, even though at least a quarter of people killed by the police exhibited signs of mental illness. And while nearly half of the people killed by the police are nonwhite, law enforcement has only limited training related to culture and bias, and many states have no bias training mandates at all. In the absence of national standards, different training programs may provide different levels of coverage for combat training, mental health, culture and bias, and other topics. Graduates of different police training programs may therefore respond differently when confronted with scenarios involving these topics.

In this paper I examine how police training programs differ based on whether the programs are administered by local/county law enforcement agencies or by the California Peace Officer Standards and Training (POST) Commission. I then examine how funding from the POST Commission—which is used to reimburse POST-administered courses—affects law enforcement outcomes. These outcomes are the number of complaints that are received by and sustained against the police agency, and the number of police shootings involving the agency. I rely on a detailed database of standards and training funds received by local and county police departments and a new database documenting police complaints and use of force by officers.

Relative to training materials from county and local law enforcement agencies, I find that course outlines from courses administered by the California POST Commission appear to have more positive coverage of five topics—(1) force and weapons, (2) mental and physical health,

[51] *The Wrong Training*, THE INSTITUTE FOR CRIMINAL JUSTICE TRAINING REFORM, https://www.trainingreform.org/the-wrong-training (last visited Sept. 26, 2021).

(3) professionalism, (4) rights, and (5) the rule of law. I find that increased funding from the California POST Commission appears to lead to a small temporary decrease in civilian complaints sustained. There is some evidence that increased POST funding leads to a decrease in police shootings in each of the three years following the increase, though these results are statistically indistinguishable from zero. The results suggest that improved training may improve law enforcement practices and reduce the use of lethal force in the following years.

There is modest evidence that contemporaneous political changes may occur with the changes in POST funding. The strongest evidence of contemporaneous political change suggests that policing-related ballot measures are less likely to be introduced two years after a police agency sees an increase in POST funding. This indicates that increased reliance on POST training programs may actually improve officer conduct, and this reduces the perceived need to address policing and police reform through the ballot measure process.

There has been some interest in measuring the effect of improved police training on law enforcement outcomes. Previous literature has examined whether greater amounts of training hours reduces use of force by police officers (Lee et al. 2010). This analysis used a multilevel analysis/hierarchical linear modeling and found that greater hours spent in police training is correlated with greater use of force. However, the direction of causality in this relationship is ambiguous: as the authors note, the finding may simply indicate that police departments with higher levels of use of force just provided longer in-service training hours to address the use of force problem. More recent literature related to police training exploited random assignment of police officers to a procedural justice training program that attempts to slow down their thought processes during citizen encounters (Owens et al. 2018). The research found that attending the procedural justice training resulted in a lower likelihood of resolving incidents with arrests and lower use of force. A similar study, which relied on variation in the timing that officers receive procedural justice training, found that the training resulted in lower use of force, but there is no clear evidence that it reduced reported or sustained police complaints (Wood et al. 2020). Yet there has been a general dearth of research examining how the source and amount of police training funding affect the perception or actuality of misconduct among police.

There has also been some recent interest in police complaints and the factors that affect the decision to file a complaint against the police. Prior research has focused on the process of filing a complaint, including research on the complainants' experiences with and perceptions of the complaints process (Waters & Brown 2000). Recent research examined the link between body-worn cameras, police complaints, and police use of force by relying on random assignment of police officers to shifts where they would wear a camera (Ariel et al. 2015). The research found that the cameras substantially reduced police use of force and complaints. Other research has examined the factors that influence whether a citizen will file a complaint against the police, finding that physical distance from a reporting center decreases the probability of signing a police complaint (Ba 2017). However, there has generally been a lack of empirical research on the factors that influence the number of complaints reported against the police, and how those complaints are resolved. The police complaints process is one of the primary components of police accountability.[52] Furthermore, police complaints are one of the few measures of a police department's level of support in its community, especially in cities and towns where police leadership is generally not elected. Failing to examine the factors that affect police complaints therefore risks overlooking the most important factors that affect public support for the police.

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[52] *Principles of Good Policing: Avoiding Violence Between Police and Citizens*, UNITED STATES DEPARTMENT OF JUSTICE, https://www.justice.gov/archive/crs/pubs/principlesofgoodpolicingfinal092003.htm (last updated Sept. 2003).

This paper makes two main contributions to this body of literature. First, this is the first paper that seeks to credibly examine how the amount of police training funding received from the state affects reported and sustained police complaints and the use of force by officers. While prior literature has examined how certain types of training—for example, training focused on procedural justice—affects various law enforcement outcomes, no paper to date has credibly assessed the relationship between state training funding, police complaints, and the use of force by officers. Second, this paper examines the differences between POST and non-POST training programs, which provides possible explanations for the paper's main results. This provides a basis for future researchers and policymakers to determine whether reforming the content of training programs could reduce the perception or actuality of police misconduct.

The remainder of this paper is organized as follows. Section II provides additional background on standards and training funding and how this may affect law enforcement outcomes. Section III describes the data and methods that I use to estimate how training funding may impact the law enforcement outcomes of interest. Section IV provides the results, and Section V discusses the results and concludes.

3.2 Background

To fully appreciate how training funded through POST revenue may impact law enforcement outcomes, it is important to understand the purpose and details of this revenue. To evaluate how police training might affect law enforcement outcomes, I rely on a revenue stream known as the peace officer standards and training functional revenues. The source of this revenue is the California Commission on Peace Officer Standards and Training (POST).

POST was established by the California Legislature in 1959 to set minimum selection and training standards for law enforcement agencies in California.[53] POST funding comes from the General Fund and the State Penalty Fund, and the POST program is voluntary and incentive-based. There are more than 600 agencies that participate in the POST program, and those agencies agree to abide by the standards established by POST. In addition to other services and benefits, those agencies receive reimbursement for training officers.

The amount of money received from the peace officer standards and training functional revenues was high in fiscal year 2020 relative to the previous ten years. In 2020, 237 local agencies received a total of $15,284,357 from this program, or $64,491 on average. Likewise, 43 county agencies received a total of $16,394,980 from this program, or $381,279 on average. At a per-officer level, the local agencies received approximately $673 per officer from this revenue, while the county agencies received approximately $669 per officer from this revenue. Figure 3.1 below shows total POST funding received by law enforcement agencies from fiscal years 2010 to 2020. As Figure 3.1 illustrates, fiscal year 2020 may have been an outlier for POST funding, with agencies receiving substantially less POST funding in previous years.

[53] *About POST*, COMMISSION ON PEACE OFFICER STANDARDS AND TRAINING, https://post.ca.gov/About-Us (last updated July 14, 2021).

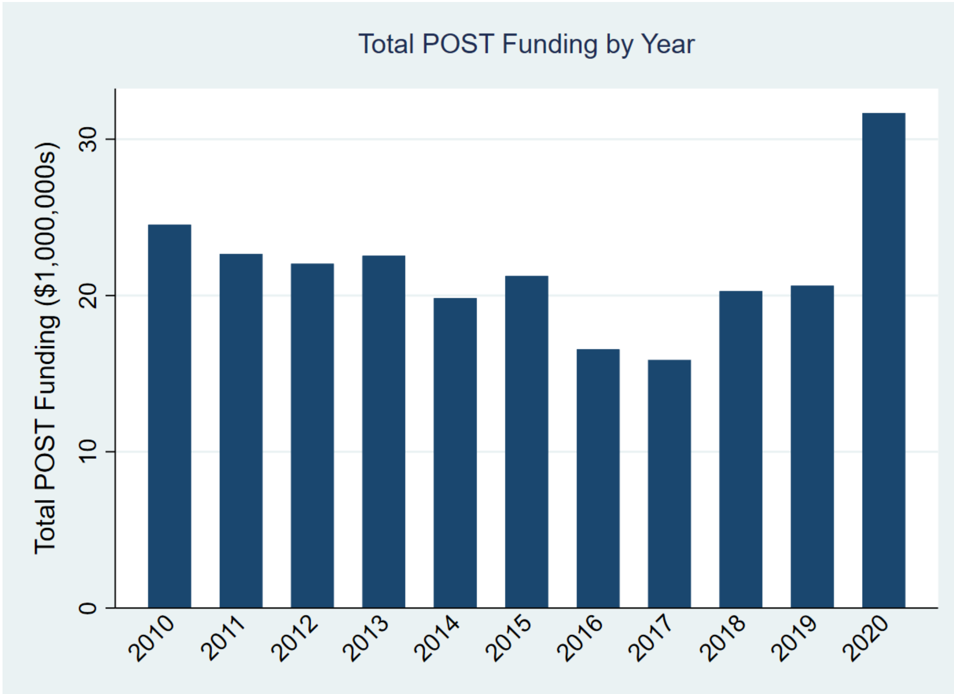

Figure 3.1: Total POST Funding by Year (California)

Law enforcement agencies generally receive POST revenue as reimbursement for sending officers through training academies or courses that are administered by the California POST Commission or a related agency. Law enforcement agencies that choose to train their officers in academies that are not administered by the POST Commission will not be eligible for POST revenue. However, these agencies may choose to forgo POST revenue because they want a training academy that is more closely tailored to the needs of their agency.

Thus, POST revenue comes with a tradeoff: the law enforcement agency will save money on training costs (which might be spent on other resources), but the agency will have less discretion to tailor the training to the needs of the agency. Additionally, POST-administered training academies may focus on different topics than other training academies. If this is the case, then we may expect to see different law enforcement outcomes between officers trained in POST courses and officers trained in other courses. Thus, agencies that receive significant POST revenue may have law enforcement outcomes that are better than, worse than, or the same as agencies that do not receive significant POST revenue.

3.3 Data and Methods

In this paper I use data from several sources for evaluating how state police training funding affects law enforcement outcomes. I use this data to construct a single database with information on police training, law enforcement outcomes, and various controls. The database is organized by town/county and year, and it contains 539 California towns/counties from 2010 to 2020, for a total of 5,926 observations included in the database.[54] In most cases, a variable is available for all or nearly all of the towns and counties. However, as discussed in more detail below, some important variables (specifically, the data related to total police training expenditures) are only available for a small subset of the data, and therefore the number of useful observations is much smaller than the 5,926 observations in the database.

First, I obtained data on county and local revenues and expenditures from the California State Controller.[55] This dataset includes revenue, spending, and other data for virtually every city and county in California for each fiscal year from 2002–2003 to 2019–2020. This dataset includes the amount of functional revenues received from the state of California for peace officer standards and training (POST revenue). This revenue is primarily used to reimburse local and county police agencies for the tuition associated with POST-administered training courses. California law requires officers to complete twenty-four hours of continuing professional

[54] Two towns, Eastvale and Jurupa Valley, were incorporated in 2010 and 2011, respectively. Their late incorporation explains the three missing observations from the database.
[55] *California State Controller's Office Local Government Financial Data*, CALIFORNIA STATE CONTROLLER, https://bythenumbers.sco.ca.gov/ (last updated May 28, 2021).

training (CPT) every two years,[56] and officers often enroll in these courses to satisfy their CPT requirements. This revenue stream is the main explanatory variable that I rely on to determine how POST training affects policing outcomes.

Second, I obtained data for various law enforcement outcomes from Police Scorecard.[57] Police Scorecard is the first nationwide public evaluation of policing in the United States, and it calculates measures of police violence, accountability, racial bias, and other factors for over 16,000 local and county law enforcement agencies.[58] Police Scorecard collects and provides data on several variables related to police conduct, including data on police complaints, police shootings, killings by police, and homicide clearance rates. In this analysis I use the complaints data and the police shootings data, both of which are available for California agencies from 2016 to 2020. Both of these variables are useful for measuring the effect of POST training on police conduct. And unlike homicides and killings by police, both police complaints and police shootings happen relatively frequently, and so there is likely enough data to determine if POST funding has an effect on these variables. The complaints dataset includes the following variables for each agency and year: (1) the number of civilian complaints reported and sustained; and (2) the number of discrimination complaints reported and sustained. Because both the complaints and the shootings datasets are available for each agency and each year, changes in each of these variables within each agency can be tracked.

Third, I obtained data on total police training expenditures from individual budget documents for several dozen police agencies. To obtain this data, I searched through budgets or

[56] *Legislative Mandated Section*, COMMISSION ON PEACE OFFICER STANDARDS AND TRAINING, https://post.ca.gov/legislative-mandated-training (last updated Feb. 25, 2022 9:43 AM).
[57] POLICE SCORECARD, https://policescorecard.org/ (last visited Sept. 26, 2021).
[58] *Police Scorecard Project Methodology*, POLICE SCORECARD, https://policescorecard.org/about (last visited Sept. 26, 2021).

other financial data for the 132 largest towns and counties in California. These are towns and counties that had a population of at least 50,000 people between 2010 and 2019. I found that seventy-two of these towns and counties provided information on the amount of money spent on police training, and I collected the amount of training expenditures for each of these agencies for each year that was available from fiscal year 2009–2010 to fiscal year 2019–2020.

In addition to these datasets, I obtained data on additional variables from various sources. I obtained data on the typical value of a single-family home by county and city from Zillow.[59] I obtained data for the population of each city and county in each year from the California Department of Finance,[60] and I obtained data on the unemployment rate of each city and county in each year from the California Employment Development Department.[61] Table 3.1 below provides descriptive statistics for all of the variables that I use in my main analysis from 2016 to 2019. The values in Table 3.1 are restricted to police agencies for which at least one outcome variable (related to police complaints or police shootings) is present for at least one year.

Table 3.1: Descriptive Statistics

Variable	Obs.	Mean	SD	Minimum	Maximum
Total POST Functional Revenues	1,528	47300	217000	-358	3350000
Per Officer POST Functional Revenues ($100s)	1,508	4.39	41.7	-0.325	1590
Total Training Expenditures ($1000s)	284	1170	3490	5.15	26100
Per Officer Training Expenditures ($1000s)	280	3.85	5.14	0.0780	31.5
Total Police Expenditures ($1000s)	1,528	45100	181000	30	2720000
Per Officer Police Expenditures ($100,000s)	1,508	2.69	0.913	0.0429	8.44
Total Police Officers	1,508	179	756	1	10000
Police Officers Per 1,000 Residents	1,508	1.81	6.89	0.278	138
Typical Home Price ($100,000s)	1,499	6.62	6.76	0.811	66.4
Unemployment Rate	1,504	5.43	3.72	0	39.2
Civilian Complaints Reported Per 100 Officers	1,507	13.9	16.6	0	191
Civilian Complaints Sustained Per 100 Officers	1,507	1.74	4.23	0	70.6
Discrimination Complaints Reported Per 100 Officers	1,506	0.991	2.63	0	35
Discrimination Complaints Sustained Per 100 Officers	1,504	0.0423	0.461	0	10
Police Shootings Per 100 Officers	912	0.881	1.54	0	11.8

[59] *Housing Data*, ZILLOW, https://www.zillow.com/research/data/ (last visited Sept. 26, 2021).
[60] *Estimates*, STATE OF CALIFORNIA DEPARTMENT OF FINANCE, https://www.dof.ca.gov/forecasting/demographics/estimates/ (last visited Sept. 26, 2021).
[61] *Labor Force and Unemployment Rate for Cities and Census Designated Places*, EMPLOYMENT DEVELOPMENT DEPARTMENT, https://www.labormarketinfo.edd.ca.gov/data/labor-force-and-unemployment-for-cities-and-census-areas.html (last visited Sept. 26, 2021).

The dependent variables of interest are the complaints variables (each calculated per 100 officers) and police shootings per 100 officers. Civilian complaints are include discrimination complaints as a subset category. Agencies see an average of 13.9 civilian complaints reported and 1.74 civilian complaints sustained per 100 officers, compared to 0.991 discrimination complaints reported and 0.0423 discrimination complaints sustained per 100 officers. Overall, 12.5% of civilian complaints are sustained, versus 4.27% of discrimination complaints. On average there are approximately 0.881 police shootings per 100 officers each year.

The two most important independent variables in this analysis are per-officer training expenditures for each agency, and per-officer POST functional revenues received by the agency. On average each agency spends roughly $3,849 per officer on training expenditures, of which approximately $439 per officer is received from the POST functional revenues. However, because total training expenditures are parsed from budget documents which are unavailable for most agencies, the sample size for per-officer training expenditures is only 280 observations. The per-officer training expenditures will be the bottleneck in the empirical analysis: there are only seventy law enforcement agencies for which I have both police complaints data and per-officer training expenditures, and there are only sixty-three agencies for which I have both police shootings data and per-officer training expenditures.

To successfully evaluate how standards and training funding affects law enforcement outcomes of interest, there must be variation in the amount of standards and training funding that police agencies receive over time. One measure for the amount of within-agency variation in standards and training funding is the difference between the maximum amount and minimum amount of per officer POST revenues received by an agency. Figure 3.2 below displays the number of police agencies by the maximum difference in per officer POST revenues received from 2011 to 2020. There are 53 agencies for which the maximum difference in per capita POST revenues received is $0. Of these 53 agencies, 52 of them received no POST revenues during the entire period, and the remaining agency only appears in the dataset in one year. Excluding these 53 agencies, the average maximum difference is $1,693, and the median maximum difference is $876. Given that the average value for per officer POST revenues is $470, there is considerable variation in standards and training funding over this time period, including within individual law enforcement agencies.

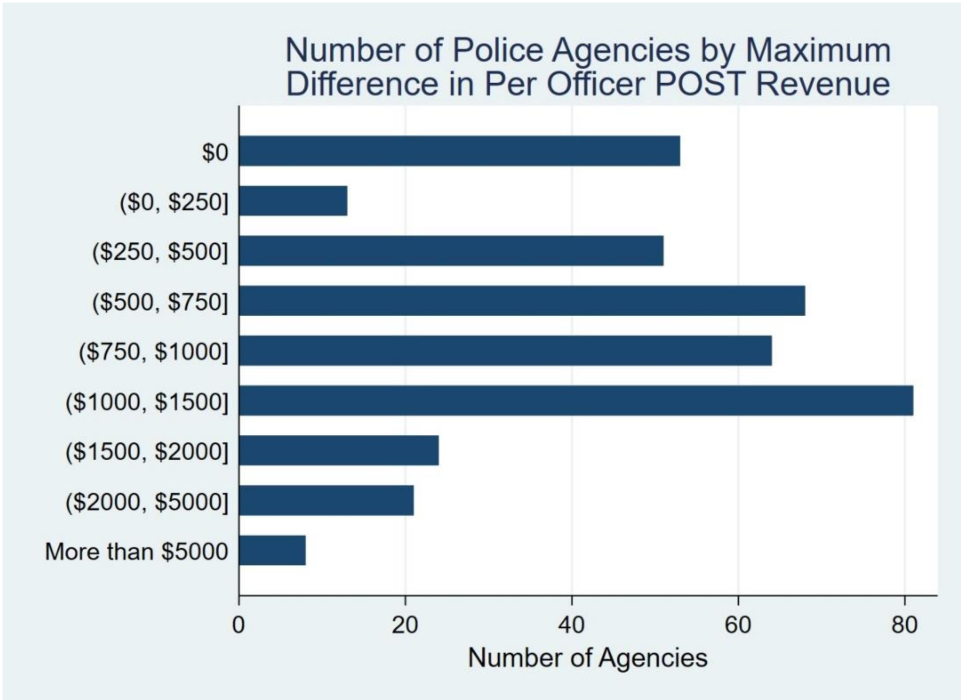

Figure 3.2: California Agencies by Maximum Difference in Per Officer POST Revenue Received

Before assessing the link between police training expenditures and funding and law enforcement outcomes, I analyze the training documents of various California law enforcement agencies to determine the differences that exist between POST and non-POST training programs in California. I obtained training documents for thirty-six law enforcement agencies and academies across California. I obtained two types of training documents: (1) POST course outlines and (2) field training manuals from local and county law enforcement agencies. The documents from POST training courses are the outlines of the basic intensive training course offered by twenty-six of thirty California POST training academies available at the POST Commission's Open Data portal.[62] The remaining documents are the training documents available from eighteen county and local law enforcement agencies. Each of these agencies provides law enforcement services to cities or counties in California, each with a population of at least 100,000. I obtained these documents from the websites of each of the agencies, which are required under Senate Bill 978 to publicly display these files.[63] For these documents, I restrict the sample to training materials that provide guidance on a broad range of topics, as opposed to documents that are narrowly focused on a particular topic.

I use textual analysis methods to identify nine major topics related to policing contained in these documents. Specifically, I use a semisupervised Latent Dirichlet allocation (seeded LDA) process to identify 50 groups of words, with each group containing 25 words. After eliminating duplicate words, I obtain approximately 640 words in the documents. Of these 640 words, I identify 95 words/phrases that fall into nine topics, and I sort the 95 words/phrases into one of the nine topics. The nine topics are (1) community policing,[64] (2) culture and identity,[65] (3) family conflict,[66] (4) force and weapons,[67] (5) mental and physical health,[68] (6)

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[62] *OpenData*, POST, https://opendata.post.ca.gov/ (last visited March 2, 2022).

[63] Senate Bill 978, which was enacted in September of 2018, "requires POST and local law enforcement agencies to post conspicuously on their websites 'all current standards, policies, practices, operating procedures, and education and training materials that would otherwise be available to the public'" under the California Public Records Act. *Id.*

[64] The terms included for this topic are: community; conflict management; de-escalation; deterrence; effective; first aid; leadership; partnership; prevention; problem-solving; skills; rehabilitation; respect; trust; welfare.

[65] The terms included for this topic are: bias; color; culture; discrimination; ethnic; gay; gender; hate crime; LGBT; national origin; prejudice; profiling; race; religion; religious; sexual orientation; sexual preference; stereotype.

[66] The terms included for this topic are: child abuse; domestic abuse; domestic violence; domestic disturb; elder abuse; exploitation.

[67] The terms included for this topic are: baton; canine; carotid; chemical agent; chemical spray; choke; conflict; firearm; force; gun; lethal; revolver; taser; tear gas; violence; weapon.

[68] The terms included for this topic are: developmental; disability; disorder; DSM; homeless; illness; intellectual; mental.

professionalism,[69] (7) rights,[70] (8) rule of law,[71] and (9) sex-related offenses.[72] For both document types (POST course outlines and non-POST training documents), I recorded the number of paragraphs mentioning each of the nine topics, and I compared how frequently each of these topics was mentioned based on whether the document was from a POST training academy course or from a local or county law enforcement agency.

Next, for each topic, I calculate the sentiment value for each paragraph containing a word or phrase relevant to the topic. I then compare the average sentiment value of POST course outlines and non-POST training manuals for each topic. This provides evidence on how positively or negatively the document treats each topic. This follows loosely in the footsteps of Johansen (2007), who systematically analyzes Prussian and French police documents from the late nineteenth and early twentieth centuries to compare how law enforcement forces legitimized police violence in these areas. This analysis provides insights into how POST training programs differs from non-POST training programs, and how the amount of POST training funding received by a police agency may affect law enforcement outcomes.

I next move on to the main analysis of this paper, where I evaluate the effect of POST training funding on policing outcomes. To do this, I regress each law enforcement outcome of interest against the amount of per officer training expenditures and POST funding received in the current year, as well as three years of lags. The econometric specification for this analysis is:

[69] The terms included for this topic are: communication; ethic; obligation; policy; professional; responsibility; values.

[70] The terms included for this topic are: amendment; attorney; constitution; freedom; lawyer; liberty; Miranda; privacy; probable cause; reasonable suspicion; rights; waive; warrant.

[71] The terms included for this topic are: court; due process; judicial; jury; justice; procedure protocol; trial.

[72] The terms included for this topic are: consensual; consent; harassment; rape; sex.

$$Y_{i,t} = \sum_i \alpha_i * Agency_i + \sum_t \beta_t * Year_t + \sum_{j=0}^{j=3} \gamma_j * POTE_{i,t-j} + \sum_{j=0}^{j=3} \delta_j * POPOSTR_{i,t-j} + \kappa * X_{i,t} + \epsilon_{i,t} \tag{1}$$

Y represents the law enforcement outcome of interest (for example, the number of complaints filed against the agency per 100 officers). POTE is the per officer amount of total training revenue (in thousands of dollars) received by agency i in year t-j. Similarly, POPOSTR is the per officer amount of POST revenue (in hundreds of dollars) received by agency i in year t-j. Each γ and δ coefficient estimates the effect of increased per officer training expenditures or POST revenue j years in the past on the outcome variable of interest. Fixed effects are included for each law enforcement agency and year, X is a vector of controls, and ε is the error term.

Two limitations in this methodological framework are important to acknowledge. First, this analysis examines the correlation between training funding/expenditures and various law enforcement outcomes, with controls for possible confounding factors and fixed effects for each agency and year. However, it is unclear if there are other changes that occur when law enforcement agencies obtain more POST funding or increase training expenditures. For example, the agencies that request and receive additional POST funding may be taking other steps to reduce police misconduct and the excessive use of force. Additionally, because these agencies may be saving money by relying on training academies and courses that provide reimbursement, these agencies may be spending additional money on other resources or tactics.

To assess whether other contemporaneous changes may be impacting an agency's training expenditures or receipt of POST revenue, I run two additional sets of analysis. First, it is possible that changes in attitudes about policing may be driving both changes in the amount of POST revenue received and changes in other factors that may affect the outcomes of interest.

Using the introduction of local ballot measures as a proxy for attitudes about policing, I analyze whether the passage of local ballot measures related to policing and police reform are correlated with changes in POST funding. The econometric specification is:

$$POPOSTR_{i,t} = \sum_{i} \alpha_i * Agency_i + \sum_{t} \beta_t * Year_t + \sum_{j=-2}^{j=2} \gamma_j * I_{t-j}\{\geq\ 1\ Police\ Ballot\ Measure\ on\ Ballot\} + \kappa * X_{i,t} + \epsilon_{i,t} \tag{2}$$

The dependent variable of interest is per-officer POST revenue received by agency i in year t. I regress this against a series of five indicator variables. The indicator variable codes for whether at least one ballot measure related to policing/police reform was on the ballot for that town or county in that year, as well as two years of leads and two years of lags. Nonzero coefficient estimates on these variables would suggest that changes in the introduction of ballot measures related to policing/police reform are correlated with contemporaneous (or nearly contemporaneous) changes in POST revenue. As before, fixed effects are included for each law enforcement agency and year, X is a vector of controls, and ε is the error term.

Second, it is possible that changes in department leadership lead to changes in police training expenditures or POST revenue received, along with other changes that may affect the law enforcement outcomes of interest. I therefore evaluate whether changes in department leadership are correlated with changes in training expenditures or POST revenue. I regress training expenditures and POST revenue against an indicator variable that codes for whether a county law enforcement agency had a new (non-incumbent) sheriff elected that year, along with four years of lags. The econometric specification for police training expenditures is:

$$POTE_{i,t} = \sum_i \alpha_i * Agency_i + \sum_t \beta_t * Year_t + \\ \sum_{j=0}^{j=4} \gamma_j * I_{t-j}\{New\ Sheriff\ Elected\} + \kappa * X_{i,t} + \epsilon_{i,t} \tag{3}$$

And the econometric specification for POST revenue is:

$$POPOSTR_{i,t} = \sum_i \alpha_i * Agency_i + \sum_t \beta_t * Year_t + \\ \sum_{j=0}^{j=4} \gamma_j * I_{t-j}\{New\ Sheriff\ Elected\} + \kappa * X_{i,t} + \epsilon_{i,t} \tag{4}$$

A nonzero estimate for any of the γ coefficients would suggest that the election of a new sheriff does indeed lead to changes in training expenditures or POST revenue. If this is the case, then other changes in departmental policies could be driving the estimated effects of training expenditures or POST revenue on the law enforcement outcomes of interest.

A second limitation in this methodology is that the analysis relies on only four years of data—from 2016 through 2019. Moreover, due to limitations in the number of towns and counties that publish police training expenditures, the analysis for police complaints is based on only seventy law enforcement agencies, and the analysis for police shootings is based on only sixty-three agencies. As data for more agencies becomes available and as the datasets extend to include additional years, future researchers will be able to provide more precise and accurate estimates of the impact of standards and training funding on law enforcement outcomes.

3.4 Results

3.4.1 POST-Operated Training Academies

Before determining the effect of POST training on law enforcement outcomes, I assess what differences exist between POST-operated training academies and other training academies. To do this, I examine the differences that exist between training documents from POST training courses and training documents from county and local law enforcement agencies. This provides insights into how law enforcement outcomes may change when law enforcement agencies rely more on POST training and less on training from other sources.

First, I assess how the amount of attention each document directs to each topic depends on whether the document is from a POST training course or from a county or local law enforcement agency. Table 3.2 below displays the frequency with which various policing-related topics are mentioned in training documents. The nine topics are again (1) community policing, (2) culture and identity, (3) family conflict, (4) force and weapons, (5) mental and physical health, (6) professionalism, (7) rights, (8) rule of law, and (9) sex-related offenses. Table 3.2 displays the number of paragraphs mentioning each topic per 100 paragraphs in agency training documents and POST course outlines. The table also displays each topic's frequency for agency training documents divided by the topic's frequency for POST course outlines, and the lesser of the frequencies divided by the greater of the frequencies.

Table 3.2: Frequency Nine Topics Are Mentioned Per 100 Paragraphs

Variable	Agency Training Documents	POST Course Outlines	Agency/POST	Less Frequent/ More Frequent
Observations	25	27		
Total Number of Paragraphs	54085	38975		
Community Policing	4.13	7.55	0.547	0.547
Culture and Identity	1.07	2.02	0.531	0.531
Family Conflict	0.361	0.731	0.493	0.493
Force and Weapons	4.11	6.26	0.657	0.657
Mental and Physical Health	1.09	1.80	0.604	0.604
Professionalism	6.30	5.53	1.14	0.879
Rights	2.58	3.99	0.647	0.647
Rule of Law	3.77	3.78	0.997	0.997
Sex-Related Offenses	1.03	2.04	0.506	0.506

Several observations are notable. First, Table 3.2 shows that there are no topics for which the coverage in POST course outlines and the coverage in agency training documents differ vastly. The greatest percentage differences are for the coverage of family conflict, sex-related offenses, culture and identity, and community policing. Agency training documents mentioned family conflict only 49% as often as POST course outlines did, and for the other topics, agency training documents mentioned each topic between 50% and 55% as often as POST course outlines did. For the remaining topics, the agency training documents mentioned each topic at least 60% as often as POST course outlines did (or vice versa). There was only one topic for which agency training documents mentioned the topic more often than POST course outlines: professionalism. POST course outlines mentioned professionalism 88% as often as did agency training documents. On average the document that mentioned a topic less often mentioned it 65% as often as the document that mentioned it more often.

There are therefore no enormous differences between how often the two types of training documents mention any of these topics. But the agency training documents mentioned seven of the nine topics less than two-thirds as frequently as the POST course outlines did. In contrast, the agency training documents mentioned rule of law almost exactly as frequently as did POST course outlines. And as mentioned above, the agency training documents mentioned professionalism 14% more often than did POST course outlines.

Table 3.3 displays the sentiment analysis results for each of the nine topics for agency training documents and POST course outlines. For each topic and each type of document, I isolate all paragraphs that contain a term associated with each topic. I then calculate the sentiment (polarity) of each paragraph based on the QDAP[73] dictionary. Table 3.3 displays the average sentiment and standard deviation of sentiment for each topic and each document type, along with the number of paragraphs containing a term that is relevant to that topic. The fifth column displays the result of the t-test comparing the sentiment values for each type of document for each topic, and whether the difference in sentiment values is statistically significant.

Table 3.3: Sentiment Analysis on Nine Topics

	Agency Training Documents		POST Course Outlines		
	Mean [SD]	N	Mean [SD]	N	t-test
Community Policing	0.112 [0.003]	2235	0.117 [0.003]	2944	-0.005
Culture and Identity	0.024 [0.006]	581	0.010 [0.006]	788	0.015
Family Conflict	0.000 [0.009]	195	-0.012 [0.011]	285	0.011
Force and Weapons	0.021 [0.003]	2225	0.032 [0.003]	2439	-0.011**
Mental and Physical Health	0.006 [0.006]	588	0.032 [0.003]	2439	-0.026***
Professionalism	0.095 [0.002]	3406	0.129 [0.003]	2157	-0.035***
Rights	0.078 [0.003]	1395	0.095 [0.004]	1554	-0.017***
Rule of Law	0.079 [0.003]	2037	0.087 [0.004]	1473	-0.008*
Sex-Related Offenses	-0.008 [0.006]	558	-0.004 [0.006]	795	-0.004

This table displays the average level of sentiment for paragraphs containing terms related to each of the topics on the left. Sentiment levels are restricted from -1 to 1, with positive numbers reflecting positive sentiment and negative numbers reflecting negative sentiment. Significance: * 0.10 ** 0.05 *** 0.01

[73] This is the Quantitative Discourse Analysis Package in R.

Of the nine topics, there is no topic for which the average sentiment is negative for one type of document but positive for the other type of document. Family conflict and sex-related offenses result in negative sentiment (precisely zero for family conflict for agency training documents), and the other topics result in positive sentiment—which might be expected from the subject matter of family conflict and sex-related offenses.

In general the POST course outlines tend to use more positive language for the nine topics than do the agency training documents. This could simply reflect a difference in tone between training documents and course outlines. However, the average sentiment value across all paragraphs for non-POST documents is actually higher than the average sentiment value across all paragraphs for POST documents (0.040 for non-POST documents compared to 0.005 for POST documents). So it is not the case that POST course outlines are simply more positive in attitude than non-POST training documents. Based on the t-test, POST course outlines have a statistically significant more favorable sentiment than agency training documents for force and weapons, mental and physical health, professionalism, rights, and rule of law (with the difference for rule of law significant at the 10% level).

The sentiment analysis suggests that, relative to agency training programs, POST training programs may encourage officers to have higher regard for professionalism, the rights of individuals, and the rule of law, and to take issues related to weapons, force and the mental/physical health of individuals seriously. This difference in sentiment values towards these topics may lead to differences in how officers trained using POST funding operate on the field. This in turn may lead to differences in the number of complaints filed against the agency, how many of the complaints are sustained, and the prevalence of police shootings involving the agency. I assess the relationship between POST revenue and policing outcomes in the following section.

3.4.2 Complaints Reported and Sustained

The main purpose of this paper is to assess how the amount and type of police training affect law enforcement outcomes related to police misconduct and use of force. I begin with an analysis of the effect of increased POST revenue on the per officer number of complaints reported against and sustained by each law enforcement agency.

Table 3.4 below provides the regression results for the specification given in Equation (1), modeling the per-officer number of complaints reported and sustained against POST revenue received and three years of lags.

Table 3.4: Civilian and Discrimination Complaints Per 100 Officers

	Civilian Complaints		Discrimination Complaints	
	Reported	Sustained	Reported	Sustained
Per Officer Police Expenditures	-0.751	-0.817**	-0.0803	0.0186
($100,000s)	(1.93)	(0.316)	(0.587)	(0.0310)
Police Officers Per 1000 Residents	-27.3***	-2.10	0.535	0.0558
	(7.27)	(3.74)	(3.70)	(0.116)
Typical Home Price ($100,000s)	5.25	0.543*	-0.131	-0.0334
	(3.10)	(0.266)	(0.568)	(0.0280)
Unemployment	-0.605	-0.0999	0.543	-0.0413
	(1.35)	(0.362)	(0.395)	(0.0358)
Per Officer Training Expenditures	-0.751***	-0.0656	0.0802	0.0137
($1000s)	(0.273)	(0.0993)	(0.0888)	(0.0130)
Per Officer Training Expenditures	-0.577	-0.0594	-0.454	0.00233
($1000s) 1 Year Lag	(0.535)	(0.125)	(0.288)	(0.00547)
Per Officer Training Expenditures	0.435	-0.278***	-0.0426	-0.0155***
($1000s) 2 Year Lag	(0.483)	(0.0949)	(0.105)	(0.00564)
Per Officer Training Expenditures	-1.53**	-0.00417	-0.184	-0.0157
($1000s) 3 Year Lag	(0.636)	(0.117)	(0.153)	(0.0157)
Per Officer POST Revenue ($100s)	-0.0944	-0.0324*	0.0353	0.000401
	(0.0854)	(0.0183)	(0.0542)	(0.000869)
Per Officer POST Revenue ($100s) 1	-0.0535	-0.0413*	-0.0483	-0.000996
Year Lag	(0.0792)	(0.0218)	(0.0418)	(0.00102)
Per Officer POST Revenue ($100s) 2	0.353	0.159	0.0176	0.00486
Year Lag	(0.423)	(0.117)	(0.102)	(0.00471)
Per Officer POST Revenue ($100s) 3	-0.247	-0.0365	0.0529	0.0145
Year Lag	(0.502)	(0.0808)	(0.102)	(0.00918)
Mean Value	12.3	1.31	1.29	0.0110
Agency Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
N	253	253	253	253
Adjusted R^2	0.8430	0.4166	0.7312	0.0850

Robust standard errors clustered by county are used in each specification. Observations are weighted by the town/county's population. Significance: * 0.10 ** 0.05 *** 0.01

Figures 3.3 and 3.4 below graph the regression results from Table 3.4. Figure 3.3 displays the estimated effect of an increase in POST revenue on civilian and discrimination complaints. The top-left panel displays the estimated impact of a $100-per-officer increase in POST revenue on civilian complaints reported, and the top-right panel displays the estimated effect of this increase on civilian complaints sustained. The bottom-left panel shows the estimated effect of this increase on discrimination complaints reported, and the bottom-right panel shows the estimated effect of this increase on discrimination complaints sustained. Figure 3.4 is analogous to Figure 3.3 for a $1,000-per-officer increase in training expenditures.

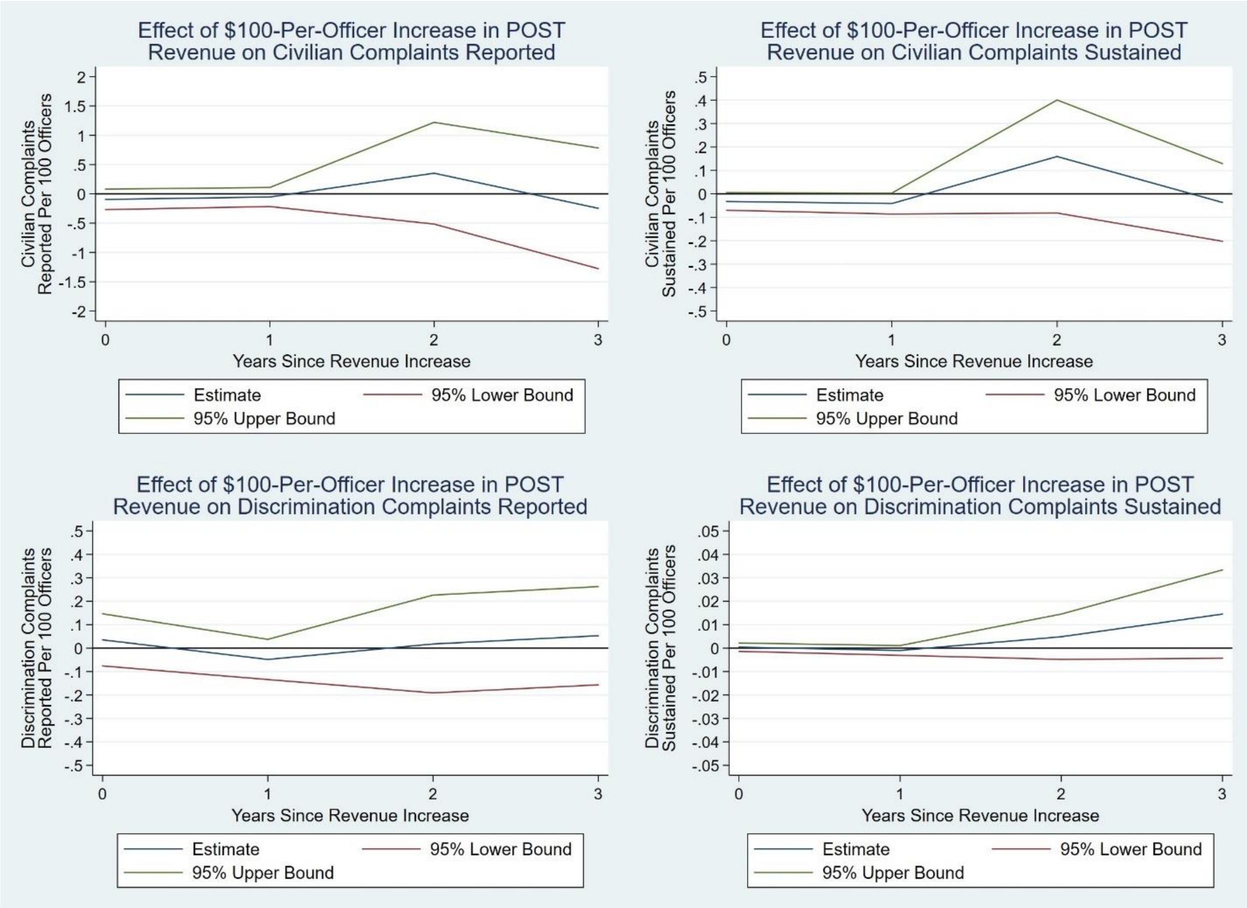

Figure 3.3: Effect of $100-Per-Officer Increase in POST Revenue on Complaints Reported and Sustained

Based on Figure 3.3, an increase in POST funding does not significantly affect reported civilian complaints. There is some evidence that an increase in POST funding causes civilian complaints sustained to decrease slightly for two years (statistically significant at the 10% significance level). However, sustained civilian complaints increase more substantially in the third year. Over the four years, the total estimated effect of a $100-per-officer increase in POST revenue is a decrease in reported civilian complaints of 0.0105 complaints per 100 officers per year on average (approximately 0.085% of the mean value of reported civilian complaints), and an increase in sustained civilian complaints of 0.0123 complaints per 100 officers per year on average (approximately 0.939% of the mean value of sustained civilian complaints).

There is also no clear evidence that an increase in POST funding significantly affects discrimination complaints reported or sustained. None of the coefficient estimates on POST revenue or its lags are statistically significant at any standard significance level. A $100-per-officer increase in POST revenue is correlated on average with an increase in reported discrimination complaints of 0.0144 complaints per 100 officers per year over the four years, which is roughly 1.12% of the mean value of reported discrimination complaints. Notably, the same increase in POST revenue is correlated with, on average, an increase in sustained discrimination complaints of 0.00469 complaints per 100 officers per year over the four years, which is roughly 42.6% of the mean value of sustained discrimination complaints. This incredibly large increase is primarily driven by the large but imprecise coefficient estimate associated with the three-year-lag of POST revenue. Although there is no clear relationship between POST revenue and either category of reported or sustained complaints, the massive increase in sustained discrimination complaints may lead agencies that have received significant POST training to sustain more complaints that allege some form of discrimination by a police officer. Alternatively, there may be no true effect.

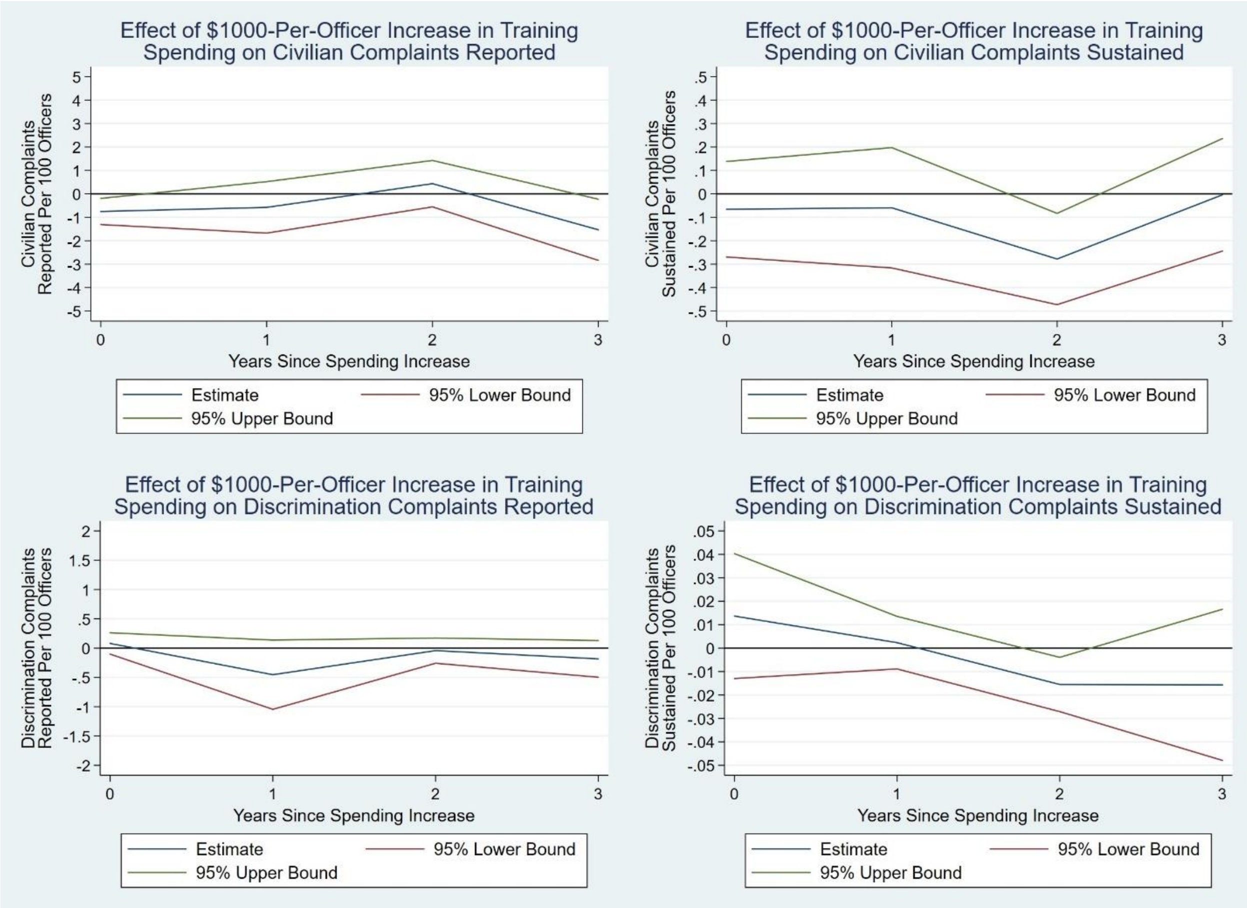

Figure 3.4: Effect of $1000-Per-Officer Increase in Training Expenditures on Complaints Reported and Sustained

Based on Figure 3.4, an increase in total training expenditures is strongly correlated with a reduction in both reported and sustained civilian complaints. An increase in training expenditures is correlated with a decrease in civilian complaints reported in the year of the increase (statistically significant at the 1% level) and three years later (significant at the 5% level). Likewise, an increase in training expenditures is correlated with a decrease in civilian complaints sustained two years after the increase (1% significance level). On average over the four years, a $1,000-per-officer increase in training expenditures is correlated with a decrease in civilian complaints reported of 0.606 complaints per 100 officers per year (approximately 4.94% of the mean value), and the same increase is correlated with a decrease in civilian complaints sustained of 0.102 complaints per 100 officers per year (roughly 7.77% of the mean value).

Meanwhile, a $1,000 increase in total training expenditures is correlated with a decrease in discrimination complaints sustained of 0.0155 complaints per 100 officers two years after the increase (significant at the 1% significance level). On average over the four years, a $1,000-per-officer increase in training expenditures is correlated with a decrease in discrimination complaints sustained of 0.00379 complaints per 100 officers per year, roughly 34.4% of the mean value of discrimination complaints sustained. And although there is no statistically significant effect of total training expenditures on discrimination complaints reported, the point estimates suggest that over the four years a $1,000-per-officer increase in training expenditures is correlated with a decrease in discrimination complaints reported of 0.150 complaints per 100 officers per year on average (roughly 11.7% of the mean value of discrimination complaints reported). The change in discrimination complaints sustained is incredibly large in magnitude, even though only one coefficient estimate is statistically distinguishable from zero.

A brief caution on causal analysis is warranted here. The OLS results for the effect of training expenditures on complaints reported and sustained should be interpreted with significant caution. Changes in training expenditures may be made in conjunction with other changes that drive the results, and there is no control for these other contemporaneous changes. This is likely less of a concern in the analysis for POST revenue because the inclusion of overall training expenditures controls for many of the other contemporaneous changes that may introduce bias in the coefficient estimates for POST revenue.

The specifications used in this analysis all include four regressors for training expenditures (one for the current value of training expenditures and three lags), as well as four regressors for POST revenue. To guard against Type I error, one can apply a Bonferroni adjustment, which involves dividing the α value in the regression[74] by the number of tests (four here based on the present value and the three lags). Applying the Bonferroni correction and dividing the α value in each regression by four, the statistically significant results are as follows: (1) an increase in training expenditures is correlated with a decrease in civilian complaints reported in the current year (10% significance) and in three years (10% significance); (2) an increase in training expenditures is correlated with a decrease in civilian complaints sustained in two years (5% significance); and (3) an increase in training expenditures is correlated with a decrease in discrimination complaints sustained in two years (10% significance). There is no statistically significant effect of increased POST funding on complaints reported or sustained.

Overall, the results provide some evidence that increased training expenditures are correlated with a reduction in civilian complaints reported and sustained, as well as a reduction in discrimination complaints sustained. There is only modest evidence that increased training from POST facilities and programs has any effect on any category of complaints reported or sustained. Increased POST revenue may lead to a temporary decrease in civilian complaints sustained in the year of the increase and one year later, but the total estimated effect of an increase in POST revenue on civilian complaints sustained over the four years is actually slightly positive.

3.4.3 Police Shootings

Lastly, I examine the effect of increased POST revenue on police shootings. Table 3.5 below provides the regression results for the specification given in Equation (1), modeling the number of police shootings against POST revenue, along with three years of lags.

[74] The α value is the cutoff for significance. For example, for the 5% significance level, the α value is 0.05.

Table 3.5: Police Shootings

	Per 100 Officers		Per 100,000 Residents	
Police Expenditures	0.0251	0.0365	-0.205	-0.183
	(0.201)	(0.298)	(0.213)	(0.295)
Police Officers Per 1000 Residents	-1.05	-0.979	-0.374	-0.372
	(1.04)	(1.14)	(1.29)	(1.23)
Typical Home Price ($100,000s)	-0.130*	-0.114	-0.206**	-0.195
	(0.0752)	(0.0969)	(0.0940)	(0.132)
Unemployment	-0.0422	-0.00234	-0.0753	-0.0499
	(0.113)	(0.201)	(0.0922)	(0.127)
Training Expenditures	0.0264	0.0315	0.0478	0.0487
	(0.0322)	(0.0346)	(0.0455)	(0.0470)
Training Expenditures 1 Year Lag	0.00813	0.0101	0.0264	0.0286
	(0.0372)	(0.0399)	(0.0327)	(0.0326)
Training Expenditures 2 Year Lag	0.0693*	0.0661*	0.0803**	0.0739**
	(0.0362)	(0.0368)	(0.0298)	(0.0323)
Training Expenditures 3 Year Lag	-0.0309	-0.0246	0.0241	0.0265
	(0.0621)	(0.0644)	(0.0618)	(0.0646)
POST Revenue	-0.000507	0.00328	-0.0111	0.00800
	(0.0256)	(0.0311)	(0.166)	(0.181)
POST Revenue 1 Year Lag	-0.0514	-0.0501	-0.411	-0.400
	(0.0332)	(0.0300)	(0.320)	(0.291)
POST Revenue 2 Year Lag	-0.0121	-0.0175	-0.196	-0.211
	(0.0361)	(0.0380)	(0.304)	(0.299)
POST Revenue 3 Year Lag	-0.0106	-0.0151	-0.189	-0.206
	(0.0391)	(0.0385)	(0.308)	(0.307)
Mean Value	0.721	0.721	0.771	0.771
Agency Fixed Effects	✓	✓	✓	✓
Year Fixed Effects		✓		✓
N	229	229	229	229
Adjusted R^2	0.1460	0.1328	0.2435	0.2318

Robust standard errors clustered by county are used in each specification. Observations are weighted by the population of the town or county over which the agency exercises authority. In the first and second columns, police expenditures, training expenditures, and POST funding are all per-officer. Police expenditures are denoted in hundreds of thousands of dollars, training expenditures are denoted in thousands of dollars, and POST revenue is denoted in hundreds of dollars. In the third and fourth columns, these variables are all per capita. Police expenditures are denoted in hundreds of dollars, while training expenditures and POST funding are denoted simply in dollars. Significance: * 0.10 ** 0.05 *** 0.01

Figure 3.5 below illustrates the estimated impact of an increase in POST revenue and training expenditures on police shootings. Figure 3.5 provides the coefficient estimates and 95% confidence bands based on the results shown in column (2) of Table 3.5, where the dependent variable is police shootings per 100 officers and which includes year fixed effects.

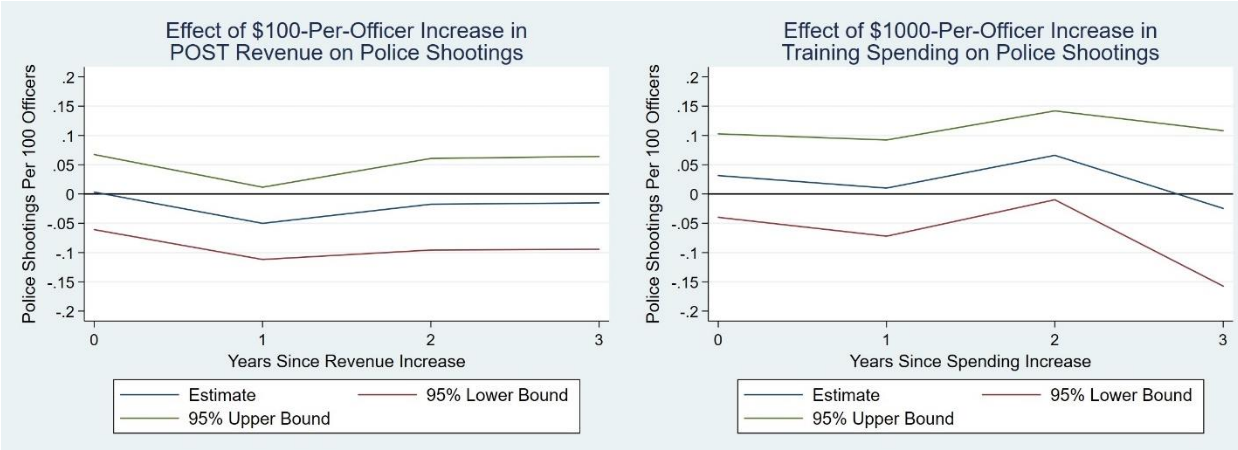

Figure 3.5: Effect of Training on Police Shootings

The point estimates for POST revenue and its lags suggest that an increase in POST funding decreases police shootings in each of the following three years, but none of the effects are statistically distinguishable from zero. On average over the four years, a $100-per-officer increase in POST funding decreases police shootings by 0.0199 shootings per 100 officers per year (approximately 2.75% of the mean value of shootings). There is some evidence that a $1,000-per-officer increase in training expenditures leads to an increase in police shootings of 0.0661 shootings per 100 officers two years after the expenditures (significant at the 10% significance level). Over the four years, a $1,000-per-officer increase in training expenditures is estimated to increase police shootings by 0.0208 shootings per 100 officers per year on average (roughly 2.88% of the mean value). However, applying the Bonferroni correction here and dividing the α value in each regression by four, none of the results are statistically significant at any standard level of significance. And as discussed in the previous subsection, this analysis does not control for other changes that may occur contemporaneously with changes in training expenditures that may drive changes in police shootings. Thus, the coefficient estimates for training expenditures should be interpreted as reflecting correlation and not causation.

It is not immediately obvious why an increase in POST funding is correlated with a decrease in police shootings, nor why an increase in total training expenditures is correlated with an increase in shootings. However, as Table 3.2 shows, POST course outlines appear to place more emphasis on the topic of weapons and force than county and local agency training documents. POST course outlines mention the topic of weapons and force approximately 52% more frequently than agency training documents. As Table 3.3 shows, force and weapons have a higher average sentiment value in POST course outlines than in local and county agency training documents. Differences in sentiment towards other topics (POST course outlines have higher average sentiment values towards mental/physical health, professionalism, rights, and the rule of law) could also lead to changes in behavior that result in lower levels of police shootings. Alternatively, the results could simply indicate that changes in POST revenue and training expenditures have no discernable effect on police shootings.

3.4.4 Political Mechanisms

In assessing the effect of increased POST revenue on policing outcomes, I control for the total amount of police training expenditures for each agency, and so the analysis provides credible causal inference about the effect of substituting $100 per officer away from non-POST training programs and towards POST training programs. However, one of the most prominent threats to identification in this analysis relates to potential political endogeneity. It may be that increased reliance on POST revenue occurs simultaneously with other police reforms. And it may be that these other police reforms drive the changes in policing outcomes that I associate with increased POST revenue.

This subsection tests this potential threat to identification in two ways. First, I examine if changes in POST revenues occur simultaneously with the introduction of policing-related ballot measures, including those bond measures that are specifically addressed at police reform. Second, I examine if the election of a new individual as the county sheriff precedes changes in police training expenditures or POST revenue.

Table 3.6 below provides the regression results for the specification given in Equation (2), modeling per-officer POST revenue received against an indicator variable for ballot measure introduction, along with two years of leads and two years of lags for this indicator variable. The indicator variable codes for, alternatively, whether any police-related ballot measure was introduced in the town or county served by the agency, and whether any ballot measure specifically addressed at police reform was introduced in the town or county.

Table 3.6: Effect of Ballot Measure Introduction on POST Funding

Dependent variable: Per-Officer POST Funding (Hundreds of Dollars)		
	All Measures Related to Policing	Measures Related to Police Reform
Typical Home Price ($100,000s)	-0.346*	-0.362**
	(0.165)	(0.165)
Unemployment	-0.170	-0.146
	(0.207)	(0.204)
≥ 1 Ballot Measure 2 Year Lead	-0.837***	-1.12***
	(0.299)	(0.397)
≥ 1 Ballot Measure 1 Year Lead	-0.304	-0.799
	(0.393)	(0.542)
≥ 1 Ballot Measure	-0.521	-1.16*
	(0.482)	(0.657)
≥ 1 Ballot Measure 1 Year Lag	-0.145	-0.412
	(0.621)	(0.598)
≥ 1 Ballot Measure 2 Year Lag	1.06	1.78
	(1.08)	(1.55)
Mean Value	3.90	3.90
Agency Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
N	2,581	2,581
Adjusted R^2	0.6867	0.6876

Robust standard errors clustered by county are used in each specification. Observations are weighted by the population of the town or county over which the agency exercises authority. During the period and for the agencies included in this analysis, there are nineteen measures related to policing, and five measures related to police reform. Significance: * 0.10 ** 0.05 *** 0.01

None of the coefficients for the lags are statistically different from zero, which suggests that the introduction of these policing-related ballot measures are not causing the results from Tables 3.4 and 3.5. However, it is important to note the large coefficient estimates for the effect of bond measure introduction on POST revenue received two years after bond measure introduction. Although it is statistically indistinguishable from zero, the large coefficient estimate suggests that introduction of a bond measure may result in greater POST revenue received several years later. Two explanations for this finding are plausible. First, there may be a change in crime, policing, or some other factor that leads to the introduction of a policing-related ballot measure, and also leads the police agency to rely more on POST revenue. Second, it could be that the introduction and passage of ballot measures leads the policing agency to need more POST funding, which they seek roughly two years later.

Furthermore, the statistically significant result for the two-year lead indicates that policing-related bond measures, and bond measures specifically related to police reform, are more likely to be introduced two years following a decrease in per capita POST revenue. There is also some evidence that the introduction of a ballot measure related to police reform is correlated with a decrease in per capita POST revenue received by the agency. Increases in POST revenue appear to be negatively correlated with police shootings in the three years following the increase in POST revenue. Based on Table 3.6, it could be that when POST revenue increases, police conduct improves, and there is a smaller perceived need to address policing through ballot measures. Alternatively, when POST revenue decreases, police conduct may worsen, and there may be a greater perceived need to address policing through the ballot measure process. However, it is important to note that the results above are based on nineteen measures related to policing, and five measures specifically related to police reform. Thus, the results could be driven by the small sample size of relevant ballot measures.

Next, I assess how the election of a new sheriff affects police training expenditures and POST revenue received. Table 3.7 below provides regression results for the specification given in Equation (4), modeling per-officer police training expenditures/POST revenue received against an indicator variable for whether a new sheriff was elected, along with four years of lags.

Table 3.7: Election of New Sheriff

	Per-Officer Training Expenditures ($1000s)		Per-Officer POST Revenue ($100s)	
Typical Home Price ($100,000s)	0.348*	0.0544	0.549	1.02
	(0.179)	(0.149)	(0.510)	(0.778)
Unemployment	-0.00513	-0.115	0.212**	-0.475*
	(0.0231)	(0.0861)	(0.0915)	(0.210)
New Sheriff Elected	1.43***	1.45***	0.496	0.488
	(0.197)	(0.221)	(0.476)	(0.532)
New Sheriff Elected 1 Year Lag	1.42***	1.16***	0.359	0.0415
	(0.302)	(0.245)	(0.502)	(0.507)
New Sheriff Elected 2 Year Lag	1.40***	0.934**	0.567	0.698
	(0.416)	(0.384)	(0.572)	(0.541)
New Sheriff Elected 3 Year Lag	1.45***	1.26***	0.739*	0.741
	(0.318)	(0.190)	(0.413)	(0.489)
New Sheriff Elected 4 Year Lag	-0.250	-0.375	-0.0910	-0.234
	(0.277)	(0.334)	(0.608)	(0.395)
Mean Value	3.74	3.74	3.90	3.90
Agency Fixed Effects	✓	✓	✓	✓
Year Fixed Effects		✓		✓
N	466	466	2,581	2,581
Adjusted R^2	0.9636	0.9650	0.7318	0.7448

Robust standard errors clustered by county are used in each specification. Observations are weighted by the county’s population. During the period included in this analysis, there are ten newly elected sheriffs for agencies covered in the analysis on training expenditures, and thirty-two newly elected sheriffs for agencies covered in the analysis on POST revenue. Significance: * 0.10 ** 0.05 *** 0.01

There is strong evidence that the election of a new sheriff is correlated with an increase in per-officer training expenditures in that year and in the next three years. Based on the specification given in the second column, a new sheriff leads to an increase in per-officer training expenditures of $1,446 in the current year, $1,158 one year later, $934 two years later, and $1,263 three years later (all statistically significant at the 5% significance level or better). But the effect on POST revenue received is ambiguous. There is some evidence that a new sheriff leads to an increase in POST revenue received of $74 per officer three years later (significant at the 10% level). However, no other estimated effect for POST revenue is statistically significant at any standard significance level.

Overall, this analysis suggests that there is only modest evidence that political factors are driving the estimated effect of POST revenue received on police complaints and police shootings. But the analysis does indicate that political factors are indeed correlated with changes in police training expenditures. Thus, the coefficient estimates for the effect of police training expenditures on police complaints and police shootings should be interpreted with caution.

3.5 Discussion

This paper examined how the amount and type of police training affects various law enforcement outcomes related to police complaints and use of force. Relative to local and county training documents, training courses that are administered by the California POST Commission tend to have more positive sentiment towards five topics: (1) force and weapons, (2) mental and physical health, (3) professionalism, (4) rights, and (5) rule of law. Increased training expenditures are associated with decreases in reported and sustained complaints against law enforcement agencies, though increased training expenditures are associated with increases in police shootings. Increased POST funding may lead to a temporary decrease in civilian complaints sustained, but the estimated effect of increased POST funding on civilian complaints sustained over four years is actually positive. Increased POST funding is followed by a decrease in police shootings in each of the following three years, though these results are statistically indistinguishable from zero. Although the exact cause of this negative correlation is unclear, the fact that POST training appears more likely to prioritize coverage of mental and physical health, professionalism, rights, and the rule of law may lead to de-escalation and a reduction in the use of fatal force by officers.

I find evidence that changes in police training expenditures appear contemporaneously with the election of new police department leadership, and so other changes occurring within the department may be driving the estimated effects of increased training expenditures on the studied outcomes. However, there is significantly less evidence that changes in agency leadership are occurring contemporaneously with changes in the amount of POST revenue received by the agency. And while there is some evidence that changes in the introduction of policing-related ballot measures are occurring contemporaneously with changes in POST revenue received, the evidence indicates that there are fewer policing related ballot measures introduced two years after an increase in POST revenue received. This suggests that after the amount of POST revenue received by the agency increases, there is a smaller perceived need to address policing and police reform through the ballot measure process, perhaps due to improved conduct of police officers following the increased reliance on POST training programs.

In light of the increased attention that police training has received in recent years, future research in this area is promising. This paper focused on identifying the association that exists between the type of program providing police training and law enforcement outcomes. As discussed earlier, this paper does not identify the effect of increased training expenditures on law enforcement outcomes, and so the causal inference is limited to the effect of increased POST revenue on these outcomes. Future researchers may search for exogenous variation in total training expenditures to more credibly identify the causal effect of increased training expenditures on law enforcement outcomes. Furthermore, as policing outcomes for more years and more agencies becomes available, future research should take advantage of this new data and attempt to more precisely estimate the effect of increased training funding and training expenditures on law enforcement outcomes. In sum, this paper provides initial evidence that the type of training that law enforcement personnel receive may have an effect on outcomes related to the use of force by officers, and policymakers may consider additional or better training as a means to improve law enforcement outcomes and possibly perceptions of the police.

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