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Essays on Regional Amenities and Public Policies

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ABSTRACT

ESSAYS ON REGIONAL AMENITIES AND PUBLIC POLICIES

By

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This dissertation investigates how alterations in government policies affect the level of, and access to, public amenities, and how outcomes vary across space. The first essay sheds light on whether the recentralization of political institutions in Russia affected the provision of regional public services. First, I exploit regional variation in governors' party affiliation to assess the impact of a uniform change in political institutions towards more centralization on the level of public services provision across states. Second, I investigate whether the combined effect, recentralization and party affiliation, is different among local and global public services. I find that a change in the region's affiliation newly aligning with the central government party induced by the policy change increases the level of global public services by 1-2 percent. However, I find no such effect on the provision of local public services.

The second essay is joint work with Carlianne Patrick. We exploit a unique characteristic of 10 charter schools in the metropolitan Atlanta area to identify property value capitalization of charter schools. Each of the 10 charter schools has two priority zones: households located in priority zone one have a higher probability of admission than households located in priority zone two. This study exploits spatial variation in the likelihood of attending a charter school between priority zone one and two to identify their effect on single-family home values using annual data on housing transactions. Our results indicate that prices increased by 6-8% for priority one zone homes compared to priority two zone homes after the opening of a new charter school. We also

find that the priority one zone capitalization increases as the home's traditional public school performs worse.

The third essay is joint work with Benjamin Ukert. We estimate the causal effect of the Australian National Firearms Agreement on firearm mortality. Our identification strategy relies on state variation in the pre-NFA firearm death rates in 1994-1996. The results suggest that the NFA decreased the total firearm death rate by 60%. The reduction in the total firearm death rate is driven by large decreases in the firearm homicide and firearm suicide rates, while we find no changes in the accidental and undetermined intent firearm death rate.

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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Introduction

Communities are heavily influenced by a variety of public policies and institutions. In this dissertation I explore how alterations in government policies affect the level of, or access to, public amenities, and how outcomes vary across space.

In the first essay I evaluate whether the recentralization of regional political institutions in Russia had significant effects on the provision of regional public services. Specifically, I look at the new legislation that replaced popular elections of regional governors with direct presidential appointments in December of 2004. I contribute to existing literature in a number of ways: first, I exploit novel data on regional variation in governors' party affiliation (i.e. in opposition to or aligned with the central administration) to assess the impact of a uniform change in regional political institutions towards more centralization on the level of public services provision across states. In addition, I investigate whether the combined effect, recentralization and party affiliation, varies across types of public services with respect to their interregional spillover effects. I find that a change in the region's affiliation newly aligning with the central government party induced by the policy change increased the level of high-spillover public services, such as education and healthcare outcomes, by 1 and 2-6%, respectively. I also find that the provision of public services without interregional spillovers, such as public utilities and public transportation, was not significantly affected by the region's realignment with the central government party.

The second essay is joint work with Carlianne Patrick. This work complements literature on school choice and its effects on housing prices. We exploit a unique feature of ten charter schools in the metropolitan Atlanta area to identify their property value capitalization. The charter schools in our study designate small geographic areas within their attendance areas as

priority zones. Each charter school in our sample has two priority zones: families residing in priority zone one have a higher probability of admission than families residing in priority zone two. We use spatial variation in the charter school enrollment probabilities between priority zone one and two to identify the effect on single-family home values using annual data on residential transactions. We find that parents are willing to pay about 6-8% - or $8,845-$13,470 – more for houses located in priority zone one within 0.3 miles from the border with priority zone two, following the charter school opening. The capitalization effects for houses located in priority zone one within 0.1 miles and 0.5 miles from the border with priority zone two are similar. In addition, we find that the priority one zone capitalization increases as the home's traditional public school performs worse.

The third essay is joint work with Benjamin Ukert. We evaluate the Australian National Firearms Agreement (NFA), which introduced comprehensive national gun regulations and a gun buyback program in May of 1996 after a mass shooting that killed 35 people in Tasmania, Australia. This paper estimates the causal effect of the NFA on firearm and non-firearm mortality. We estimate a difference-in-differences model relying on cross-sectional variation in the pre-NFA firearm mortality rates in 1994-1996. Our approach relies on the assumption that the law was more effective in states with higher pre-1997 firearm mortality rates. Our results suggest that the NFA implementation decreased the total firearm mortality rate by 60%. The reduction in the total firearm mortality rate is predominantly driven by reductions in the firearm homicide rates, 96%, and firearm suicide rates, 50%, while we find no changes in the accidental and undetermined intent firearm mortality rate. We also find evidence that the non-firearm mortality rate decreased, which is driven by a reduction in the non-firearm accidents and undetermined intent rate. Finally, using regression discontinuity design, and applying monthly

robbery data for the state of New South Wales, we find that non-firearm and non-weapon robberies have increased by 35 and 20%, respectively, following the passage of the NFA, which may suggest substitution away from firearms.

Chapter I: Governors' Party Affiliation and Public Services: A Difference-in-Differences Analysis of Recentralization in Russia

1. Introduction

Over the past several decades the impact of political institutions on the provision of public services has been one of the most contested topics in political economy. Decentralization improves quality of local service provision due to the information advantage of local governments (through reduction in matching and signaling costs), their accountability to constituents (Oates 1972), and their control over public spending (Brennan and Buchanan 1980). However, political decentralization is not without downsides. It may increase administrative and coordination costs (Breton and Scott 1978). Local governments are also more susceptible to "capture by local elite" than are central governments (Bardhan and Mookherjee 2000, Bardhan 2002, Blanchard and Schleifer 2001). Finally, they can also suffer from lack of talented and qualified people in key administrative positions increasing poor-quality decision making (Bardhan 2002, Fisman and Gatti 2002, Brueckner 2000).

The aforementioned downsides of decentralization have led to recentralization of political power in some developing nations (Dickovick 2011). Empirical evidence on the effects of recentralization on subnational provision of public services, however, is limited since recentralization is a relatively recent phenomena, and its analysis is difficult.[1] As a result, further research on recentralization is necessary especially if more nations choose to recentralize in the future.

In this paper I contribute to the empirical literature on recentralization by analyzing what happens to the level of provision of public services when a nation's regional political institutions

[1] Most empirical testing has been done using either cross-country studies, or country-level panel studies, both of which lack presence of a uniform change in political institutions across all observations. In addition, many studies lack data on regional-level recentralization.

are transformed from being democratically decentralized to democratically centralized, using the change in political institutions that took place in the Russian Federation in 2004. Democratic decentralization is expressed in the popular election of regional governors, while democratic centralization is expressed in the presidential appointment of governors. Notably, I evaluate whether the effect of recentralization on the provision of public services is a function of the region's party affiliation (central or opposition) and the type of public services in question, with an emphasis on the degree of interregional spillover of services. The analysis is twofold. First, I evaluate whether the regions with a longer history of affiliation with a strong central party fair differently after recentralization than regions whose affiliation with the central party was induced just recently by the shift towards more centralized political institutions. Second, I investigate whether the combined effect, recentralization and party affiliation, is different across types of public services with respect to their interregional spillover effects.

Why would party affiliation have a differential effect on public services with higher vs. lower spillover effects? The central government as opposed to a local government tends to provide public services if those public services generate large spillover effects across jurisdictions (Inman and Rubinfeld 1997). The reason is that local governments aim to satisfy only local demand for public spending since their future reelection depends primarily on local constituents' satisfaction with executive performance. As a result, public services, whose benefits cross municipal and state lines, are often underprovided. However, Riker (1964) argues that the

presence of strong central parties in regional government can effectively internalize spillovers by aligning regional public spending with national interests.[23]

Using data on history of governors' party affiliation between 2000 and 2012, my empirical strategy compares regions affiliated with central and opposition parties. I present estimates identified by the difference in pre- and post- recentralization levels of public services provision. I find that a change in the region's affiliation from opposition to central party induced by the shift towards more centralized political institutions increases provision of public services with larger spillover effects, such as education and healthcare. Specifically, school enrollment increased by roughly 1 percent, population-to-doctor ratio decreased by close to 2 percent, and infant mortality decreased by almost 6 percent.[4] Second, the provision of public services with low spillover effects, such as public utilities and transportation, did not appear to significantly depend on pre- and post-centralization party affiliation.[5] The results are consistent with hypothesis that centralized party affiliation should primarily affect public services with substantial interregional spillovers.

The remainder of the paper is organized as follows. Section 2 discusses the existing literature on both decentralized and centralized political institutions. Section 3 describes in more detail the 2004 change in governors' appointment procedure, the Russian's central party, and the opposing parties. Section 4 presents the theory on the influence of the new governors' appointment statute

[2] Riker (1964) describes presence of strong central parties as an alternative to centralization when it comes to internalizing interregional spillovers.
[3] A number of previous studies augment decentralization with presence of strong central parties (Enikolopov and Zhyravskaya 2007, Ponce-Rodriguez et al. 2012, Lago-Penas and Lago-Penas 2006). They find their positive joint impact on public goods' provision, particularly in existence of spillover effects.
[4] Educational and health outcomes, in addition to the level of sulfur dioxide emissions, are my proxies for public services with higher interregional spillover effects.
[5] Public utilities and public transportation are my proxies for public services with lower interregional spillover effects.

on the level public service provision. Section 5 lays out the empirical approach. Section 6 describes data. Section 7 presents the main results. Section 8 concludes.

2. Literature Review

A series of previous studies have analyzed the consequences of a switch in political institutions on the provision of local public services. A group of these papers discuss practical issues regarding the decentralization of political powers, i.e. redistribution of political functions away from the central authority (Bardhan 2002, Fisman and Gatti 2002, Blanchard and Schleifer 2001, Bardhan and Mookherjee 2000, Breton and Scott, 1978). Even though conventional theory predicts more efficient provision of public services in economies with greater political decentralization, a positive relationship between political decentralization and provision of local public services may be undermined by such factors as inferior-quality decision-making at the local level, weak local accountability even in the presence of direct elections, and capture by local elites. Poor-quality decision making may result from inefficient methods of bureaucratic monitoring or lack of talented and qualified people to fill the administrative positions (Bardhan 2002, Fisman and Gatti, 2002). Political decentralization might also increase administrative and coordination costs (Breton and Scott 1978). Previous studies argue that administrative subordination, i.e. the appointment of the regional branches of the executive government by the central government, may improve the outcomes of decentralization (Blanchard and Schleifer 2001).

Another body of literature illustrates the advantages of political decentralization for public services provision (Enikolopov and Zhyravskaya 2007, Bardhan 2002, Gallego 2010, Fisman and Gatti 2002). Bardhan (2002) concludes that the correlation between decentralization and local services provision is on average positive after reviewing case studies on Bolivia, Brazil, Bangladesh, Mexico, India and a number of other developing countries. Gallego (2010) finds

that cross-country differences in education are positively affected by such political institutions as local democracy, which in turn are correlated with historic colonial factors.

An important factor that drives a positive relationship between political decentralization and public services provision in the literature is the presence of local accountability which incentivizes politicians who gain their offices through popular elections to acquire complete information on local preferences (Fisman and Gatti 2002, Enikolopov and Zhyravskaya 2007, Riker 1964).

The previous literature highlights the debate on whether some political institutions are better equipped to provide public services with significant interregional spillover effects. Some studies theorize that if spillovers reach a critical level then centralization is preferable since decentralization will lead to under-provision of those services (Besley and Coate 2003, Seabright 1996)[6]. However, empirical studies demonstrate that decentralization can succeed in efficiently allocating resources even in cases of public services with higher inter-jurisdictional spillovers (Ogawa and Wildasin 2007).

A final relevant series of studies investigates the consequences for public spending when political decentralization is enhanced by the presence of strong central parties. Previous research suggests that presence of strong political parties may help to internalize spillover effects of local public services provision even if regional political leaders are subject to popular elections (Enikolopov and Zhyravskaya 2007, Ponce-Rodriguez et al. 2012, Lago-Penas and Lago-Penas 2006). Political decentralization generates local accountability, while strong central parties incentivize elected officials to provide public services with spillover effects. Strong national parties may solve the issue of "local capture" through generating career concerns for locally

[6] Bordignon, Colombo and Galmarini (2003) add that centralization can solve the underprovision issue only under certain conditions, like absence of cooperative lobbying interests in the regions.

elected political leaders, and incentivizing them to resist local elite influence (Enikolopov and Zhyravskaya 2007). Ponce-Rodriguez et al. (2012) find that interaction of political decentralization and party centralization produces the most efficient level of public service provision. Lago-Penas and Lago-Penas (2006) argue that the degree of nationalization of party system is important in determining how maneuverable governments can be in their decisions over implementation of fiscal policy by changing the transactions cost of collective decision making.

This study contributes to the existing literature in several ways. First, I assess the impact of a uniform change in political institutions towards more centralization, in the form of regional governors' office appointments, on the level of public services provision across regions. The application of the same law to all regions of Russia allows me to avoid issues related to country-level analysis arising from within country heterogeneity in quality of government, or degree of decentralization (Cerulli and Filippetti 2014). Second, I test what role strong national parties play in changing the provision of public spending by exploiting regional variation in governors' timing and strength of affiliation with the central party. Third, I investigate whether the combined effect, recentralization and party affiliation, is different across types of public services with respect to their interregional spillover effects.

3. Historical Background

Two events contributed to the recentralization of Russia's regional political institutions: the new governors' appointment law of 2004, and the increased influence of the pro-central government United Russia (UR) party. Both events may have affected the level of public service provisions in Russia after 2004. In the first case, governors were no longer subject to popular elections and their local accountability was significantly weakened. Weak accountability might have resulted in less responsiveness to their constituents' demands leading to deterioration in

funding of public services, which may not have been a priority for the central authorities. In the second case, a higher incidence of affiliation with the UR party among governors might have increased spending on mutually beneficial public goods by lowering the bargaining cost.

3.1 New governors' appointment law

To test whether decentralization of political power leads to a different level of local public services provision than the centralization of political power (i.e., having central government directly appoint regional leaders), I use a law change that took place in 2004 with regards to how regions (states) in Russia determined their governors. Since the collapse of the Soviet Union in 1991 and up until 2005, the governors of the 89 regions of the Russian Federation were mostly elected by popular vote of their respective constituencies.[78] However, on September 13, 2004 President Vladimir Putin addressed his government with a proposal to eliminate the popular election of governors and replace it with a system of presidential appointment.

According to the proposed legislation, the governors would no longer be directly elected by popular vote, but would be nominated by the President, and confirmed by the regional assemblies. Putin's argument against the existing system of popular elections was the supposed inability of regional executive branches to successfully deal with potential crises (as demonstrated by the Beslan hostage crisis). The new bill would also give Putin the right to fire governors on the grounds of "loss of President's confidence". Additionally, regional assemblies would face dissolution if they rejected the proposed candidate three times. Governors who were elected by popular vote before the passage of the new legislation were allowed to finish serving

[7] Between 1991 and 1993 gubernatorial elections were mostly unsuccessful for Kremlin appointees. As a result, President Yeltsin appointed 45 out of 49 regional governors between 1991 and 1995. The first widespread round of governors' elections took place in 1996-1997, which led to a failure rate of more than 50% for Kremlin's incumbents (Goode 2007).

[8] Russia has 89 states according to the 1993 constitution. Overtime some of them have been merged, leaving only 83 as of January 1, 2008.

their current term. They could however appeal to the President for reappointment before their term was out (Goode 2007). [9] The bill was signed into law on December 11, 2004. The first round of appointments and reappointments began as soon as February 2005.

3.2 United Russia from 1999 to 2003

The UR party was originally named Unity, and created as a pro-presidential party just prior to the 1999 State Duma[10] elections. Its main campaign characteristics were national integrity and association and support for Putin who at the time was serving as acting President of Russia. After the 1999 elections, which were relatively successful for Unity, it merged with another party, the Fatherland-All Russia to form the UR party. In the 2003 Duma elections, UR won 68 percent of the parliament seats. It also managed to establish large majorities in the 2003 regional elections. According to Konitzer and Wegren (2006), the regional branches of UR were more loyal to the President and to the central party organization than to the elected governors, which helped strengthened the "democratic centralization." As a result, many regional governors had to either associate themselves with the presidential party, or face strong central opposition.[11] In summary, party's regional dominance was sufficient to align regional and national interests internalizing the spillover effects.

4. Theoretical Framework and Main Hypothesis

Consider an economy that consists of distinct geographically divided regions. The government of each region provides two types of public services, with higher and lower interregional spillover effects. The provision of public services with high spillover effects benefits the local population of the region, and populations of the surrounding regions. The

[9] During the first round of appointments in 2005-2006, 24 out 47 sitting governors appealed for the President's decision for nomination before their term has expired. Putin agreed to appoint all of them as governors for their respective regions. The majority of the governors who did not appeal directly to the President, and followed the nomination procedure, lost their offices (Goode 2007).

[10] The State Duma is a lower house of the Federal Assembly of Russia (Russian parliament).

[11] During the 2003 governor elections, candidates backed by the United Russia won in 81 percent of the cases (Konitzer and Wefren 2006).

provision of public services with low spillover effects benefits only the local population of the region. The question is whether different political institutions vary in the level of spending on public services with high vs. low spillovers.

An economy may choose to provide public services in a decentralized fashion, where regional governments make the expenditure decision, or in a centralized fashion, where the central government makes expenditure decisions providing uniform level of public services across all regions.[12] Besley and Coate (2003) suggest that one may compare performance of decentralized and centralized political institutions by looking at aggregate surplus of public services. The authors argue that in a decentralized system public services are under-provided in the presence of spillovers, accordingly, the surplus level is decreasing in the degree of spillovers. On the contrary, in a centralized system the level of public services is exogenous with respect to the amount of spillovers. Besley and Coate (2003) show that centralized institutions produce higher level of public services' surplus than decentralized institutions under two conditions. First, if districts are homogeneous and spillovers are present, and second, if districts are heterogeneous, and spillovers are sufficiently high. However, a centralized regime may overprovide public goods if spillover effects across districts are higher than a certain "critical" level.

Given the theoretical predictions described above, why would the passage of new legislation regarding governors' appointment lead to a different level of spending on public services across Russian states? Regions in Russia are considerably heterogeneous in their socio-economic

[12] Some studies state that the uniformity condition initially assumed by Oates does not have to be the case in a centralized system. Besley and Coate (2003) mention that theoretical and empirical evidence does not reject the hypothesis that central government might be able to provide heterogeneous amounts of public goods across regions according to local preferences. Bardhan (2002) argues that centralized government may be able to get the same precise information through local agents. However, in this study, I continue to assume uniformity for practical considerations.

characteristics, therefore the central government would be less likely to satisfy local preferences providing uniform level of public services across all regions. In addition, the absence of direct elections would weaken governors' incentives to satisfy local demand for public services reducing incentives to provide the central government with complete information on local preferences. Even in a scenario where governors provide the central government with as complete information as possible, the central government may find differentiation among regions costly due to political reasons. Similarly, even when spillover effects are large, decentralization might not necessarily lead to under-provision of public services. Studies by Ponce-Rodriguez et al. (2012) as well as Riker (1964) showed that strong central parties align political choices of local officials with national preferences, thus effectively internalizing spillover effects across jurisdictions. Since a central party seeks to maximize the number of regions it can win the elections in, its objective is to satisfy local preferences for public services across the country, which can be achieved through internalizing the spillover effects in each region. Members of the central party can achieve their objective by impacting regional governors' political careers, thus encouraging them to choose a level of public services provision that generates more surplus.

Based on the framework described above I propose to test the following hypothesis:

1. Governors' centralized appointment after the new law should be associated with an increase in provision of public services with large interregional spillover effects, for all regions in which the previously elected governor did not belong to the central party of power.
2. Governors' centralized appointment after the new law should be associated with no significant differences in provision of public services with large spillover effects, for

all regions in which the previously elected governor was already a member of the central party of power.

3. Governors' centralized appointment after the new law should be associated with insignificant changes in provision of public services without spillover effects regardless of party affiliation.

Of course the level of public services provision by subnational governments depends not only on political institutions, but more directly on regional differences in tax rates and revenues. According to Tiebout (1956), constituents sort into jurisdictions with higher or lower tax rates based on their demand for public services. As a results, regions with higher levels of public services are often regions with higher tax rates and revenues. Accordingly, one might argue that my analysis is incomplete without controlling for the differences in tax statutes across Russian regions. However, changes in the Russian tax code demonstrate why differences in subnational taxes are not an issue in the case of the Russian Federation's regional governments. The most significant change is centralization of tax revenue between 2000 and 2005.

During the 1990's, the Russian intergovernmental system of tax collection and sharing was rapidly changing, with wealthier regions, looking for more budgetary autonomy from the center, and poorer regions continuously relying on Moscow for federal grants. During the years of the Yeltsin presidency, stronger regional governors successfully lobbied for larger revenue autonomy as well as a larger share of central government taxes, such as the value added tax. Putin's administration spent his first presidential term trying to recentralize many taxes in order to increase central authority in the country. The administration introduced the new Tax Code in 2000. Its chapters were gradually enacted in 2000-2005. The new legislature prohibited introduction of any new regional or municipal taxes not outlined in the new Tax Code.

According to the new Tax Code, value added tax became recentralized once again, and regional sales taxes, which provided a significant amount of own revenues for regional governments, were eliminated. As a result, federal government's proportion of total tax revenues reached 63 percent by 2004 decreasing the share of subnational governments from 43.5 to 36.17 percent between 2000 and 2004. As a result, the new Tax Code reduced the budgetary autonomy of regional and local governments relative to 1990's (*Tax Reform in Russia* 2008).

5. Empirical Approach and Identification Strategy

There are two sources of variation that I use to test my hypotheses. First, some governors chose to be affiliated with either the central party (UR), which has had the majority of Parliament seats since 2003, or one of the opposition parties, with a consistent minority in Parliament seats (KPRF, LDPR, SR), or an inconsistent minority with the possibility of complete absence of any Parliament seats (SPS, APR, DPR, Rodina, RPSD, TU, NPSR, RNR, UDM)[13]. For my analysis, I use the governor's party affiliation as a proxy for the strength of the central party in each region. All regions in Russia are divided into three distinct groups: group 1 consists of regions with governors who affiliated themselves with the opposition party before the 2004 law change, and the UR party shortly after the change[14]; group 2 consists of regions that had governors from the UR party before and after the change in appointment law; group 3 consists of regions with governors from an opposing party before and after the change, with a later transition to UR party affiliation (see Figure 7 in Appendix 1).

Second, I use variation in the intensity of public services interregional spillovers. Based on the framework discussed in Section 4, the provision of public services with higher spillovers may benefit more from centralization than the provision of public services with weak spillovers if

[13] See Appendix 1 for more details on opposition parties.
[14] There is a regional variation in the year of change.

those goods were underprovided prior to the new bill[15]. However, there should be no effect on public services without spillovers.

My objective is to identify the average effect of political centralization on the provision of public services in regions whose affiliation with the central party was incentivized by the new appointment law. As a result, I compare the level of public services provision after centralization when the state is affiliated with the central party to the counterfactual, the level of public services provision for the same state affiliated with the opposition party at the same point in time. However, since the counterfactual is not observable, I need to find a good representation. Ideally, I would need to randomly assign party affiliation across states, and compare their average outcomes. Since a controlled randomized experiment is not possible, I turn to an alternative non-experimental method that most closely mimics a randomized trial under a set of assumptions.

In order to identify the causal relationship of interest, I use a difference-in-differences methodology[16] by exploiting variation in timing of affiliation with the central party across states. A key identification assumption in this strategy is that in the absence of regional variation in affiliation with the central party, public services provision would have trended similarly among all states--- conditional on regional fixed effects-- after the passage of the appointment law. One potential threat is that governors' decisions to change their party affiliation from opposition to the central government was correlated with the way public services were provided in their regions. In reality, it is more likely that governors' choice to change party affiliation was driven by the desire to remain or become the head of the region. Affiliation with UR served as a means to signal political loyalty to the central government, and specifically to President Putin.

[15] Though Sigman (2013) found a positive relationship between decentralization and interregional variation in provision of public services with spillover effects like environmental protection if the central government does not allow for sufficient variation across regions with heterogeneous preferences.
[16] Difference-in-differences models allow to control for time-invariant unobserved heterogeneity across regions.

Figure 1 illustrates the trends in public services' provision for regions in groups 1 and 2. The line with solid diamonds represents the average level of public services' provision in any given year among the treatment states, i.e., states where governors have change their party affiliation to UR after 2004. The vertical line reflects the year in which the new appointment law was passed. The line with solid circles shows the average level of public services' provision in any given year among states that belong to the control group, i.e., states where governors affiliated themselves with UR before 2004.

Figure 1: Trends in public services provision before and after the new appointment law

Notes: Dependent variables are on the y-axis: enrollment rate is measured as a percentage of school-age children enrolled in elementary and secondary public schools; infant mortality is measured as deaths of children under 1 year of age per 1000 live births; SO2 emissions are measured in 1,000 tons; sewage is measured as a number of central sewage systems, excluding rain drainages, located in population districts per capita; bus volume is measured in passenger-kilometers, distance traveled by passengers on buses, per capita. The line with solid diamonds represents the average annual level of public services' provision in treatment states. The vertical line shows the year (2005) in which the governors' appointment law was enacted. The line with solid circles shows the average level of public services' provision in control states.

As shown in Figure 1, treatment and control states follow somewhat similar provision trends prior to the appointment law suggesting that there were little systematic differences between the two types of states other than differences in levels, while controlling for by state fixed effects and time dummies.

The difference-in-differences approach can be specified in the following fixed effect linear regression model:

$$y_{it} = \alpha Post_{it} + \beta Party\Delta_{it} + \gamma(PostParty\ \Delta)_{it} + \delta X_{it} + \theta_i + \mu_t + \epsilon_{it} \qquad (1)$$

where the dependent variable y is a measure of a public service in region *i* and year *t*; *X* is an array of regional controls; θ_i is a fixed effect unique to state *i*, and μ_t is a time fixed effect common to all states in period *t*. The error ϵ_{it} is a state time-varying error, which is assumed to be independently distributed for all θ_i and μ_t. Since the errors might be correlated across time and space, I compute my standard errors clustered at the state level to allow for an arbitrary covariance structure within states over time.

In this model, α is the difference-in-differences estimate of the average effect of a change in governor's party affiliation in region *i* induced by the recentralization on the level of public services provision.

In the above model, the dependent variable *y* is a measure of a specific public service like healthcare, education, pollution, public utilities and transportation. I expect α parameters to be different for public services with higher spillover effects (pollution, education and healthcare) than those with lower spillover effects (public utilities and public transportation) since presence of strong central party influences regional provision of former, but not later types of services.

The vector of controls, *X*, includes the percentage of population living below the poverty threshold, real GDP per capita or income per capita, population density, urban population, percentage of school-age children, and ethnic fractionalization, which is measured as a percent of population who report belonging to a non-Russian ethnicity, and a dummy for whether the region is affiliated with the UR. The control variables try to account for the degree of decentralization as well as the rationales for potential reappointment, which may depend on the ethnic origins of a

particular area (Enikolopov and Zhyravskaya 2007, Robalino et al. 2001). Additionally, real GDP per capita, population density, and urbanization typically have large effects on the degree of decentralization (Arzaghi and Henderson 2005).

6. Data

Table 1 reports summary statistics for all the depended and independent variables included in the analysis.

Table 1: Summary Statistics

Variable	Units	N	Mean	S.D.
Dependent variables				
General public school enrollment	%	848	86.83	5.23
Pupil to teacher ratio	#	1101	11.01	1.99
SO2 emissions	1,000 ton	995	20.48	34.38
NO2 emissions	1,000 ton	1000	15.31	19.53
Infant mortality	per 1,000	1048	11.74	5.00
Population to doctor ratio	#	1111	225.41	52.05
# of heat sources per capita	#	1108	825.92	633.55
# of sewage systems per capita	#	1018	130.61	139.34
Bus volume	Passenger-km	1061	189,258.6	184,723
Paved road volume	km/area (1000 sq. km)	1035	13,239.59	14,369.37
Water cleaning per capita	1,000 cubic meters	1005	0.07	0.03
Socio-economic characteristics				
Real GDP per capita	RUB	1027	137,713.10	161,553.80
Real income per capita	RUB	929	10,912.82	8,133.40
Urbanization rate	%	1119	68.14	14.56
Population density	population/area (sq. km)	1118	178.63	1113.13
Ethnic fractionalization	%	1131	0.24	0.23
Poverty rate	%	1064	22.99	12.53
School-age children (7-16) to population ratio	%	956	0.13	0.03
Unemployment rate	%	820	8.45	6.00

The dataset on outcomes of public goods provision, governors' party affiliation, and control variables covers 91 federal regions of Russia[17] for the years 2000-2012.[18] Data are available from the Russian Federation Federal State Statistics Service (FSSS) as well as 2002 and 2010 Russian Census. My dependent variables of interest are an array of public services with and without interregional spillover effects. Public services with spillovers are environmental quality, education outcomes, and healthcare outcomes.[19] Environmental quality is measured in terms of sulfur dioxide emissions, which are linked with adverse effects on the respiratory system[20]. According to the Arctic Monitoring and Assessment Programme, the biggest sources of its emissions in Russia are fuel (coal) combustion at metallurgical and power plants. Education outcomes are measured as a percentage of school-age children enrolled in elementary and secondary public schools, and pupil-to-teacher ratio. Infant mortality is calculated as the number of deaths of children under the age of 1 per 1000 live births; population-to-doctor ratio also serves as a proxy for the effectiveness of the health care system[21] (Robalino et al. 2001). Public services without interregional spillover effects are public utilities, such as heating and sewage sources, and water treatment[22], and public transportation[23]. Public utilities are measured as a number of central sewage systems, excluding rain drainages, located in population districts per

[17] Chechen Republic is excluded from the analysis due to the Second Chechen War, which started in 1999, continued until 2003.
[18] Data for some dependent variables and covariates is missing for some of the earlier years.
[19] A large percentage of infant deaths in Russia are due to infections and parasitic diseases as well as poor environmental quality. As a result, infant mortality represents a category of healthcare provision that might exhibit interregional spillover effects (1996 WHO report).
[20] Pollution data at the regional level are scarce and published infrequently.
[21] Changes in immunization would indicate a more immediate effect on the efficiency of the health care system than infant mortality. However no pre-2005 immunization information exists at the required geographic level. In addition, infant mortality can only serve as a proxy for public services as it is really an output of a function of the quality of public services provision, and not an input into it.
[22] Perhaps, a better measure of public utilities provision is percentage of households with access to residential water or sewage systems as opposed to counts of water and sewage systems. However, data on household access to public utilities is not available prior to 2007.
[23] Measures like public street lightning or street paving represent better examples of public services with low spillovers. However, statistics on these services are not available at the required geographic level, and time frame.

capita; thousands of cubic meters of water run though sewage treatment per capita; and number of central heating systems per capita. Public transportation is measured in passenger-kilometers, which is the distance traveled by passengers on buses. In addition, transportation is measured by density of paved roads, which is number of kilometers of roads per 1,000 square kilometers. The array of dependent variables was chosen to maximize the probability that regional political leaders would have a significant impact on the provision of public goods. According to Kraan et al. (2008), regional authorities in Russia oversee 49% of total expenditures on household utilities, 59% of total expenditures on transportation, 56% of total expenditures on environmental protection, 68% of the expenditures on health care, and 26% of total expenditures on primary education.

The main independent variables of interest include a time dummy for whether a regional governor was elected or appointed, a dummy for whether a regional governor belongs to the treatment or control group, and an indicator for the governor's pre- and post- law party affiliation[24]. The time dummy takes a value of 1 for the years 2005-2012, and 0 for the years 2000-2004. There is no uniform data source for the governors' party affiliation since UR does not specify the accession of high-ranking officials to the party. As a result, I collected data on the governors' party affiliation directly from online news sources. The treatment dummy takes a value of 1 if a governor changed his party affiliation to UR between 2005 and 2007, and 0 if he has been affiliated with UR prior to 2005. Governors' affiliation is a dummy variable that takes a value of 1 if a regional governor is an official member of UR, or at least expresses a public support for it, as determined by public news announcements, and 0 if he is a member of an

[24] Since I employ those dummies to essentially measure recentralization of regional political institutions I avoid using the share of sub-national revenues/expenditures in total revenues/expenditures, which often overestimates the degree of fiscal autonomy (Stegarescu 2005).

opposition party or an independent candidate. The treatment effect of interest is expressed by the interaction of the time dummy and the treatment dummy.

7. Results

I present estimation results for Equation (1) with education, healthcare, and environmental outcomes as the dependent variable in Tables 2, 3, and 4, respectively.

Table 2: Impact of appointment law induced affiliation with the central party on education

	(1) Log (enrollment rate)	(2) Log (pupil/teacher)
Party Change * Post	0.012**	-0.023
	(0.005)	(0.018)
Socio-economic characteristics	YES	YES
R-squared	0.636	0.787
Observations	511	579

Notes: Treatment group is all states that have changed party affiliation to UR in 2005-2008. All regression include year and state fixed effects. Standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

The difference-in-differences estimator for party change has a positive and statistically significant effect on enrollment rate in all public educational institutions. The coefficient on enrollment rate indicates that regions where governors have changed their party affiliation to UR after the new appointment law increased public school enrollment. A law induced change in governor's party affiliation from opposing to central party increases public school enrollment rate by roughly 1 percent. This translates to a 0.8718 percentage point increase in enrollment or 2,000 additional enrolled students[25].

The party affiliation effect on pupil to teacher ratio, column 2, yields a similar coefficient to column 1, however it is insignificant. Statistical significance aside, the result is consistent with

[25] The average enrollment rate and student enrollment for the control states during the pre-treatment period are 87.18 and 238,712, respectively.

the hypothesis that for all regions in which the governor did not belong to the UR party prior to 2005, a governors' centralized appointment after the new statue should be associated with an increase in provision of public services with higher interregional spillover effects.

Column 1 in Table 3 shows that the difference-in-differences estimator for party change takes a positive and significant value indicating that larger volumes of sulfur dioxide (SO_2) emissions occur in regions where governors changed their party affiliation from opposing to central after 2004. According to point estimates, SO_2 emissions increased by over 20 percent in regions where governors had chosen to affiliate themselves with UR after the 2004 statue changing the average SO_2 particles emission level by roughly 4,000 tons.[26] Coefficient on NO_2 emissions is also positive, but statistically insignificant.

Table 3: Impact of appointment law induced affiliation with the central party on emissions

	(1) Log (SO2 emissions)	(2) Log (NO2 emissions)
Party Change * Post	0.235*	0.074
	(0.138)	(0.097)
Socio-economic characteristics	YES	YES
R-squared	0.338	0.076
Observations	606	613

Notes: Treatment group is all states that have changed party affiliation to UR in 2005-2008. All regression include year and state fixed effects. Standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

The results in Table 3 do not support the argument for the beneficial effects of the presence of centralized party on public services with interregional spillover effects. However, environmental pollution is often correlated with economic activity. Previous research shows that up to a certain level of per capita income, less than $8,000, growth in GDP might be associated

[26] The average SO2 emissions for the control states during the pre-treatment period are 17.3 thousand tons.

with a decline in environmental conditions (Grossman and Krueger, 1995). As a result, any potential reductions in SO2 emissions facilitated by presence of stronger central party would have been offset by growth in production accompanied by heavy SO2 emissions.[27][28] Such explanation is supported by negative and significant coefficient on unemployment rate in specification (1), which indicates that a 1 percent increase in unemployment reduces SO2 emissions by over 3 percent[29].

It is important to point out prior to interpreting results in Table 4 that the dependent variable population to doctor ratio is more likely to be immediately impacted by changes in public spending from a switch in political institutions than infant mortality (Enikolopov and Zhyravskaya 2007).[30] In addition, changes in public expenditures leading to new capital investments in hospitals would affect infant mortality with a significant time lag. Thus, given the relatively short time period of my analysis I expect to find stronger effects in the population to doctor ratio regression.

Table 4: Impact of appointment law induced affiliation with the central party on healthcare

	(1) Infant mortality	(2) Log (population/doctor)
Party Change * Post	-0.849*	-0.018**
	(0.522)	(0.009)
Socio-economic characteristics	YES	YES
R-squared	0.718	0.401
Observations	644	709

Notes: Treatment group is all states that have changed party affiliation to UR in 2005-2008. All regression include year and state fixed effects. Standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

[27] According to EPA, 73% of SO2 emissions result from fuel combustion at power plants, with remaining amount produced by other industrial facilities.
[28] Per capita GDP in Russia was under $8,000 during the sample time period (in 2000$).
[29] The Pearson's correlation coefficient between logarithm of SO2 emissions and unemployment rate is -0.25, which represents a weak negative relationship.
[30] Infant mortality is likely a function of more than just democratic institutions. It may depend on specific population characteristics, for which I do not have appropriate measures.

Table 4 shows that regions where governors had changed their affiliation from the opposition to the central party in 2005-2012 experience statistically significant reductions in infant mortality as well as population-to-doctor ratio, which is consistent with the predicted relationship. Post-2004 change in governor's affiliation to UR reduced infant mortality by 0.85 deaths under the age of 1 per 1,000 live births[31]. This translates into a 6 percent reduction in infant mortality.[32] Similarly, the population-to-doctor ratio decreased by approximately 2 percent.

The estimation results for Equation (1) for public services without significant interregional spillover effects are presented in Table 5.

Table 5: Impact of appointment law induced affiliation with the central party on public utilities and transportation

	(1) Log (heat pc)	(2) Log (sewage pc)	(3) Log (water pc)	(4) Log (road volume)	(5) Log (bus volume)
Party Change * Post	-0.064	-0.073	-0.009	0.016	-0.102
	(0.083)	(0.050)	(0.031)	(0.032)	(0.095)
Socio-economic characteristics	YES	YES	YES	YES	YES
R-squared	0.125	0.395	0.230	0.634	0.240
Observations	737	676	676	724	791

Notes: Treatment group is all states that have changed party affiliation to UR in 2005-2008. All regression include year and state fixed effects. Standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

None of the coefficients of interest illustrated in Table 5 are statistically significant, and all of them are negative. Since the presence of strong central party is hypothesized to only affect the

[31] Smaller coefficient on infant mortality may result from the fact that it responds to institutional changes as well as any potential changes in public expenditures with a significant time lag (Enikolopov and Zhuravskaya 2007).
[32] The average infant mortality for the control states during the pre-treatment period is 13.8 deaths under the age of 1 per 1,000 live births.

provision of public services with higher interregional spillover effects through a better alignment of regional and national interest, the results are expected. Estimation of Equation (1) in Table 5 confirms an earlier hypothesis that governors' centralized appointment after the new law should be associated with insignificant changes in provision of public services with low spillover effects regardless of party affiliation.

Does the timing of affiliation change matter? That is, did states whose governors changed their affiliation soon after 2004 fared better with respect to public services provision than states whose governors changed their affiliation at a later point in time? Table 6 illustrates results for Equation (1) estimations for public services with higher spillover effects. These estimations include as a treatment group a sample of states that changed party affiliation from opposition to central anytime during 2005-2012 as opposed to only during 2005-2008 as do Tables 2-5.[33]

Table 6: Impact of appointment law induced affiliation with the central party at any point in time on public services with high spillover effects

	(1) Log (enrollment rate)	(2) Log (pupil/ teacher)	(3) Infant mortality	(4) Log (population/ doctor)	(5) Log (SO2 emissions)	(6) Log (NO2 emissions)
Party Change * Post	-0.0005	-0.031**	-0.833**	-0.004	0.132	0.100
	(0.006)	(0.013)	(0.415)	(0.009)	(0.113)	(0.073)
Socio-economic characteristics	YES	YES	YES	YES	YES	YES
R-squared	0.608	0.793	0.689	0.359	0.306	0.061
Observations	616	699	779	858	742	749

Notes: Treatment group is all states that have changed their party affiliation to UR at any point between 2005 and 2012. All regression include year and state fixed effects. Standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

[33] Control group remains the same, i.e. governors that have been affiliated with the UR prior to 2004 law change.

The conclusions derived from Table 6 are twofold. On the one hand, the coefficients for enrollment rate, SO2 emissions, and population-to-doctor ratio lost their significance relative to results reported in Tables 2-4. On the other hand, the estimator for infant mortality retains its significance, and pupil-to-teacher ratio becomes significant relative to the results reported in Tables 1 and 3. The coefficients imply that a change in party affiliation from opposition to central at any time after the 2004 appointment statue is associated with a 3 percent decline in pupil-to-teacher ratio, and close to a 6 percent decline in infant mortality. As a result, I did not find consistent evidence that the timing of affiliation change has impacted the provision of all analyzed public services with higher spillover effects in treatment states. Time effect appears to be sensitive to the choice of the public service.

Estimation results for public services with low spillover effects indicate that the level of their provision is not sensitive to the timing of a change in party affiliation as difference-in-differences coefficients for all of them remain insignificant (see Table 23 in Appendix 1).

8. Conclusion

This study explores whether centralization of political power leads to similar levels of public services provision as in the case of decentralization of political power by looking at the change in political institutions that took place in the Russian Federation in 2004. Overall, the results suggest that recentralization of political institutions does alter subnational levels of public spending. However, the magnitude of the change depends on the type of public service and the variation in region's party affiliation before and after the countrywide switch to a more centralized system of government. In line with Riker's (1964) conclusions, I find that a strong central party is an effective tool that aligns regional and national political interests, thus leading to a higher degree of internalization of spillover effects from provision of certain public services. Governors who change party affiliation from opposition to central after the 2004 law saw a

positive level effect in provisions of all analyzed public services with interregional spillover effects in their respective states, except the environmental protection measures.

My results also support prior hypothesis that governors' change in party affiliation from opposition to central induced by the new appointment law had insignificant influence on level of provision of public services without inter-regional spillover effects, suggesting, that the majority party only expanded public provisions that benefits its goal of securing national votes.

Lastly, I did not find consistent evidence that the timing of a change in party affiliation from opposition to central might have mattered for the resulting provision of public services with higher interregional spillover effects.

What is the most direct policy implication on this study? Given potential underprovision of public services with higher spillover effects, predicted by existing theoretical literature, it is important to identify aspects of political institutions that can minimize the loss of interregional spillover effects other than centralization of political institutions. I believe that this study provides further evidence that strong central parties might act as important players in doing so.

Chapter II: Charter Schools: Property Value Capitalization[34]

1. Introduction

Charter schools represent a special version of public schools that are allowed to be more novel with their educational approach, while still being held accountable for student achievement. Similar to traditional public schools, charter schools are open to all students, charge no tuition, and have no particular enrollment requirements. Charter schools were designed to improve US public school system as well as offer parents more choice in their children's education. The number of charter schools in the United States has been on the rise since early 1990's. According to the National Alliance for Public Charter Schools, the number of charter schools in the US has increased from 1,500 in 1999-2000 school year to more than 6,000 in 2012-2013 school year. At the same time, the number of charter schools in Georgia increased almost six-fold: from 18 in 1999-2000 to over 100 by 2012-2013.[35] This growth in the number of charter schools and enrollees is part of a larger trend in the United States towards more school choice, which also includes inter- and intra-district choice programs, school voucher programs, magnet schools, and private schools.

Evidence on changes in home values associated with school choice generally suggests home buyers value it. Yet, previous research on the impact of charters schools on housing values remains largely inconclusive with some studies finding positive effect (Shapiro and Hasset 2013, Buerger 2014), while others finding no significant effect (Brehm et al. 2016, Horowitz et al. 2009). As a result, little is known about how the general public values the school choice option of charter schools.

[34] This essay is based on joint work with Carlianne Patrick of Georgia State University.
[35] The number of charter schools in 2012-2013 does not include schools in a charter system, as they are not comparable to the two types of charter schools included in the count by the National Alliance for Public Charter Schools - conversion and start-up.

We fill the void in the literature by analyzing charter schools with designated attendance zones. The charter schools studies thus far have dispersed geographic attendance areas. It is therefore possible that these charter schools do not create significant additional housing demand in nearby neighborhoods, leading researchers to find no significant relationship. For example, in Brehm et al. (2016) most charter schools in Los Angeles County are open to any student regardless of where they reside. The same is true for the charter system in Lucas County, Ohio, in Horowitz et al. (2009).

This paper exploits a unique feature of some charter schools in the metropolitan Atlanta area to identify property value capitalization of charter schools. The charter schools in this study designate small geographic areas within their attendance areas as priority zones. Students applying for admission and residing within a charter school's designated priority zones receive admission offers with different probabilities. Priority one zone applicants have the highest probability of admission, priority two zone applicants the next highest, and so on. This paper estimates the capitalization effects of the increased charter school admission probability conferred by location within priority one zones.

Our empirical strategy compares homes on either side of shared priority one and two zone borders for the metropolitan Atlanta charter schools with priority zones. We present estimates identified by the difference in pre- and post- opening sales prices for priority zone one and two homes within the same border area as well as a repeat sales specification. Our results indicate that prices rose by six to seven percent for priority one zone homes compared to priority two zone homes after the opening of a new charter school. We also find that the priority one zone capitalization increases as the home's traditional public school performs worse.

Our results indicate that families value the school choice in the form of charter schools since

they are willing to pay a premium to live in neighborhoods with increased charter schools admission probability. This study presents the strongest evidence on the importance of charter schools' accessibility to local communities so far.

The remainder of the paper is organized as follows. Section 2 discusses the existing literature on the effects of school choice on residential property values, and describes in detail the sample of charter schools in our analysis. Section 3 lays out the empirical approach, and describes data. Section 4 presents the main results. Section 5 concludes.

2. Background

Charter schools are publicly funded alternatives to traditional public schools in their attendance areas and are a form of school choice. Parents choose whether to enroll their students in their designated traditional public school or to send their student(s) to a charter school if they reside in the schools attendance area. Charter schools are managed by independent operators under a contract with the local authorizing agency, typically the local school board or state educational agency. They are permitted substantially more operational and programmatic flexibility than traditional public schools. For examples, charter schools may require longer school days than permitted under traditional public school regulation or deviate from state mandated curriculum. In a sense, charter schools operate similarly to private schools. The differences between charter schools and private schools are two-fold. Charter school attendance is publicly-funded and charter schools exchange their increased flexibility for increased accountability. Charter schools are subject to public oversight and are typically governed by a board of parents and teachers. Charter contracts may be revoked if governing or oversight entities determine the school underperforms with respect to student achievement.

Although there is a substantial literature on charter schools and student outcomes, there is little evidence on the relationship between charter schools and property values. There is a long

history of research documenting the capitalization of school quality and public investments into property values (Brunner et al. 2012, Chung 2015, Reback 2005, Schwartz et al. 2014, Brunner and Sonstelie 2003, Nechyba 2000, Fack and Grenet 2010, Cannon et al. 2015, Merrifield et al. 2011, Bonilla et al. 2015, Walden 1990, Brehm et al. 2016, Shapiro and Hasset 2013, Buerger 2014, Horowitz et al. 2009, Billings et al. 2014). If households value the charter school option, charter schools improve traditional public school performance, or charter schools represent a significant investment in valuable public facilities, then it would be reasonable to expect charter schools to have an effect on property values as well. The notion that charters influence property values through their option value or through their effect on traditional public schools is supported by a number of studies that find various forms of school choice significantly affect property values in the areas in which they are implemented.

2.1 School Choice and Property Values

School choice comes in a variety of forms. Inter- and intra-district programs allow parents to choose an alternative traditional public school from the one servicing the attendance area in which the household resides. Voucher programs, also known as opportunity scholarships, are state-funded programs that provide students with an opportunity to attend private schools by paying some portion of their tuition (National Conference of State Legislatures). Magnet schools represent type of public schools with a concentration in certain curricula, such as STEM, Fine and Performing Arts, International Studies, World Languages, etc. (Magnet Schools of America). Finally, private schools are non-state funded schools that charge tuition. They are exempt from many government regulations that apply to other school choice options.

Studies evaluating the impact of inter- and intra-district choice establish that property values reflect households' positive valuation of access to better performing schools, but also that choice decreases the premium associated with living in the best performing school attendance areas.

Brunner et al. (2012) evaluate twelve states that have passed legislation mandating inter-district school choice by 1998. They show that districts in close proximity to desirable out-of-district public school options have experienced a significant increase in residential property values. Chung (2015) analyzes both inter- and intra-district schools choice programs in Seoul area. He discovered that they led to a decrease in housing values in better-performing school districts by 10-27% relative to housing values in worse-performing school districts. Echoing Chung's predictions, Reback (2005) documents that inter-district schools choice in Minnesota led to property values increasing in school districts where students are able to transfer to a different school, but decreasing in those school districts that are accepting a lot of transferring students. Finally, Schwartz et al. (2014) show that general availability of the school choice in New York City prior to 2003 increased property values by 2.2%.

The literature on school voucher programs documents a similar relationship, with property values reflecting households' positive valuation of choice and a decline in the premium for superior quality schools. This research highlights a positive relationship between voucher programs and residential property values, particularly in districts looking to take the biggest advantage of the voucher-enabled access to better schools. Brunner and Sonstelie (2003) evaluate 2000 voucher initiative in California. They find that it led to a decline in property values in areas with superior public schools, and an increase in property values in areas with inferior public schools. Nechyba (2000) shows that universal vouchers for private schools located in low-income districts lead to an increase in housing prices in those districts due to the migration of high- and middle-income families looking to take advantage of relatively lower housing prices. Nechyba argues that vouchers for private schools located in lower-income districts in essence diminished existing positive relationship between public school quality and property values.

Fack and Grenet (2010) make a similar statement having evaluated the effect of private schools on housing values in surrounding neighborhoods in Paris, France. Cannon et al. (2015) evaluate a schools choice system in Vermont. They find that areas offering tuition vouchers experienced an increase in residential property values by anywhere from 3 to 16% depending on the school quality. Finally, Merrifield et al. (2011) analyze the effect of temporary tuition voucher program in Edgewood Independent School District, San Antonio, Texas. The tuition program increased residential home transaction values by almost 10% during its early years, and by 1% during its final years.

The magnet school alternative to traditional public schools also appears to have a similar effect on property values. Increased access to the magnet school alternative is positively capitalized into property values. Bonilla et al. (2015) show that higher probability of admission to Chicago magnet schools for students living within the 1.5-mile radius of a magnet school made possible in 1997 increased housing values in those areas by 5.4%. In addition, premium on properties located in close proximity to multiple magnet schools is even higher. Echoing Nechyba (2000) and Fack and Grenet (2010), Walden (1990) demonstrates that presence of a magnet school in Wake County, North Carolina, reduced the capitalized value of traditional public schools quality in the school district, especially for elementary schools. Thus, by decoupling access from residential location, magnet schools change the relationship between public school quality and property values. Finally, Billings et al. (2014) evaluate the consequences No Child Left Behind policy for Charlotte, North Carolina school district, where students attending consistently failing schools get an opportunity to enroll in high-quality, over-subscribed schools, including magnet schools. Their results indicate that higher-income families

are willing to pay more for houses in highest quality neighborhoods within the attendance boundaries of failing schools.

Charter schools similarly introduce an alternative choice to traditional public schools. In the studies that are most closely related to ours, researchers estimate property value capitalization of charter schools without catchment areas or with dispersed geographic attendance areas (Brehm et al. 2016, Shapiro and Hasset 2013, Buerger 2014, and Horowitz et al. 2009). Brehm et al. (2016) and Horowitz et al. (2009) find no significant relationship between charter schools and housing values. Specifically, Brehm et al. (2016) and Horowitz et al. (2009) document no positive effect of charter schools on residential property values in Los Angeles County, California, and Lucas County, Ohio, respectively. These studies suggest the option value created by charter school choice differs from the school choice initiatives discussed above. On the contrary, Shapiro and Hasset (2013) show that the expansion of charter schools in New York City between 2002 and 2013 increased property values by 3.84% at the zip code level. Buerger (2014) also finds that charter penetration in upstate New York had a positive impact on housing prices. The New York studies indicate choice is positively capitalized when charter schools are the alternative, similar to the findings for other forms of school choice.

The mixed evidence on the relationship between charter schools and property values could be attributable to the nature of charter schools and their perceived value as an alternative. It could also arise from the offsetting effects of increased values associated with choice and changes in the relationship between property values and school quality. Our analysis focuses on charter schools with a unique feature tying residential location with access to the charter school. As discussed in detail below, we examine charter schools with well-defined attendance areas and small, geographic areas designated for priority admissions within those attendance areas. Similar

to the aforementioned Bonilla et al (2015) study of Chicago magnet schools, these priority zones are associated with differential probability of gaining admission to the charter school. We focus on homes located within close proximity to the border between priority one and two zones, thereby minimizing the potentially countervailing effects of underlying changes in the relationship between traditional school performance and the introduction of school choice through charter school openings.

2.2 Metro Atlanta Charter School Priority Zones

There are three types of charter schools in Georgia: conversion, start-ups, and schools in a charter system. The latter are relatively new and unique to Georgia. As such they are not included in our analysis and our results should not be extrapolated to schools in charter systems. Conversion and start-up charter schools operate under contracts with State that specify the school enrollment cap, attendance zones – the geographic area from which students may be drawn – and enrollments priorities. In Georgia, charters schools may also define priority zones within the charter's designated attendance zone. The designation of priority zones confers different probabilities of gaining admittance to a charter school to different geographic areas within the school's attendance area. Conversion charters are existing traditional public schools that convert to a charter school after a vote by the faculty and parents. Priority one status for conversion charters is therefore generally the attendance boundary designated by the local school board, with the local school district comprising the attendance zone. Start-up charters, as the name implies, did not exist prior to being authorized and are created by a petition brought forth by individuals, private organizations, or a state or local public entity. Start-up charters have substantial flexibility in setting attendance areas and priority zones. Only a small subset of Georgia schools designate priority zones and currently all that do so are located in the metropolitan Atlanta area and either Fulton or DeKalb Counties. They are also located within

one of three local public school districts - Atlanta Public School District, Fulton County School District, or DeKalb County School District.

Table 24 in Appendix 2 details the priority zones, year opened, and grades served by each of the thirteen metropolitan Atlanta charter schools with priority zones. Kingsley Charter Elementary and North Springs Charter High School are both conversion charter schools and the remaining schools are start-up schools. The Atlanta Neighborhood Charter Schools, Charles Drew Charter School, and The Museum at Avondale Estates were created through local parent-driven initiatives. KIPP, a nationwide charter school organization, worked with local parents to open its six metro Atlanta charters. The start-up charter school priority zones do not necessarily align with local school attendance boundaries. For example, the priority one zone for KIPP STRIVE Academy is the 30310 zip code -- a relatively arbitrary boundary in terms of variation in neighborhood and housing attributes.

Figure 2 maps the priority one zone for each of the charter schools listed in Table 24. It should be clear that charter schools do not locate at random. The schools vary in their origins as well as their target populations. Location choice and priority attendance areas reflect this variation. The KIPP schools, for example, are clustered near one another, both because of economies of scale in management from the national organization and because target populations are clustered in that area. The non-random nature of charter school location creates a challenge for identifying their capitalization into property values. Figure 3 depicts the annual mean sales price for priority one zone single-family residential, fair market value sales as well as the mean for Fulton and DeKalb Counties. It reveals that a naive comparison of priority one zone property values would likely suggest negative capitalization attributable to charter school priority zones disproportionately located in lower value areas.

Figure 2: Priority zone map - Atlanta metro area

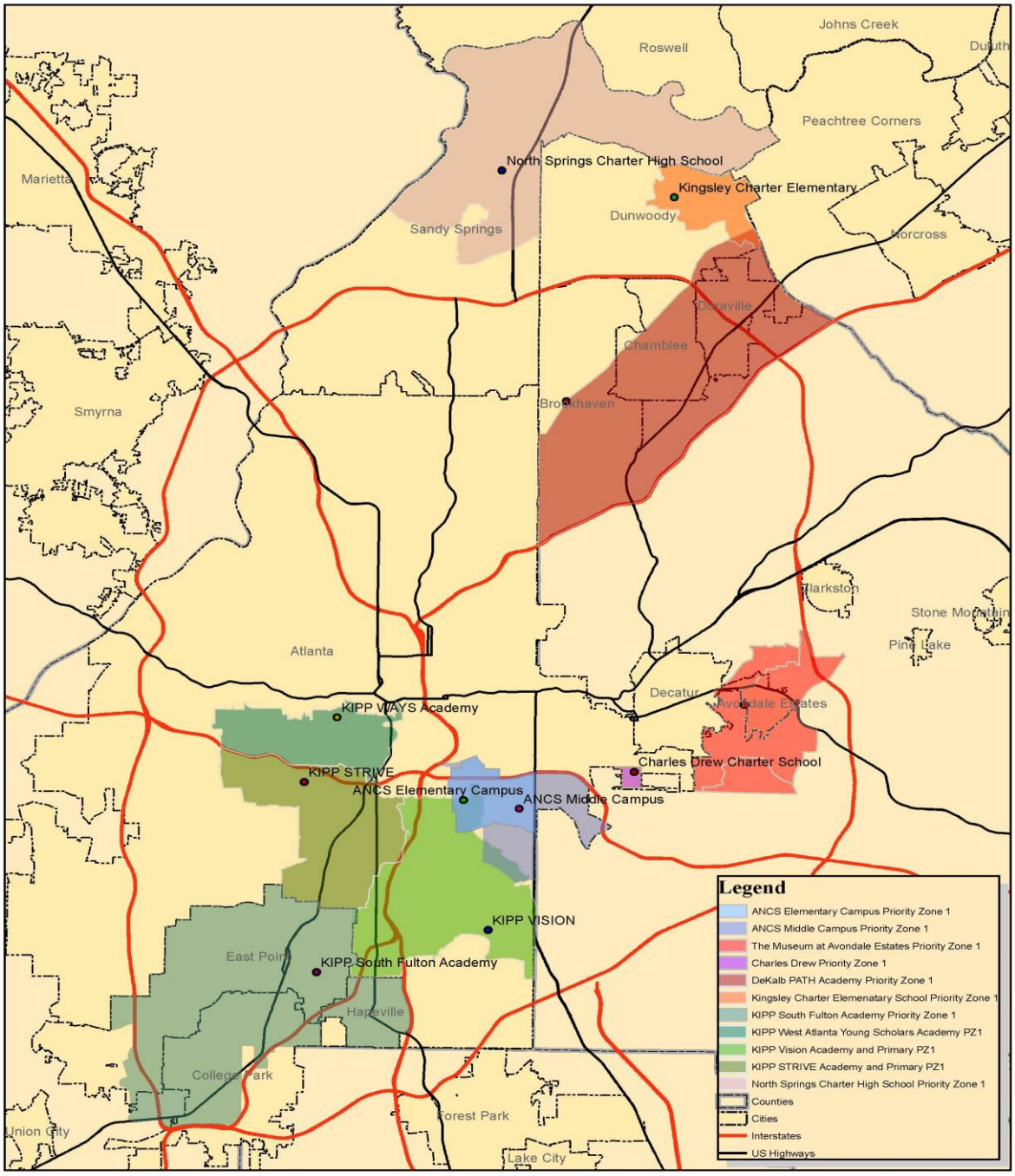
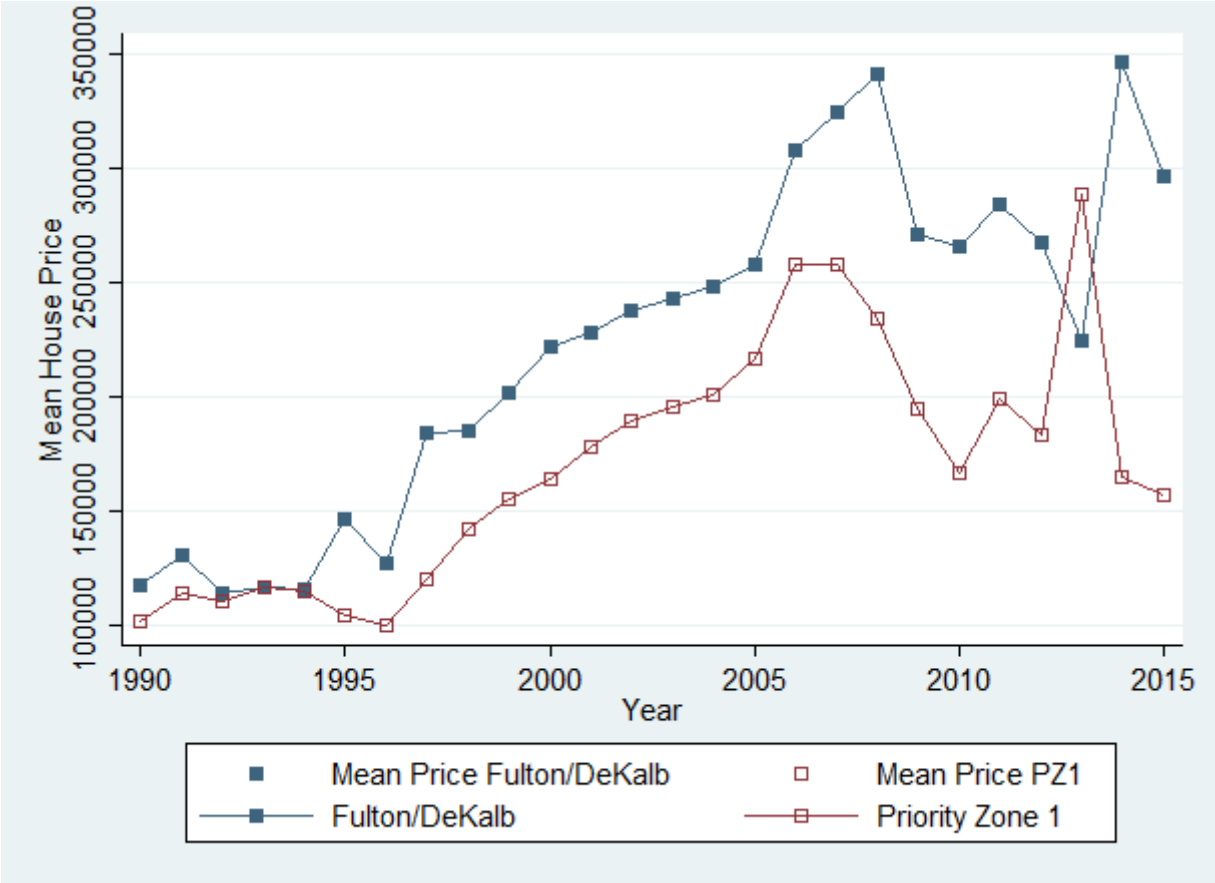


Figure 3: Annual mean sales price comparison - priority one zone, Fulton County and DeKalb County



It is for this reason that our analysis focuses on properties located close to the border between priority one and two zones for each charter school. The priority one and two zones for DeKalb Path Academy are not adjacent, but otherwise priority two zones share a border with the priority one zones. We use that shared border to identify properties treated with priority one status and counterfactual properties as nearby homes with priority two status. DeKalb PATH is therefore removed from our analysis. In some cases, priority one zones for different charter schools overlap. We discuss how we address those areas in more detail in the data section below.

As discussed above, priority zones confer different probabilities of gaining admission to the charter school. Charter schools set enrollment caps that dictate the maximum number of students per grade level. Returning students, siblings of returning students, children of full-time employees, and children of governing board members typically receive first priority in allocating available slots in each grade. Although there is some variation across schools, the remaining available slots are filled by priority one zone applicants, priority two zone applicants, and so on. If there are more applicants than available slots, then a random lottery determines which applicants receive offers of admission. The following summarizes this process (Patrick 2015):

i) If applications for students residing within the priority one zone exceed available slots, then a random lottery selects applicants from priority one zone applicants only. All applicants living within the priority two zone are waitlisted, with waitlist order determined by random lottery.

ii) If applications from students residing within the priority one zone do not exceed available slots but the combined number of priority one and two applicants does, then either

a. Priority one applicants receive offers of admission and a lottery determines admission offers and waitlist order for priority two applicants; or,

b. Priority one and two applicants are pooled in the lottery to determine admission offers and waitlist order.

iii) If priority one and two applicants do not exceed available slots but the total number of applicants from the designated attendance area exceeds available slots, then priority one and two applicants receive offers of admission. A random lottery determines admission offers and waitlist priorities for remaining applicants.

The admission probability conferred by living in the priority zones varies by school, grade, and year. Kingsley Charter Elementary School and North Springs Charter High School are outliers in this process because they are conversion charters. Priority one zone students are guaranteed admission. Zone two applicants may receive admission offers by lottery until the enrollment cap.[36] The admission probability associated with living in the priority one zone is therefore one and less than one for priority two zone residents.

Similarly, Figure 2 reveals that the start-up charter school Charles Drew priority one zone covers a very small geographic area. There are, consequently, a small number of households in the Charles Drew Charter School priority one zone relative to the average number of available slots below the enrollment cap. The Charles Drew admission process implies a probability of one for zone one applicants. The probability for Drew priority two zone two residents varies from year to year, with Drew reporting no lottery for the 2009-2010 school year (implying a probability of one) and lotteries in subsequent years (implying a probability of less than one).[37] Thus, at least in expectation, there is a discrete change in probability for households in Drew priority zones one and two.

Atlanta Neighborhood Charter Schools (ANCS), on the other hand, report regularly receiving more applicants from priority one zone students than available slots in some grade-years. As noted above, a lottery determines which zone one applicants receive admission offers when there are more applicants from zone one than available slots. In this case, the zone one admissions probability is less than one (for oversubscribed grade-years) and zero for priority two zone

[36] According to information provided by the school, Kingsley has not had available lottery slots for zone two applicants in recent years. The conversion of North Springs Charter High School to a school in a charter system (from a conversion charter school) that began last year eliminates zone two effective probabilities of admission outside the magnet programs.

[37] For example, the 2014-2015 Pre-K lottery admitted 108 applicants and waitlisted 168, implying an acceptance probability of 0.39.

applicants. In other grade-years, ANCS did not received more applicants from zone one than available slots. In this case, all zone one and two applicants are pooled in the lottery, with equal probability of being chosen. Parents cannot know a priori whether there will be more applicants for their student's grade level from zone one than available slots. The expected probability of admission for ANCS zone one applicants therefore is higher than the expected probability for zone two applicants, but still less than one.

3. Empirical Implementation

3.1 Empirical Strategy

As noted above, charter school priority zones are not randomly located across space and potential for biased estimates arise to the extent that unobservable attributes of the homes or neighborhoods determining treatment (charter school priority zone designation) also influences sale price. Our empirical strategy minimizes this risk by comparing the change in sale prices for homes located within close proximity to the border between priority zones one and two. Homes located near the shared border should be similar with respect to unobservable attributes such as access to employment opportunities, transportation networks, parks, shopping and dining, the neighbors with whom residents interact, etc. It is reasonable to expect that homes become more similar as they become closer in space. Defining "close" as a very small distance from the border therefore has the advantage of comparing similar homes, but at the cost of decreased sample sizes and loss of information from other home sales in the area. Our primary results define "close" as being within 0.3 miles of the shared border, which we believe balances the trade-off between minimizing unobserved heterogeneity and sample size. We also present estimates for "close" defined as 0.1 miles and 0.5 miles.

We are interested in the "causal" or "capitalization" effect of the increased probability of gaining admission to the charter school conferred by being located in a priority one zone. We

therefore adopt a standard strategy in the literature and estimate the following hedonic difference-in-differences for the change in the log of sales price *y* for single-family house *i* border area *j* at time *t*:

$$y_{ijt} = \beta' X_{it} + \delta 1(PZ1)_{ij} + \kappa 1(\tau > 0)_{jt} + \theta\left[1(PZ1)_{ij} \times 1(\tau > 0)_{jt}\right] + \alpha_j + \mu_t + \varepsilon_{ijt} \quad (2)$$

where X_{it} is a vector of observable characteristics of house *i*, α_j is a vector of border area indicator variables, μ_t is a vector of quarter-year fixed effects, $1(PZ1)_{ij}$ is an indicator variable equal to one for homes located on the priority zone one side of shared border *j*, $1(\tau > 0)_{jt}$ is an indicator equal to one indicator for *t* being after the charter school opened, and τ is year normalized such that $\tau = 0$ in the charter school opening year for each school.

Our parameter of interest is θ, the average change in sale prices for priority one zone homes after the charter school opening, which we refer to as the capitalization effect. This effect is identified by comparing the difference in pre- and post- opening sales prices for priority zone one and two homes within the same border area while controlling for observable characteristics and common quarter-year shocks to sale prices.

Threats to identification arise to the extent that the specification does not account for unobservables correlated with sales price and treatment (priority one zone status and charter school openings). Our vector of observable characteristics includes measures of house size, lot size, bathrooms, bedrooms, age, condition of the home, fireplaces, garages, and recent renovation. It also includes measures for the number of nearby distressed transactions within the last six months. The border area fixed effects control for other unobservable characteristics such as access to employment opportunities, transportation networks, parks, shopping and dining, the neighbors with whom residents interact, etc. The priority one zone indicator absorbs unobservables common across areas designated as priority one zones (as opposed to priority

two). Unobservables may still pose a threat, though, through at least three channels.

The first channel is through unobservable differences in house characteristics. In particular, the concern is that the sample of sales in the post-treatment areas (or, equivalently, in the counterfactual areas or pre-treatment period) is composed of homes with unusually high or low values of these unobservable characteristics. This "sample selection" problem, as it is commonly referred to in the literature, introduces the potential for omitted variable bias that is typically addressed by using repeat sales (McMillen 2012). We follow the literature and our primary results use a repeat sales version of Equation (2). We present the repeat sales estimates alongside estimates from Equation (2) to allay any concerns that homes selling multiple times during the sample period may not be representative of typical homes in the area.

Recent critiques of the hedonic difference-in-difference estimation identify a second channel through which unobservables may threaten identification of θ as the capitalization effect of treatment– changing hedonic price functions over time or as a result of treatment (Klaiber and Smith 2013; Kuminoff and Pope 2014). Hedonic equilibria occur across space within a single time period. Hedonic difference-in-differences therefore compare prices from two (or more) equilibria in which treatment or changes in the underlying economic environment may have altered the hedonic price function (Banzhaf 2015). Critiques of hedonic difference-in-differences suggest the resulting estimates do not identify the capitalization effect because they mix information from two (or more) equilibria and information on treatment. More recently, Banzhaf (2015) provides fairly weak conditions under which difference-in-differences hedonic estimates identify the direct (unmediated) effect of treatment. It is this effect that we will refer to as the capitalization effect of priority one zone increases in the probability of charter school admission.

We estimate capitalization using a repeated cross-section of single-family, residential homes or repeated sales of single-family, residential homes. Our data do not provide detailed characteristics on the home buyers. Bayer et al (2007) poignantly demonstrate that households sort across borders in response to changes in school quality and that estimates of willingness-to-pay for school quality include the value of school quality as well as the value of changes in resident characteristics. If neighborhood composition changes in response to priority one zone status after charter school openings, then our capitalization estimates also include changes in price associated with neighbors who also value increased admission probability.

3.2 Data

The housing price data cover all transactions from 1990 to 2010 for Fulton and DeKalb counties in Georgia, all parts of Atlanta metro area. As discussed above, we chose Georgia as our study sample because its charter schools have designated priority attendance zones, a characteristic that sets out sample of charter schools apart from samples used in previous studies. We use elementary, middle and high schools.

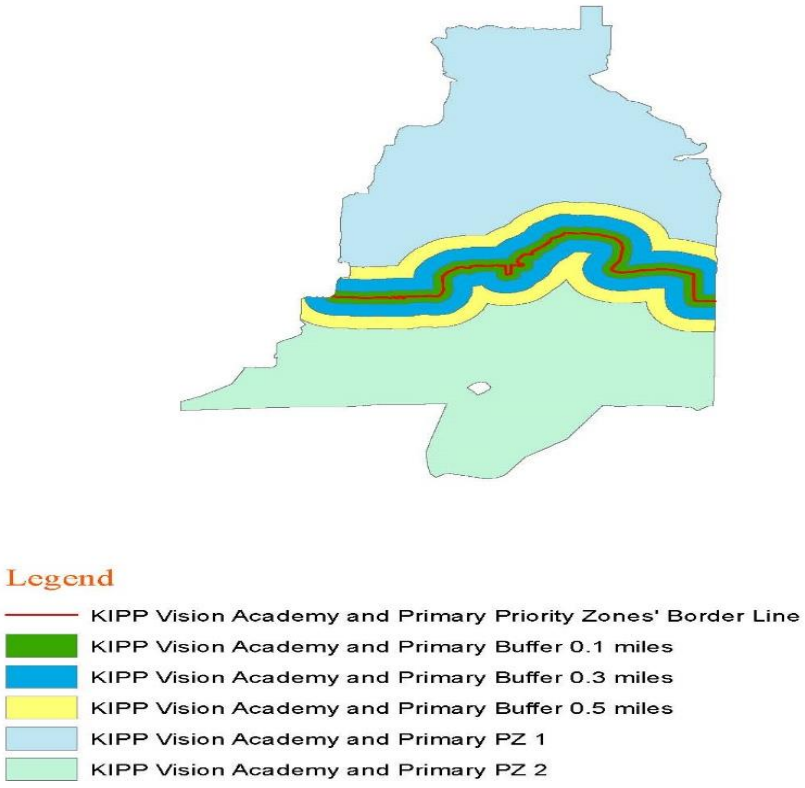
Figure 2 represents part of the Atlanta metro area used in our analysis. Using Fulton and DeKalb counties' parcel data, which include geographic coordinates, we matched housing transactions to charter schools' priority zones.

As mentioned in section 2.2, the following charter schools are included in the analysis: Atlanta Neighborhood Charter School (middle and elementary campuses), Charles R. Drew Charter School, KIPP South Fulton Academy, KIPP STRIVE Academy and Primary, KIPP Vision Academy and Primary, and KIPP WAYS Academy, the Museum School of Avondale Estates, Kingsley Charter Elementary School, and North Springs Charter High School. The charter schools for the analysis were selected based on whether they had at least two priority attendance zones. The priority zones were determined from the approved charters for schools

provided by the Georgia Department of Education. The maps were obtained as either JPEG or PDF files directly from schools' websites, or provided as GIS shapefiles by the Atlanta Regional Commission.

We generated buffer areas on each side of the border between priority zones 1 and 2 of each charter school in the sample. We created 0.1, 0.3, and 0.5 mile buffers for each set of priority zones. Figure 4 represent an example of charter schools priority zones and corresponding buffers in our sample.

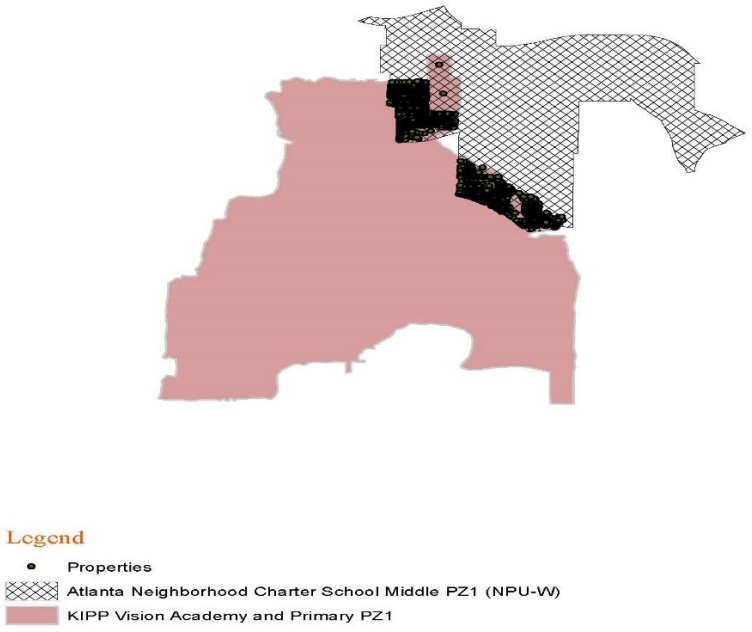
Figure 4: Example of charter school priority zones and corresponding buffers



We exclude all single-family residences located in more than one charter school priority one zone as they are not immediately comparable to the rest of the houses in the sample. For example, in Figure 5 part of the Atlanta Neighborhood Charter School middle campus priority

zone 1 overlaps with the KIPP Vision Academy priority zone 1. As a result, families residing in properties located at the intersection of two priority zones, indicated by green dots, are eligible for acceptance into either Atlanta Neighborhood Charter School (ANCS) middle school, or KIPP Vision Academy, which is also a middle school. Not only are homes in these areas treated with more than one charter school priority one zone, the counterfactuals in the border areas are also treated with priority one zone admission probabilities from another charter school. We exclude 3,327 single-family, residential fair market sales that occur from 1990-2015 in the overlapping priority one zone 0.3 mile border areas.

Figure 5: Atlanta Neighborhood Charter School priority zone 1 and KIPP Vision Academy priority zone 1



The housing price data comes from DataQuick. The sample within 0.3 miles of the priority one and two zone borders consists of 28,654 single-family residences within 10 charter schools' attendance boundaries. The sample of repeated sales consists of 22,860 single-family residences within the attendance boundaries of the same 10 charter schools. Table 7 summarizes the data.

Table 7: Summary Statistics

	0.5 miles				0.3 miles				0.1 miles			
	Total		Repeated Sales Only		Total		Repeated Sales Only		Total		Repeated Sales Only	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Bedrooms	3.00	0.82	2.96	0.81	2.96	0.81	2.92	0.79	2.89	0.81	2.86	0.79
Bathrooms	1.99	1.02	1.93	0.98	1.94	0.99	1.88	0.94	1.84	0.92	1.78	0.87
House price	178,579	168,302	175,098	157,283	177,360	161,292	174,390	147,254	163,142	150,133	158,338	135,816
House size (sqft)	1,711	874	1,658	822	1,669	841	1,619	781	1,565	742	1,516	662
Lot Size (sqft)	13,105	11,313	12,225	9,993	12,252	10,860	11,385	9,437	10,943	10,160	10,147	8,401
House below average	0.03	0.17	0.03	0.16	0.03	0.17	0.03	0.16	0.02	0.15	0.02	0.14
House above average	0.13	0.34	0.15	0.36	0.13	0.34	0.15	0.36	0.14	0.34	0.15	0.36
Fireplace	0.51	0.50	0.50	0.50	0.48	0.50	0.47	0.50	0.43	0.50	0.42	0.49
Garage	0.12	0.33	0.12	0.32	0.11	0.31	0.11	0.31	0.10	0.31	0.09	0.29
Age of building	44.40	26.60	46.69	26.70	46.77	27.06	49.10	27.16	49.01	27.39	51.42	27.14
Distress m1	41.76	53.60	44.92	55.01	44.03	53.82	46.94	54.79	47.30	54.14	49.86	54.90
Distress m5	12.97	18.65	13.94	19.24	13.76	19.22	14.65	19.69	15.48	20.51	16.27	20.87
Distress m25	3.95	6.23	4.25	6.43	4.16	6.46	4.42	6.62	4.66	6.83	4.88	6.92
Renovations	0.05	0.22	0.06	0.23	0.06	0.23	0.06	0.24	0.08	0.26	0.09	0.28
N	43,730		33,785		28,654		22,860		9,138		7,453	

The mean housing price in the full sample is $177,360, with a standard deviation of $161,292. The mean housing price in the repeated sales sample is $174,390, with a standard deviation of $147,254. We control for other housing characteristics such as number of bedrooms and bathrooms, house and lot square footage, age of the building, presence of fireplace and garage, presence of recent renovations, number of distress transactions in close proximity, and dummy variables for whether the house is in below, at, or above the average condition as determined by the county assessor. Table 7 also shows descriptive statistics for two subsamples of single-family residences located inside the 0.5-mile buffer, and 0.1-mile buffer of the 10 charter schools attendance boundaries.

It is plausible that homebuyers will value eligibility for charter school enrollment even more if it resides in the attendance zone of a relatively underperforming traditional public school. To test this notion, we add an interaction between $\left[1(PZ1)_{ij} \times 1(\tau > 0)_{jt}\right]$ and a measure for the quality of homes' traditional public schools to our empirical specification. We spatially match each housing transaction to its designated public elementary, middle, and high schools. School attendance boundaries change over time and we obtained data on the boundary changes directly from the local school districts. Our proxies for school quality are Criterion-Referenced Competency Tests (CRCT) in English, Reading, and Math for elementary and middle schools, and End of Course Tests (EOCT) in Algebra and English Composition for high schools from 2004 to 2013.[38] CRCT was a state-wide assessment performed every year on students in grades 1 through 8. EOCT was largely used for high school accountability assessment. We acquired CRCT and EOCT results from the Georgia Governor's Office of Student Achievement for every

[38] Both CRCT and EOCT programs were discontinued at the end of the 2013-2014 school year, and replaced with Georgia Milestones Assessment System (Georgia Department of Education). We were unable to get test results prior to 2004.

public school in Fulton and DeKalb counties. Specifically, the data show the mean percent of students who did not meet state standards, mean percent of students who met the state standards, and mean percent of students who exceeded the state standards in a particular subject. CRCT means were calculated across grades in elementary and middle schools, grades 3 through 5 and 6 through 8, respectively. EOCT means were calculated across grades in high schools (grades 9 through 12). In addition, the data delineate CRCT and EOCT results by race and income. All performance means were calculated at the school level. The school performance indicator is assigned to housing transactions for the traditional public school servicing the same grade level as the charter school for the transaction. For example, the performance measure for homes within the KIPP Vision Academy boundary area is the performance for the middle school that students in that home would otherwise attend. In practice, we present results for performance measured as the percentage of all, black, and economically disadvantaged students failing to meet math standards. Results using measures for reading and literature were quantitatively and qualitatively similar.

4. Results

Table 8 presents the estimated change in single-family, residential home sale price in priority one zones after the corresponding charter school opening as compared to the change in price for homes in priority two zones within the same 0.3 mile border area. Column (1) reports the hedonic difference-in-differences estimator θ from Equation (2). Column (2) adds additional city and county fixed effects to Equation (2). Column (3) contains the results from the repeat sales specification. The Table 8 estimates indicate sales prices increased for homes with the greater probability of charter school admission conferred by priority one zone status. The repeat sales estimated increase of 8.86% (Column 3) is slightly larger than the 7.26% increase from estimating Equation (2). This suggests that homes with slightly lower valued unobservables may

comprise a larger share of post-period sales; however, the difference appears slight and the change for our sample of repeat sales resemble the typical homes in the area. As noted above, the repeat sales estimator removes "sample selection" concerns and is therefore our preferred estimate.

Table 8: Estimated priority zone one capitalization within 0.3 mile border areas

	(1)	(2)	(3)
PZ1 Capitalization	0.0726***	0.0535***	0.0886***
	(0.0180)	(0.0182)	(0.0221)
Housing Characteristics	Y	Y	N
Distress Measures	Y	Y	Y
Border Area FE	Y	Y	N
Quarter-Year FE	Y	Y	Y
City/County FE	N	Y	N
Observations	27,319	27,319	21,767
R-squared	0.593	0.598	0.783

Notes: The table presents results from three separate regressions. Column (1) is the hedonic difference-in-differences for priority one zone sales prices after charter school opening compared to priority two zone home sales in the same 0.3 mile border area. Column (2) adds additional city and county fixed effects to the Column (1) specification. Column (3) contains the results from the repeat sales difference-in-differences specification. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Given the mean pretreatment sales price for priority one zone homes within 0.3 miles of the border, Table 8 suggests priority one zone prices increased by $8,845-$13,470 in the periods following charter school openings. If homebuyers spend more for priority one zone homes, it implies that households value the choice, flexibility, and accountability that characterize charter schools. In this context, it is important to note that the change in priority one zone sale prices is being compared to the change in priority two zone prices. Households in priority two zones lie within the charter school attendance areas and therefore also have access to the charter school. The difference in access is the difference in the probability of charter school admission associated. As we discussed in the Section 3.1, it is possible that households differentially sort

along the priority zone border after the charter school opening. It is therefore possible that Table 2 estimates the increase for increased charter school admission probabilities and the type of neighbors that value them.

Table 9 reports the variation in estimated capitalization effects across border areas of 0.1 (Panel A), 0.3 (Panel B), and 0.5 miles (Panel C), respectively. Columns (1) and (2) in each Panel contain the within border area difference-in-differences hedonic and repeat sales estimates, respectively. Table 9 indicates property values significantly increased between 5 and 9.5 percent in priority one zones after charter school openings, which is fairly consistent with the Table 8 results. It should be noted that statistically significant capitalization effects disappear in the 0.1 mile border area for repeat sales (Panel A, Column 2); however, this could be attributable to small sample size.

Table 9: Estimated priority zone one capitalization within 0.1, 0.3, 0.5 mile border areas

	Panel A: 0.1 miles		Panel B: 0.3 miles		Panel C: 0.5 miles	
	(1)	(2)	(1)	(2)	(1)	(2)
PZ1 Capitalization	0.0699**	0.0081	0.0726***	0.0886***	0.0472***	0.0953***
	(0.0343)	(0.0429)	(0.0180)	(0.0221)	(0.0140)	(0.0174)
Housing Characteristics	Y	N	Y	N	Y	N
Distress Measures	Y	Y	Y	Y	Y	Y
Border Area FE	Y	N	Y	N	Y	N
Quarter-Year FE	Y	Y	Y	Y	Y	Y
Observations	8,054	6,454	27,319	21,767	44,598	35,031
R-squared	0.578	0.768	0.593	0.783	0.592	0.783

Notes: The table presents results from six separate regressions. Panels A, B, and C correspond to different 0.1, 0.3, and 0.5 mile border areas, respectively. Panel A, B, and C Columns (1) report the hedonic difference-in-differences for priority one zone sales prices after charter school opening compared to priority two zone home sales in the same 0.3 mile border area. Column (2) contains the results from the repeat sales difference-in-differences specification. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Tables 8 and 9 employ data from all available years, 1990-2010. As discussed in Section 3.2, the sales have been limited to single-family, residential fair market value transactions whenever

possible, but early sales in some jurisdictions without sale type information have been retained. Table 10 presents results for repeat sales after removing any early transactions for which sale type could not be verified. Column (1) contains estimates using all available years analogous to those in Tables 8 and 9. Column (2) limits the pre- and post-period window to four years. Column (3) employs repeat sales within six years before or after the charter school opening. Panels A and B present results for the 0.3 and 0.5 mile border areas, respectively. The estimated capitalization effect ranges from 6-10% in Table 10.

Table 10: Restricted sample, repeat sales estimated capitalization by time window

	Panel A: 0.3 mile			Panel B: 0.5 mile		
	(1)	(2)	(3)	(1)	(2)	(3)
	All Years	4 Years	6 Years	All Years	4 Years	6 Years
PZ1 Capitalization	0.0739**	0.0981*	0.0672	0.0617***	0.0788*	0.0630**
	(0.0313)	(0.0547)	(0.0425)	(0.0235)	(0.0406)	(0.0321)
Observations	16,154	7,995	10,838	26,214	12,837	17,482
R-squared	0.801	0.862	0.821	0.802	0.867	0.826

Notes: The table presents results from six separate repeat sales regressions with quarter-year fixed effects and distress measures for the restricted sample of early sales. Panels A and B correspond to different the 0.3 and 0.5 mile border areas, respectively. Panel A and B Columns (1) uses all periods of available restricted data. Column (2) contains the results from repeat sales occurring within four years before or after the charter school opening. Column (3) expands the time window to six years before or after the charter school opening. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

The effect of removing the unverified early sales from the sample can be gleaned by comparing Table 10 with the analogous estimates in Table 9. It has little effect on the 0.3 mile estimates, decreasing the estimates from 8.86 percent (Table 9, Panel B, Column 2) to 7.39 percent (Table 10 Panel A Column 1). The decrease a bit more pronounced for the 0.5 mile border area estimates, decreasing the capitalization effect from 9.5 percent (Table 9, Panel C, Column 2) to 6.17 percent (Table 10, Panel B, Column 1).

Looking across the columns within each panel in Table 10 reveals some variation in capitalization effects across time. Price increases are larger when the sample is limited to repeat sales within four years before and after the charter school opening (Columns 2) than when the sample contains repeat sales within longer time windows. The six year window estimates (Column 3), however, closely resemble the estimates using all periods, suggesting the initial boost in property values levels out to a sustained priority one zone capitalization effect of approximately 6-7 percent compared to priority two zone homes.

Recall that our estimates compare home sales on either side of the priority one and two zone border. The homes have access to the charter schools with different probabilities, but both treated and control groups are within the charter school attendance boundaries. While estimated positive capitalization effect suggest household value the choice associated with charter schools, our results are identified from the discrete change in admission probability at the border. Taken together, the estimates in Tables 8-10 suggest that households value the increased probability of admission to charter schools associated with being located in priority one zones and, perhaps, the change in neighborhood resident composition associated with priority one zone status. Since our sample of residential properties is located near the shared border, and in close geographic proximity, the houses on each side of that border should be relatively similar with respect to unobservable neighborhood characteristics. As a result, we can argue that housing premiums associated with zone one are predominantly driven by the increased probability of charter school enrollment.

Table 11 presents results for the change in capitalization associated with traditional school performance for repeat sales within 0.3 mile border areas. The specification includes the priority one zone capitalization estimated above as well an interaction term between the priority one zone

post-opening indicator and a measure of school quality. We exclude charter schools with opening dates prior to 2003 because performance data was unavailable prior to the 2003-2004 school year. School quality is measured by the percentage of all, black, and economically disadvantaged students failing to meet math standards, in Columns (1) – (3), respectively. As noted in the data section, results using reading and literature were similar.

Table 11: Change in capitalization associated with traditional school performance for repeat sales within 0.3 mile border areas

	(1) All Students	(2) Black Students	(3) Disadvantaged Students
PZ1 Capitalization	0.125**	0.136***	0.120**
	(0.0546)	(0.0527)	(0.0541)
PZ1-School Quality Interaction	0.0038***	0.0034***	0.0039***
	(0.0013)	(0.0012)	(0.0012)
Observations	16,476	16,476	16,476
R-squared	0.778	0.778	0.778

Notes: The table presents results from three separate repeat sales regressions with quarter-year fixed effects and distress measures for the sample of single-family residential sales within 0.3 miles of shared priority one and two zone borders. School quality is measured by the percentage of all, black, and economically disadvantaged students failing to meet math standards, in Columns (1) – (3), respectively. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 11 indicates that capitalization is stronger for homes with underperforming traditional public schools. One percent increase in the number of students failing to meet math standards increases priority one zone sales prices by 0.4 percent after the charter school opens (Table 11, Column 1). Consistent with previous studies of school choice capitalization, charter schools appear to increase demand and/or change the type of residents buying homes in areas with struggling traditional public schools. Measuring performance by the percentage of black or economically disadvantaged students failing to meet math standards, Columns (2) and (3), respectively, produces similar estimated effects.

5. Conclusion

Do families value charter schools? Our results suggest that they do. We use a unique characteristic of ten charter schools in the Atlanta metro area to study whether households are willing to pay a premium for a house located in priority on zone as opposed to priority two zone. Even though students residing in either priority zone are eligible to attend corresponding charter school, the probability of enrollment is substantially larger for the residents of priority zone one. Our identification strategy is based on the differences in sales prices for homes in priority one and two zones before and after the opening of a charter school. We find that parents are willing to pay about 6%-8% - or $8,845-$13,470 – more for houses located in priority zone one within 0.3 miles from the border, following the charter school opening. These results are robust to different border areas, and sample sizes. We also find that the effect is stronger for houses with underperforming traditional public schools.

Our findings have several important implications. Even though our sample of Atlanta area families might not necessarily reflect the preferences of others, the study demonstrates that charter schools represent an important component of school choice. They not only have value to parents, but also to property owners and policy makers by making surrounding neighborhoods more attractive to local population.

We have concentrated on the small part of the relationship between property values and charter schools. The political economy of charter schools has many other aspects, most of which remain largely unstudied. For example, this paper ignores the issue of charter schools formation, which is not random, and depends on many observable and unobservable neighborhood characteristics. On the one hand, charter schools may be created in lower-income neighborhoods as a way to improve residents' access to better-quality education. On the other hand, residents in higher-income areas might form a charter school to ensure that their children will be surrounded

by peers from similar socio-economic background. Pursuing this line of research will shed light on which neighborhoods benefit the most from charter schools penetration.

Chapter III: Does More Gun Control Save Lives? Evidence from Australian National Firearms Agreement[39]

1. Introduction

Public debate over stricter gun laws remains one of the most important political debates, especially in the aftermath of mass shootings. After a mass shooting on April 28, 1996, near Port Arthur, Tasmania that resulted in the death of 35 people the Australian government responded by promptly securing agreement from all states[40] to implement new, stricter gun ownership regulations. The set of new firearm regulations comprised the National Firearm Agreement (NFA).

The two most important parts of the NFA included a federal ban on the sale, transfer, importation and ownership of certain types of long guns as well as heavier restrictions on civilian ownership of all types of firearms. Illegal as well as legal firearms were subject to a national buyback program, compensating owners for their surrender. According to Reuter and Mouzos (2003), the 1997 federal buyback program resulted in the surrender of over 640,000 banned firearms. In addition, the number of certified firearms owned by civilians decreased from 414,000 to 305,000 over 1996-97. Reuter and Mouzos point out that the 1997 buyback program led to the removal of approximately 20 percent of the total Australian gunstock.[41]

In this paper, we propose two different estimation strategies to evaluate the impact of the 1996 NFA on firearm deaths in Australia at the state level. First, we employ an interrupted time series or regression discontinuity (RD) design to evaluate the relationship between the NFA and

[39] This essay is based on joint work with Benjamin Ukert of Georgia State University.
[40] Australia consists of six states (New South Wales, Victoria, Queensland, South Australia, Western Australia, and Tasmania), and two territories (Australian Capital Territory and Northern Territory). However, for consistency we will use "state" to refer to both states and territories. All states have ratified the agreement within a year from its proposal.
[41] The results of 1997 buyback program would be comparable to the removal of approximately 40 million firearms in the United States (Reuter and Mouzos 2003).

changes in firearm deaths. An RD design assumes that variation in the treatment near the threshold is in essence locally randomized due to the agents' inability to precisely manipulate the timing of the introduction of the law. In other words, we expect that covariates, such as income, employment, urbanization, population, and crime are not dramatically different right before and right after the introduction of the law in 1996. Additionally, RD requires less restrictive assumptions relative to other non-experimental methods.

Second, we rely on a panel data approach, controlling for macro- and microeconomic factors that could affect gun deaths and gun related activities, using a difference-in-differences (DID) estimation strategy that gives us variation in time, and state pre-NFA firearm death rates. The analysis exploits state variation in the pre-1996 firearm mortality comprising firearm homicides, firearm suicides and accidental/undetermined firearm deaths. The DID assumes that the effect of the NFA is stronger in states with higher pre-treatment firearm death rates. This identification strategy allows us to include time fixed effects and estimate the causal effect of the NFA, something only one prior paper addresses (Leigh and Neill 2010). Similar estimation strategies have been applied in the health literature evaluating the effect of health insurance reform (Courtemanche et al. 2016, Finkelstein 2007, Miller 2012). We believe that both identification approaches will assist in shedding more light on the precise causal effect of the NFA on firearm mortality in Australia.

There are a number of reasons to believe that the NFA affected firearm related activities even though the effect of gun ownership on public safety remains theoretically ambiguous. On the one hand, gun owners can protect themselves against intruders.[42] Gun ownership also produces

[42] According to Lott 2013, Great Britain and Canada - countries with strict gun control laws - experience much higher incidences of burglaries where the resident is at home during the attack comparing to the United States. The study also shows that 95% of the time individuals use guns defensively they only need to wave a gun to prevent the attack.

spillover effects since individuals who defend themselves are indirectly defending people around them (Lott 2013). Assuming that criminals behave rationally, if the opportunity cost of committing a crime increases due to a higher probability of self-defense, less crime should be committed. On the other hand, keeping a gun at home increases the probability of accidental injuries, homicides, and suicides (Vernick et al. 1997). The NFA reduced the stock of firearms available to Australian population. Since the buyback program was eventually adopted by all states, individuals could not obtain a replacement gun in a different state. Moreover, Australia has no land borders with other countries making it more difficult for individuals to smuggle firearms into the country. Finally, the NFA also restricted the import of firearms, which combined with the absence of domestic gun manufacturers likely significantly limited the amount of guns available in the country (Neill and Leigh 2010).

Similarly, the extent of the Tasmania massacre could have changed the social acceptance of guns in Australia. For example, the atrocity of the crime and an overall shift in perception that guns enable murdering rather than protecting people, may have had its own effect on gun related crimes. For example, less robberies may have been committed with guns. Independent of the immediate restriction of the law, people may have responded by securing their firearms at home from family members and some people may have even sold their guns to buyers abroad.

The law should have also had an effect on people prone to suicide. Most gun related deaths in Australia are suicides (Kreisfeld 2005). Thus, limiting the availability of guns can have an immediate impact on the largest proportion of firearm victims. Taking guns out of the picture increases the time and effort on individuals trying to commit suicide.

The evidence on the effect of the NFA are mixed. Most studies only estimate an effect utilizing time-series variation. Three studies conclude that the NFA had a significant effect on

firearm suicides (Ozanne-Smith et al. 2004, Chapman et al. 2006, Baker et al. 2007) and firearm homicides (Chapman et al. 2006, Baker et al. 2015), while others find no effect on firearm suicides and firearm homicides (Ozanne-Smith et al. 2004, Baker et al. 2007 and 2015, Klieve et al. 2009, Lee et al. 2010). Identification with national time-series data is complicated. Any estimation strategy relying only on time-series variation requires a strong assumption that the trend in the outcome variable would have continued the same way had the law not been passed. Time-specific shocks at the time of the passage of the NFA such as changes in social attitudes towards firearms ownership could have reduced the firearm related deaths. In other words, any time-specific shocks that potentially affected firearm deaths at the same time as the NFA will be undistinguishable from the effects of the NFA, but will be claimed as a causal effect of the NFA. As a result, the current time series analyses have been criticized for their short-comings (Neill et al. 2008) because they overestimate the effect of the NFA on firearm deaths. We contribute to this literature by presenting a more commonly applied RD model, which also takes advantage of variation across states.

In response to the above mentioned shortcomings in the literature, Leigh and Neill (2010) rely on a panel approach and exploit variation across states and over time that allows the inclusion of time trends. They rely on state variation in firearm buyback rates to identify the effect of the NFA buyback program on firearm mortality rate. They find a negative and statistically significant effect of the NFA buyback program on homicides and suicides. While their paper is a clear step forward, we believe that the gun buyback rates are correlated with state specific unobservable characteristics that may overestimate the effect of the NFA. We provide a more detailed critique of Neill and Leigh' identification strategy in the literature review. In

contrast, we rely on cross-sectional variation in pre-NFA firearm mortality rates and a more extensive set of control variables to identify the effect of the NFA on firearm related activities.

We find negative, but not consistently significant effect of the NFA on firearm mortality in our RD models. However, the NFA appeared to increase non-firearm and non-weapon robberies in New South Wales suggesting that criminals were substituting guns with other means.[43] In our full sample DID regressions we show that the NFA decreased the total firearm death rate by 2.183 per 100,000, for a state with the average 1994-96 pre-treatment mean total firearm deaths rate. The reduction also translates into a 60% decrease in total firearm death rate from pre-NFA levels. The decrease in the total firearm death rate emerges from a 0.746 per 100,000 reduction in the firearm homicide rate and a 1.281 per 100,000 reduction in the firearm suicide rate. The results are robust to model specification as well as placebo tests.

The remainder of the paper is organized as follows. Section 2 discusses the background on the NFA. Section 3 summarizes existing literature. Section 4 describes data. Section 5 lays out the empirical approach. Section 6 presents results. Section 7 concludes.

2. Background on the National Firearm Agreement

The introduction of the National Firearms Agreement (NFA) followed an episode of mass shooting that took place on April 28, 1996, near Port Arthur, Tasmania, when a gunman armed with a semiautomatic rifle killed thirty-five people and injured eighteen others. With support of Prime Minister John Howard, The Australasian Police Ministers' Council (APMC) called a special session on May 10, 1996, to discuss a national plan for uniform firearms regulations across the country. The law was ratified by all states by May 1997. The two most important aspects of the new legislation included the federal ban on the sale, transfer, importation and ownership of all semi-automatic self-loading and pump action long arms as well as tighter gun

[43] This result was obtained using monthly robbery data, which we only have for New South Wales.

regulations for civilians and heavier restrictions on ownership of non-banned firearms. The buyback program that was designed to compensate the owners of the newly made illegal firearms accompanied the federal ban on ownership of certain firearms. Buyback prices were set at the level of retail prices, and did not vary across states. Civilians had an opportunity to sell their legal firearms in addition to the banned ones. Official statistics show that Australians chose to surrender over 640,000 banned firearms as well as 60,000 non-banned firearms nationwide between 1996 and 1997 (Reuter and Mouzos 2003). According to Reuter and Mouzos, the buyback program facilitated the removal of approximately 20 percent of the total amount of firearms in Australia.

The most important restrictions on firearm ownership included a separate permit for each firearm acquisition with a mandatory 28-day waiting period; establishment of the universal firearms registration system; prohibition of firearms' sales by anyone other than authorized dealers; presence of a "genuine" reason for gun ownership, which specifically excluded personal protection; minimum age requirements (18); firearm safety training; presence of identification documents at the time of firearm purchase; storage safety standards; and absence of recent criminal convictions. Leigh and Neill (2010) present a more detailed overview of the NFA's components.

The NFA became the first Australian nationwide set of laws restricting or prohibiting ownership of firearms by civilian population. However, prior- and post-NFA, some individual states designed and implemented their own sale, ownership and storage firearm regulations. The most relevant state legislations include Victoria - Control of Weapons Act of 1990 and Firearms Regulation of 2008, New South Wales - Firearms Regulation of 2006, Queensland -Weapons Act of 1990, Western Australia - Firearms Act of 1973 and Firearms Regulations of 1974, and South

Australia - Firearms Act of 1977.[44] We believe that the timing of state law changes are not close enough to the passage of the NFA and therefore allow us to clearly identify the impact of the NFA on firearm related deaths[45].

3. Literature Review

There have been a large amount of studies evaluating the impact of tighter gun regulations or related gun policies on firearm deaths. Much literature concentrated on evaluating the relationship between gun ownership and criminal activity in the United States (Duggan 2000, Vernick et al. 1997, Lott 2013, Cook 1982, Kleck and Patterson 1993). Some studies found evidence that gun ownership positively affects the amount of criminal activity, while others found negative effects. Lott (2013) provides an overview of the main methodological issues that arise in both time-series and cross-sectional studies. Cross-sectional analysis may be biased due to the fact that regions with higher crime rates often adopt stricter gun laws. The reverse causality issue leads to positive estimates of the effect of stricter gun laws on crime rates. Time-series studies fail to separate the impact of stricter gun laws from other potential causes that induce fluctuations in crime rates. Lastly, Lott mentions behavioral concerns: some people might be more likely to own guns, than others. Probability of gun ownership might be determined by the same factors that determine the likelihood of any particular person being killed with a firearm, which introduces endogeneity issues into the analysis. A solution to the behavioral problem would require a randomized study.

A number of previous studies have also debated the success of the 1996 National Firearm Agreement in reducing crimes related to firearm activity. Most of them perform time-series analysis using publicly available data provided by the Australian Bureau of Statistics (ABS). Lee

[44] Source: Library of Congress.
[45] Controlling for state laws does not change our regression results in DID model.

and Suardi (2010) find little evidence of a significant negative relationship between NFA and firearm deaths using a time series approach based on unknown structural breaks. Klieve et al. (2009) assessed pre- and post-1996 trends in firearm suicides separately for Queensland and Australia using a negative binomial regression analysis. They found no significant relationship between the NFA and male firearm suicides for Queensland, but a negative and significant relationship for Australia. The authors speculated that gradual changes in social behavior as well as cultural norms contributed more towards the observed reduction in male firearm suicides, than the NFA.

Other studies demonstrate the success of the NFA in reducing firearm deaths. Chapman et al. (2006) argue that not only the introduction of the NFA led to the reduction in firearm deaths since 1996, particularly suicides, but also prevented reoccurrence of mass shootings in Australia over the next decades. Given the fact that the NFA was designed to prevent mass shootings rather than just decrease firearm death rates, Chapman et al. argue that the legislation was successful. Baker and McPhedran (2007) use Auto Regressive Integrated Moving Average (ARIMA) to show that NFA implementation contributed to a significant decline in suicides as well as accidental firearm deaths, but that it had an insignificant impact on other firearm deaths. They also find no evidence of potential substitution from firearm homicide towards other weapons. Baker and McPhedran join existing studies in suggesting that other social factors like improved income stability might have contributed to an observed decline in firearm deaths in Australia over the past two decades[46].

Reuter and Mouzos (2003) document a sharp fall in firearm homicides between 1996 and 1999. However, they find evidence of substitution from longarm guns towards handguns that

[46] See also Duggan 2003, Beautrais et al. 2006, Kates 1990, and Kellerman et al. 1993.

remained legal after the NFA[47]. Contrary to other studies, Reuter and Mouzos find no significant effect of the NFA on the long-term downward trend in firearm suicides or accidental firearm injuries.

All the studies described above involve mostly time-series analysis for separate states or for Australia as whole. However, there are a few studies that attempt to perform analysis using variations in time of adoption of stricter gun regulations, or variations in numbers of firearms withdrawn due to the NFA buyback program. Ozanne-Smith et al. (2004) examine the effect of various gun control laws on firearm deaths using time variation in the introduction of those laws in Victoria and the rest of Australia. They find that the tightening of gun laws in Victoria that took place in 1988 led to a more rapid decline in Victoria's firearm deaths relative to the rest of the country prior to 1996. Ozanne-Smith et al. demonstrate that the rest of the country was able to "catch up" with Victoria's reduction in firearm deaths after the 1996 NFA legislation. Also a more recent study by Leigh and Neill (2010) evaluates the relationship between the NFA and firearm deaths using variation in the number of firearms withdrawn by the Australian government across states. They argue that states with more firearms bought back should have experienced a larger decline in firearm deaths relative to states with fewer firearms bought back. Leigh and Neill find that the NFA firearm buyback led to an 80 percent reduction in firearm suicides as well as significant but less precise reduction in firearm homicides at the mean baseline gun buyback level. They also demonstrate that states with larger amounts of firearms bought back experienced larger declines in all firearm deaths.

This study employs a difference-in-differences strategy that relies on state variation in pre-NFA firearm mortality rates. We believe that our strategy represents an improvement over Leigh

[47] Between 1992-1993 and 2000-2001, the proportion of homicides committed with a handgun rose from less than one-sixth of the total number of firearm homicides to almost 50 percent (Mouzos 2002a).

and Neill (2010) for a number of reasons. First, Leigh and Neill (2010) assume that variation in states gun buybacks is exogenous.[48] However, there are many potentially unobservable confounders that can drive variation in buyback rates. For example, states with less crime-prone people may see larger buyback rates. Second, Leigh and Neill's identification strategy assumes that the effect of the NFA is proportional to the gun buyback rates and is absent for a state with zero gun buybacks. However, the NFA had other regulations besides buybacks, which included strict ownership restrictions, this assumption less plausible.

In contrast, we rely on state variation in firearm death rates pre-NFA. We believe that this is a weaker assumption, since the effect of the NFA should be proportional to previous firearm use. Therefore, states with zero pre-treatment firearm death rates should see no effect on firearm death rates post NFA. Our specification measures the effect of the NFA through both its sale ownership regulations and the gun buyback program. Specifically, states with higher firearm deaths implicitly have more guns or more crime. We also include an extensive set of socio-economic characteristics to control for observable time-variant differences across Australian states.

4. **Data**

We obtain annual data for the national level and the eight states from the Australian Bureau of Statistics (ABS) and firearm states data from the dataset used by Leigh and Neil (2010), which is available online. At the state level we investigate how the regulation affected firearm suicide rate, firearm homicide rate, accidental and undetermined firearm death rate, and total firearm

[48] Leigh and Neill (2010) discuss and test for the possibility of endogenous gun buyback rates with an instrumental variable approach which by itself requires strong assumptions (Angrist and Imbens 1995).

death rate per year from 1968 to 2002.[49][50] In all cases we standardize the dependent variable by population count and generate rates per 100,000 residents. In our panel data analysis we combine our dependent variables with information on the national unemployment rate, urbanization level, the proportion of people between the ages of 20 and 25, in five year increments up to the proportion of people above 65, and average annual earnings per person. We also control for existing state gun regulations as a robustness check. However, we do not have full information on all control variables dating back to 1968. Thus, our preferred specifications only include the time frame for 1971-2002.[51]

Our DID approach identifies the effect of the NFA using variation in three year (1994-1996) mean pre-treatment total firearm death rates across states. Since the total level of firearm deaths is relatively low, even small changes in gun violence can lead to a significant spike in the firearm death rate in a state with low level of population (i.e. Tasmania, Australian Capital Territory, and Northern Territory). Thus this measure should reflect the overall trend and level of firearm deaths in each state. We also construct a two-year average (1995-1996) and test if our results are significantly different.

Lastly, for our RD design, we utilize the aforementioned annual data and monthly information on firearm, non-firearm, and non-weapon robberies from January 1995 to December 1997 for New South Wales from the New South Wales Bureau of Crime Statistics and Research. Unfortunately, we do not have information on firearm suicides, firearm homicides, and total firearm deaths by month.

[49] In addition, we have state information on firearm, non-firearm and non-weapon robberies for 1993-1997, which is a significantly shorter panel.
[50] McPhedran et al. (2012) report that the ABS systematically undercounted firearm homicides and suicide since 2003 (Bradley et al. 2011, De Leo 2007, Elnour et al. 2009).
[51] We only have unemployment information for some states beginning in 1975. This reduces our observations to 243 with all control variables.

Table 12 presents descriptive statistics for the means and standard deviations for the dependent and control variables by state. The average total firearm death, firearm suicide and firearm homicide death rates per 100, 000 residents vary across state from 2.5-8.3, 1.8-5.0, and 0.3-1.7, respectively. The 1994-1996 pre-treatment average firearm death, firearm suicide and firearm homicide death rates per 100, 000 residents are 3.62, 2.56, 0.78 respectively. We use these values to estimate the effect of the NFA at the mean level.

Table 12: Summary Statistics

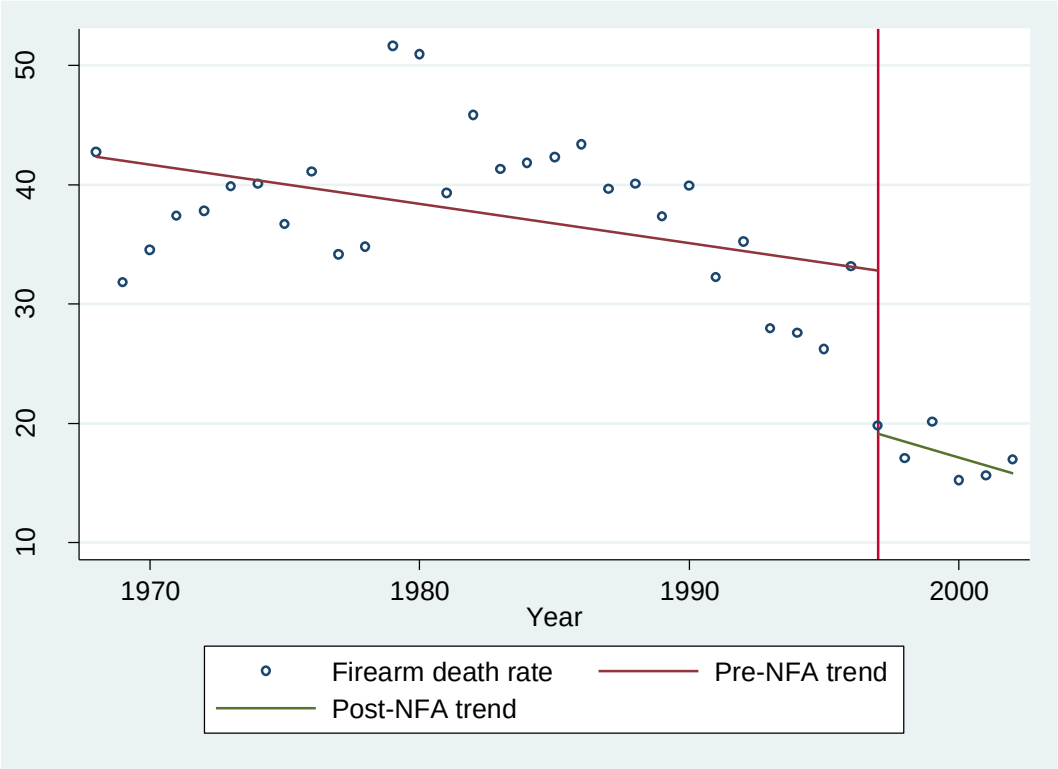
VARIABLES	N	Australian Capital Territory	New South Wales	Northern Territory	Queensland	South Australia	Tasmania	Victoria	Western Australia
Firearm Death Rate	35	2.450 (1.644)	3.253 (0.964)	8.264 (3.345)	4.975 (1.469)	3.588 (1.175)	6.464 (2.131)	3.117 (1.058)	2.512 (0.729)
Firearm Suicide Rate	35	1.882 (1.228)	2.359 (0.676)	4.995 (2.370)	3.986 (1.217)	2.881 (0.951)	4.903 (1.933)	2.327 (0.781)	1.976 (0.542)
Firearm Homicide Rate	35	0.253 (0.461)	0.551 (0.172)	1.679 (1.373)	0.568 (0.282)	0.483 (0.263)	0.837 (1.243)	0.490 (0.233)	0.271 (0.153)
Firearm Accident Rate	35	0.315 (0.478)	0.343 (0.251)	1.590 (1.369)	0.421 (0.223)	0.225 (0.184)	0.723 (0.413)	0.301 (0.187)	0.265 (0.213)
Non-Firearm Death Rate	35	35.830 (7.725)	48.315 (10.271)	97.108 (25.310)	53.398 (11.298)	45.692 (7.114)	51.640 (11.066)	45.338 (10.023)	48.485 (9.817)
Non-Firearm Suicide Rate	35	8.211 (2.579)	9.604 (1.554)	8.156 (5.978)	10.01 (2.034)	9.509 (2.021)	8.643 (2.130)	9.008 (1.664)	10.13 (1.863)
Non-Firearm Homicide Rate	35	0.806 (0.643)	1.252 (0.260)	10.16 (4.018)	1.463 (0.327)	1.079 (0.362)	0.891 (0.512)	0.988 (0.247)	1.379 (0.390)
Non-Firearm Accident Rate	35	26.81 (8.439)	37.46 (11.00)	78.79 (26.28)	41.93 (12.16)	35.10 (8.591)	42.11 (11.80)	35.34 (10.78)	36.97 (10.83)
Control Variables									
Unemployment Rate	27	6.181 (1.073)	6.143 (2.717)	6.442 (1.416)	6.874 (2.756)	7.097 (2.811)	7.380 (3.266)	6.000 (2.818)	6.117 (2.673)
Earnings per Week	30	475.6 (188.5)	381.4 (206.4)	444.9 (163.3)	346.7 (182.1)	344.0 (184.1)	339.6 (173.3)	369.2 (194.9)	358.7 (187.9)
Percent Age 20 to 25	32	11.17 (0.909)	9.511 (0.743)	12.27 (1.213)	9.742 (0.663)	9.488 (0.923)	9.182 (1.031)	9.723 (0.685)	9.951 (0.816)
Percent Age 26-30	32	9.201 (0.948)	7.949 (0.343)	10.88 (0.775)	7.832 (0.381)	7.744 (0.556)	7.470 (0.649)	7.990 (0.349)	8.241 (0.515)
Percent Age 31-35	32	8.678 (0.805)	7.572 (0.601)	9.492 (0.571)	7.386 (0.685)	7.368 (0.737)	7.210 (0.720)	7.571 (0.654)	7.823 (0.683)
Percent Age 36-40	32	7.874	7.058	7.912	6.930	6.902	6.814	7.016	7.274

		(0.971)	(0.825)	(1.002)	(0.938)	(0.950)	(0.946)	(0.860)	(0.929)
		6.865	6.448	6.292	6.331	6.372	6.251	6.398	6.547
Percent Age 41-45	32	(1.138)	(0.786)	(1.166)	(0.970)	(0.926)	(0.981)	(0.826)	(0.982)
		5.857	5.914	4.967	5.736	5.906	5.710	5.854	5.764
Percent Age 46-50	32	(1.162)	(0.661)	(1.115)	(0.838)	(0.799)	(0.835)	(0.679)	(0.858)
		4.632	5.335	3.681	5.105	5.389	5.181	5.243	4.922
Percent Age 51-55	32	(0.996)	(0.506)	(0.893)	(0.641)	(0.624)	(0.638)	(0.511)	(0.646)
		3.397	4.743	2.527	4.478	4.830	4.650	4.617	4.177
Percent Age 56-60	32	(0.582)	(0.258)	(0.499)	(0.294)	(0.317)	(0.294)	(0.242)	(0.269)
		2.520	4.229	1.645	3.968	4.360	4.177	4.094	3.625
Percent Age 61-65	32	(0.521)	(0.209)	(0.280)	(0.171)	(0.288)	(0.202)	(0.194)	(0.161)
		4.916	10.18	2.298	9.482	10.91	10.17	10.05	8.559
Percent Age 65+	32	(1.829)	(1.501)	(0.555)	(0.964)	(2.041)	(1.784)	(1.457)	(1.096)
		50.09	49.83	53.35	50.18	49.63	49.68	49.61	50.51
Percent Male	32	(0.525)	(0.169)	(1.071)	(0.174)	(0.142)	(0.292)	(0.218)	(0.318)
		0.0007	0.0008	0.005	0.0002	0.002	0.001	0.001	0.003
Urban Growth Rate	35	(0.003)	(0.005)	(0.03)	(0.008)	(0.004)	(0.001)	(0.003)	(0.005)

Notes: Standard deviation in parentheses, N represents the maximum number of observations for a state.

Lastly, Figure 6 presents the national trend in firearm deaths across time. We see that after 1997 the trend decreases more sharply, providing some preliminary evidence that the NFA may have in fact affected the firearm death rate.

Figure 6: Trends in the firearm death rate pre and post NFA



5. **Identification Strategy**

We apply two identification strategies to evaluate the relationship between the NFA and firearm death rate. First, we apply an interrupted time series (ITS) or sharp Regression Discontinuity design that is similar to the approach taken in the literature (Lee and Suardi 2010). A sharp RD design relies on several identification assumptions (Hahn, Todd, and van der

Klauuw 2001). Most notably, it relies on the assumption that the mortality trend without the law would have been continuous before and after 1997. In other words, observable characteristics right before the law and right after the law should be similar and the timing of the law is as good as random. Therefore, if the law would have been passed in 1995 we should have seen a similar effect of the NFA on mortality and gun related activities. Our RD design at the state level can be represented in a reduced form model:

$$Mortality_{it} = f(t_i, t) + \beta_1 * Postlaw_{it} + e_{it} \quad (3)$$

Where *Mortality* represents the mortality rate from firearm related death, which includes suicide, homicide, and accidental death in state *i* and year *t*; t_i represents time, measured in years from the passage of the NFA; $f(\cdot)$is a function that is continuous in 1997 with parameter vector t – flexible polynomial; and *Postlaw* is an indicator for whether the time is after the NFA (Card et al. 2009). We can interpret out coefficient of interest β_1 as an estimate of the effect of the NFA on firearm mortality rate. The law indicator variable equals 1 once the law passed. For our annual data analysis this would mean that the law switches to 1 in 1997. We interpret the beta coefficient as the causal effect of the law on the firearm mortality rate.

There are some legitimate concerns with the RD approach. It is unlikely that the trends across time are continuous especially for an outcome variable such as firearm deaths. Accordingly, the RD effect emerges as an immediate jump after the cutoff, in this case the month after the passage of the law, which seems unlikely to occur since it took longer to fully implement all aspects of the NFA. We expect that important components of the law would only affect mortality over time. For example, the gun buyback program requires participant's knowledge of refunds if they hand in their gun and it is unlikely that most residents responded quickly. There could also be an anticipation effect of the law violating the RD assumption, where individuals change behavior in

response to the announcement of a law prior to its actual implementation. Anticipation effects would lead to a shift in trend or discontinuity to the announcement of a change rather than when the change goes into law. Normally, RD requires that there would have been no fluctuations of firearm deaths from 1996 to 1997 that are unrelated to the law. We believe that it is quite possible to have fluctuations across years. They can be seen in Figure 6 for several time periods.

As a result, we also rely on a panel specification that allows for fluctuations across time in our outcome variable while controlling for several other factors that may influence firearm deaths. We estimate:

$$Mortality_{it} = \beta_0 + \beta_1 * Postlaw_t + \beta_2 * year_{it} + X_{it} + \tau_{it} + \varepsilon_{it} \quad (4)$$

Where β_1 is the same as defined in Equation (3), $year_t$ is a binary indicator for each year (time fixed effect), τ_{it} is state fixed effect, and X_t is a vector of control variables. We also estimate Equation (4) in separate regressions with a 2nd and 3rd polynomial time trends.

Our preferred methodology takes advantage of variation across states to identify the effect of the NFA on the firearm mortality rate. We utilize variation across states in pre-treatment firearm death rate in 1994-1996.[52] Our specification suggests that those states with a higher level of firearm death rate in 1994-1996 should be affected by the law to a larger extent than those states with lower levels of firearm death rate in 1994-1996. In other words, we believe that the effect of the law corresponds to the prior mortality level, because it reflects the underlying heterogeneity in firearm related crimes by state. Taking advantage of this variation allows us to add time fixed effects to capture changes in the outcome that would have occurred had the law not been passed.

[52] Each state had to ratify the NFA to make it a law. As a result, in most states buyback program as well as sale and ownership restrictions on firearms did not take effect until late 1996-early 1997. Therefore, even though the NFA regulations were proposed in mid-1996, we treat 1997 as the first year in our post-NFA time period.

This specification is equal to a difference-in-differences (DID) model:

$$Mortality_{it} = \beta_0 + \beta_1 * Mortality_i * Postlaw_t + \tau_i + \lambda_t + X_{it} + \varepsilon_{it} \quad (5)$$

Where $Mortality_i$ is the mean 1994-1996 firearm death rate in state *i*, and λ_t is a year fixed effect. In Equation (5), the effect of the law is given by $\beta_1 * Mortality_i * Postlaw_t$ and is proportionate to the pre-law mortality level increasing linearly with the mortality rate. We believe that this is a plausible identification because it assumes that the law has no effect at a mortality level of 0, while allowing for a time fixed effect.

6. **Results**

6.1 Regression Discontinuity

Tables 13-17 present the regression results for the RD estimation strategy on the effect of the NFA on total firearm death rate, firearm suicide rate, firearm homicide rate, and accidental and undetermined firearm death rate, respectively. In each table columns 1-3 show the results utilizing state variation in three regressions where the first includes only a dummy variable for the law, the second includes a quadratic time trend, and the third includes a quadratic and a cubic time trends. All specifications include state fixed effects.

The coefficient in the first column of Table 13 suggests that the law decreased the total firearm death rate by 2 per 100,000 residents. However, the result is not robust to adding time trends.

Table 13: The effect of the NFA on the firearm death rate (RD)

	(1)	(2)	(3)
	Total Firearm Death Rate (State)		
Law	-2.069***	-0.548	-0.714
	(0.569)	(0.572)	(0.440)
Quadratic Time Trend	NO	YES	YES
Cubic Time Trend	NO	NO	YES
State Fixed Effect	YES	YES	YES
Sample Size	280	280	280
R-squared	0.709	0.732	0.733

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Tables 14 and 15 indicate that the NFA caused a decline in firearm suicide rate and firearm homicide rate, though the effect is not robust to the addition of the flexible time trend. Only in Table 16 illustrating the effect of the NFA on the accidental and undetermined intent firearm death rate the coefficient switches sign. Overall, our findings using RD design do not show evidence of consistent relationship between the NFA and firearm mortality rates.

Table 14: The effect of the NFA on the firearm suicide rate (RD)

	(1)	(2)	(3)
	Firearm Suicide Rate (State)		
Law	-1.776***	-0.462	-0.506*
	(0.422)	(0.269)	(0.235)
Quadratic Time Trend	NO	YES	YES
Cubic Time Trend	NO	NO	YES
State Fixed Effect	YES	YES	YES
Sample Size	280	280	280
R-squared	0.610	0.648	0.648

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 15: The effect of the NFA on the firearm homicide rate (RD)

	(1)	(2)	(3)
	Firearm Homicide Rate (State)		
Law	-0.393*	-0.328	-0.320
	(0.186)	(0.328)	(0.407)
Quadratic Time Trend	NO	YES	YES
Cubic Time Trend	NO	NO	YES
State Fixed Effects	YES	YES	YES
Sample Size	280	280	280
R-squared	0.318	0.318	0.318

Notes: Heteroscedasticity Robust Standard errors clustered at the state level are in parentheses.

Table 16: The effect of the NFA on the accidental and undetermined intent firearm death rate (RD)

	(1)	(2)	(3)
	Firearm Accidental and Undetermined Intent Death Rate (State)		
Law	0.100**	0.243	0.112
	(0.034)	(0.128)	(0.073)
Quadratic Time Trend	NO	YES	YES
Cubic Time Trend	NO	NO	YES
State Fixed Effects	YES	YES	YES
Sample Size	280	280	280
R-squared	0.447	0.450	0.453

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

We also present results for the RD design using monthly firearm robbery information from New South Wales for the time frame from January 1995 to December 1997. This specification may be more plausible than using annual data as it would capture the immediate discontinuity in the month following the passage of the law by New South Wales government. However, we cannot make an apples-to-apples comparison because the dependent variable is different. Table 17 shows the same three time trend specifications as in the previous tables. Columns 1-3 present

the effect of the NFA on the non-weapon robbery rate and columns 4-6 present results for the non-firearm robbery rate. We find that both non-firearm and non-weapon robberies increased. The results suggest that criminals may have substituted guns for other tools or stopped using weapons during robberies. Specifically, after the passage of the NFA in New South Wales in December of 1996, non-weapon robberies increased by 1.3 from a baseline mean non-weapon robbery rate of 6.41 in 1995-1996 or over 20%, and non-firearm robberies increased by almost 1 from the baseline mean non-weapon robbery rate of 2.86 in 1995-1996 or by about 35%.

Table 17: The effect of the NFA on firearm robberies in New South Wales (RD)

	(1)	(2)	(3)	(4)	(5)	(6)
	Robbery Rate w/o Weapon			Non-firearm Robbery Rate		
Law	1.324***	1.546**	1.321**	1.126***	0.688*	0.993**
	(0.484)	(0.579)	(0.641)	(0.313)	(0.348)	(0.367)
Quadratic Time Trend	NO	YES	YES	NO	YES	YES
Cubic Time Trend	NO	NO	YES	NO	NO	YES
Sample Size	36	36	36	36	36	36
R-squared	0.643	0.648	0.656	0.873	0.892	0.904

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

In summary, RD results show that the effect of the NFA on firearm mortality rate is negative, but not robust to the addition of flexible time trend. We also find evidence that non-firearm and non-weapon robberies increased in New South Wales after the passage of the NFA. Our results are in line with the previous literature that finds no significant effect of the NFA with similar identification strategy (Ozanne-Smith et al. 2004). Given the assumptions of the RD design, we believe that identifying a robust effect across time is implausible for two reasons. The use of annual data is inappropriate and the legislation should not have necessarily led to an immediate discontinuous drop in firearm related death rates. Instead we believe that the law's components

can be captured across time with a fully specified model that includes time and year fixed effects. The next section presents our results for those regressions.

6.2 Difference-in-Differences

Panel results of Equation (4) which estimates the effect of the NFA with a dummy variable on the total firearm death rate can be found in Table 25, Appendix 3. We believe that this estimation strategy is a naïve approach. The inconceivable magnitude of the estimated results, we find point estimates ranging from -8.377 to -12.69, suggest that it is likely that the coefficients capture a significantly large time fixed effect. Therefore, we focus our discussion on the preferred panel DID model of Equation (5) in Table 18.

Table 18: The effect of the NFA on the total firearm death rate (DID)

	(1)	(2)	(3)	(4)	(5)
Post*1994-1996 Total Firearm Death Rate	-0.587***	-0.567***	-0.579***	-0.602***	-0.459***
	(0.053)	(0.071)	(0.097)	(0.160)	(0.106)
Implied Effect of NFA at Mean 1994-1996 Total Firearm Death Rate					
NFA	-2.127***	-2.054***	-2.098***	-2.183***	-1.665***
	(0.192)	(0.259)	(0.353)	(0.580)	(0.384)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.801	0.812	0.819	0.830	0.796

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

In each column we present the point estimate and the computed implied effect of the NFA on the firearm death rate at the mean-pre-treatment 1994-1996 firearm death rate. Thus, the implied

effect of the NFA equals $\beta_1 * \overline{Mortality}_i$, where $\overline{Mortality}_i$ = 3.62 firearm deaths per 100,000 residents. Each column also presents a different set of control variables. Column 1 includes economic and regional controls and state and time fixed effects. Columns 2-5 increase the amount of control variables in the model: column 2 adds the percent of male population in each state, column 3 adds the percent of young (19<age<26) and old (age>65) residents in each state, column 4 adds a full specification for each age group in five year intervals, and column 5 adds a 2nd order polynomial time-trend.

Our preferred specification includes all controls (column 4) and shows that the NFA reduced the firearm death rate by 2.183 in a state with the mean pre-treatment firearm death rate.[53] Across specification our results are robust and statistically significant at the 1% level. In other words, the NFA reduced firearm death rate by 60% from mean 1994-1996 pre-NFA levels of 3.62. Queensland has the closest pre-treatment mean of 4.14, which would imply that Queensland should see a reduction in the firearm death rate of 2.49 units to a post-treatment level of 1.65. The average three year post-treatment total firearm death rate from 1997-1999 equals 2.55 in Queensland suggesting that Queensland saw an actual reduction of 1.59 in the firearm death rate relative to the predicted 2.49 drop. This indicates that our model over predicts. However, depending on the model specification in Table 18, the NFA reduces the firearm death rate from roughly 1.7 to 2.2 for a state with the average pre-treatment total firearm death rate. Thus, our lower bound effect is not far from Queensland's actual drop.

Given the large heterogeneity in the pre-treatment firearm death rates by state (ranging from 1.31 to 7.13 firearm deaths per 100,000), the effect of the NFA for the state with the lowest and

[53] As a robustness check, we also control for any existing state gun regulations, but the coefficients remain similar to the ones in Column 4 of Tables 18-21.

the highest pre-treatment firearm death rate equals 0.79 and 4.29, respectively. This suggests that the NFA reduced the firearm death rate in a state with the lowest pre-treatment firearm death rate by 0.79 deaths per 100,000 residents, and reduced the firearm death rate in the state with the highest pre-treatment firearm death rate by 4.29 deaths per 100,000 residents. Overall, the results indicate that the NFA was effective in reducing firearm deaths in Australia. We also replace our variable of interest, the three year average pre-treatment firearm death rate, with a two year average, 1995-96, firearm death rate and a one year average, 1996, firearm death rate, and find similar results.

We also investigate how the NFA affected firearm suicides, homicides and accidents. Tables 19 through 21 present results for the firearm suicide rate, firearm homicide rate, and accidental and undetermined firearm death rate, respectively. In Table 19 our preferred specification shows that the NFA decreased the firearm suicide rate by 1.281 deaths per 100,000 residents. This results is statistically significant at the 5% level. Across model specifications we find similar effects ranging from 1 to 1.3 deaths per 100,000 residents. Relative to the three year pre-treatment baseline firearm suicide rate of 2.56, the results suggest that the NFA lowered the firearm suicide death rate by 50%. Again, given the large heterogeneity in the pre-treatment firearm death rates by state, the effect of the NFA can reduce firearm suicide rates by as low as 0.46 and as high as 2.52 deaths per 100,000 residents.

Table 19: The effect of the NFA on the firearm suicide rate (DID)

	(1)	(2)	(3)	(4)	(5)
Post*1994-1996 Total Firearm Death Rate	-0.322*** (0.085)	-0.322** (0.096)	-0.346*** (0.094)	-0.353** (0.135)	-0.274** (0.090)
Implied Effect of NFA at Mean 1994-1996 Total Firearm Death Rate					
NFA	-1.168 *** (0.307)	-1.166** (0.347)	-1.253*** (0.339)	-1.281** (0.489)	-0.992** (0.328)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.744	0.758	0.762	0.773	0.727

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Similarly, our results on the effects of the NFA on the firearm homicide rates in Table 20 suggest that the NFA reduced the firearm homicide rate by 0.746 in a state with the mean pre-treatment firearm death rate. Given a three-year mean firearm homicide rate of 0.78, the NFA appeared to reduce the firearm homicides rate by 96% for a state with the mean pre-treatment firearm death rate. The NFA reduced firearm homicide rates by as low as 0.27 and as high as 1.47 deaths per 100,000 residents depending on the state mean pre-treatment death rates. Finally, we find no significant effect of the NFA on accidental and undetermined firearm death rate in Table 21.

Table 20: The effect of the NFA on the firearm homicide rate (DID)

	(1)	(2)	(3)	(4)	(5)
Post*1994-1996 Total Firearm Death Rate	-0.222***	-0.218***	-0.210***	-0.206**	-0.191*
	(0.043)	(0.037)	(0.025)	(0.069)	(0.084)
Implied Effect of NFA at Mean 1994-1996 Total Firearm Death Rate					
NFA	-0.804***	-0.790***	-0.762***	-0.746**	-0.691*
	(0.155)	(0.135)	(0.091)	(0.250)	(0.305)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.422	0.424	0.439	0.468	0.398

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 21: The effect of the NFA on the accidental and undetermined intent firearm death rate (DID)

	(1)	(2)	(3)	(4)	(5)
Post*1994-1996 Total Firearm Death Rate	-0.043	-0.027	-0.023	-0.043	0.005
	(0.033)	(0.024)	(0.021)	(0.049)	(0.041)
Implied Effect of NFA at Mean 1994-1996 Total Firearm Death Rate					
NFA	-0.154	-0.098	-0.083	-0.156	0.018
	(0.119)	(0.089)	(0.076)	(0.179)	(0.148)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.504	0.530	0.536	0.558	0.469

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

We also test whether the non-firearm death rate changes after the NFA. For example, it is possible that the limited access to guns after the NFA led to more homicides and suicides committed by other means. In other words we might expect a substitution effect. Tables 26-29 in Appendix 3 present the results for total non-firearm death rate, non-firearm suicide rate, non-firearm homicide rate and non-firearm accidental and undetermined death rate. We find some evidence that non-firearm accidental and undetermined intent decreased after the NFA. Our preferred specification is not statistically significant, but other specifications with less control variables show a significant effect with relatively similar point estimates. This is a surprising and interesting finding. We are unsure what explains this effect. We speculate that coroners may have classified some cases that are firearm related as non-firearm related.

Overall, we find that the NFA decreased firearm death rates, with a larger proportion of the total decrease coming from firearm suicides than firearm homicides.[54] The results are not surprising. About 71% of all firearm deaths in Australia are suicides[55] and therefore we would expect to find that the NFA had a proportionally larger effect on firearm suicides than firearm homicides. We also find that the NFA decreased the firearm homicide rate dramatically by about 96% for a state with the mean pre-treatment total firearm death rates. However, we cannot disentangle how the NFA's gun buyback program and the gun possession regulations separately affected the reduction in firearm suicides and firearm homicides. We speculate that the gun buyback program affected firearm deaths over-time by reducing the overall number of guns in the country and increased the price of guns on the black market, thus possibly reducing the

[54] We also redo all estimations with the change in total firearm death rate, change in firearm suicide rate, change in firearm homicide rate, and change in accidental and undetermined firearm death rate as dependent variables to make sure we don't just pick up existing long-term downward trends in firearm mortality. The coefficients remain negative and significant. However, their magnitudes increase. This suggests that our main results might represent conservative estimates of the effect of the NFA on firearm mortality.

[55] Based on the three year pre-treatment means.

amount of firearm homicides as other means of weapons were relied on for criminal activity. At the same time, gun regulations such as the 28-day waiting period for the purchase of new guns, may have had an immediate effect on people trying to commit a suicide on impulse.

6.3 Robustness

The identification of the DID relies on the assumption that had the NFA not taken place changes in firearm death rates would not have been correlated with pre-treatment firearm death rates. We test if our specification is robust by running regressions where we change the date of the NFA implementation to an earlier year. Specifically, we run "placebo" treatments in the pre-treatment periods 1990-1995, giving us 6 estimates from our regression including all control variables. Finding significance in any of those regressions suggests that the changes in firearm deaths may have occurred even without the NFA. Table 22 presents the placebo results for the total firearm death rate. Each column presents estimates for a regression changing the NFA enactment to a different year beginning with 1990. All regressions from 1990-1995 indicate no significant effect of the NFA on total firearm death rate. These results support a conclusions that our main results have a causal interpretation. [56]

[56] Postdating the treatment to any year from 1999 to 2002 shows significant effects. The gun buyback program was not limited to 1997, instead there were periodically gun buyback programs up until 2001 (Reuter and Mouzos 2003). Thus, the differential level of gun buyback purchases, implemented by different states can be correlated with the pre-treatment firearm death rate. As a result, those estimates may in fact display the continuous impact of the NFA in later years and presents evidence that the gun buyback program was indeed an effective tool in reducing firearm deaths rather than serving as a true placebo test.

Table 22: Placebo treatment tests

Placebo Treatment year	1990	1991	1992	1993	1994	1995
Post*1994-1996 Total Firearm Death Rate	-0.0627 (0.243)	-0.295 (0.249)	-0.230 (0.262)	-0.336 (0.266)	-0.299 (0.266)	-0.309 (0.252)
State Fixed Effects	YES	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES	YES
Percent male population	YES	YES	YES	YES	YES	YES
Percent young and old population	YES	YES	YES	YES	YES	YES
More ages	YES	YES	YES	YES	YES	YES
Sample Size	243	243	243	243	243	243
R-squared	0.813	0.820	0.817	0.820	0.818	0.818

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Similarly to Leigh and Neill (2010) we drop 1996 due to its outlier observations of the increased homicide rate in Tasmania and the potential reduced firearm related activity due to the mourning of the nation. Dropping 1996 does not change our main results. We also allow for the possibility that the data collection in earlier years was less accurate and limit the sample to 1979-2002. Across all specifications we find similar results as reported in Table 18.

7. **Conclusion**

Our paper revisits the literature estimating the impact of the NFA on firearm-related activity. First, we use an RD design and find negative, but not robust effect of the NFA on firearm mortality rates. However, we find that non-firearm and non-weapon robberies in New South Wales have increased following the passage of the NFA possibly suggesting substitution away from gun use. Second, we apply a DID method that relies on state variation in the pre-treatment firearm death rates. Our main results show that the NFA reduced the total firearm death rate by 60% for a state with the mean pre-treatment total firearm death rate. The results are robust to model specification and placebo tests.

Our DID approach and findings extend the current literature. First, we provide evidence that RD may not reveal treatment effects in cases when only annual data is available. Additionally, the NFA had several components with many of those components requiring time to implement and enforce, which limits the researcher's ability to pinpoint an exact treatment timeframe.

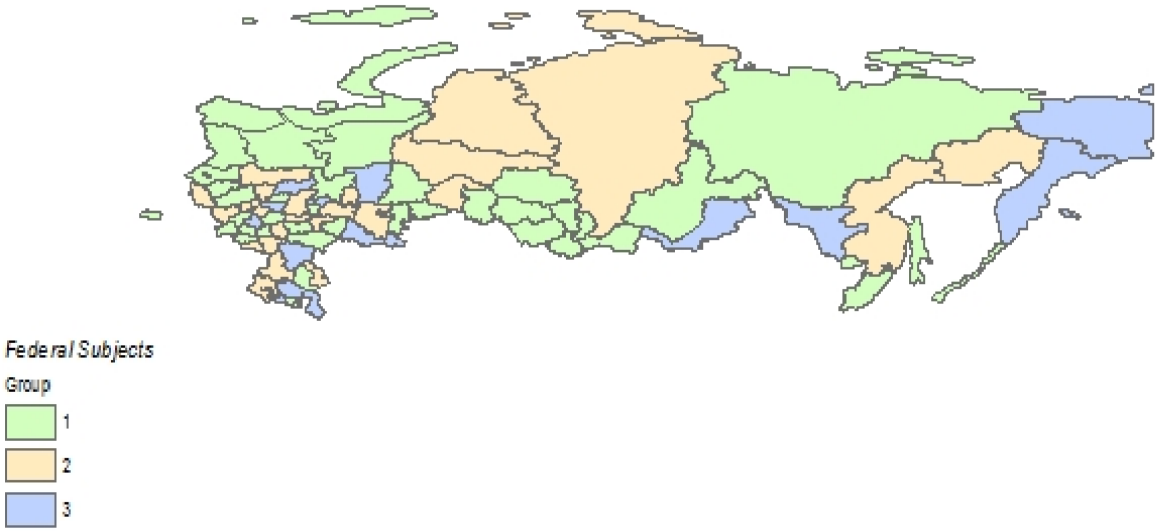
Second, we rely on cross-sectional variation that allows us to identify the impact of the NFA given the plausible assumption that it had no effect in states with a zero pre-NFA firearm death rate. This approach also allows us to include time and state fixed effects, something that is not common in studies measuring the effect of national policies over time. We also believe that our DID approach requires weaker assumptions than Leigh and Neill (2010), but presents stronger and more robust results.

Our results indicate that comprehensive firearm regulations indeed contribute to a decline in firearm mortality. However, due to its relative geographic isolation, Australia might have more favorable conditions for controlling its firearm supply and availability than other countries that share land borders with each other. As a result, we cannot claim that out results have external validity, and would be applicable if other nations pass comprehensive firearm regulations.

Future research on federal firearm regulations should concentrate on disentangling various components of those regulations to identify which regulation most significantly affects firearm mortality (i.e. buyback program, waiting period, registration, mandatory training, etc.) This will allow policy makers to focus their attention on specific ways to combat negative aspects of civilian gun ownership.

Appendix 1

Figure 7: Federal subjects by groups



Group 1

Altai, Altai Krai, Arkhangelsk, Bryansk, Chelyabinsk, Chuvashia, Ingushetia, Irkutsk, Ivanovo, Jewish Autonomous Region, Kabardino-Balkaria, Kaliningrad, Kalmykia, Kaluga, Karelia, Kemerovo, Khakassia, Kirov, Komi, Kurgan, Kursk, Leningrad, Lipetsk, Murmansk, Nenets Autonomous Region, North Ossetia – Alania, Novgorod, Novosibirsk, Omsk, Orel, Penza, Primorye, Ryazan, Sakhalin, Samara, Saratov, Sverdlovsk, Taymyr Autonomous Region, Tomsk, Tuva, Tver, Yakutia

Group 2:

Adygea, Astrakhan, Bashkortostan, Belgorod, Evenk Autonomous Region, Khabarovsk, Khanty-Mansi Autonomous Region, Koryak Autonomous Region, Krasnodar, Krasnoyarsk, Magadan, Mordovia, Moscow, Moscow City, Nizhegorodsk, Pskov, Rostov, Smolensk, Tambov, Tatarstan, Tyumen, Udmurtia, Ulyanovsk, Ust-Orda Buryat Autonomous Region, Vologda, Voronezh, Yamalo-Nenets Autonomous Region, Yaroslavl

Group 3:

Amur, Buryatia, Chukotka Autonomous Region, Dagestan, Karachay-Cherkessia, Kostroma, Mari El, Orenburg, Perm, St. Petersburg, Stavropol, Tula, Vladimir, Volgograd

Table 23: Impact of appointment law induced affiliation with the central party at any point in time on public utilities and transportation

VARIABLES	(1) Log heat pc	(2) Log sewage pc	(3) Log water pc	(6) Log road volume	(7) Log bus volume
Party Change * Post	-0.003	-0.044	0.030	-0.012	-0.025
	(0.066)	(0.045)	(0.035)	(0.032)	(0.086)
Socio-economic characteristics	YES	YES	YES	YES	YES
R-squared	0.115	0.387	0.232	0.613	0.182
Observations	899	825	814	872	896

Notes: Treatment group is all states that have changed their party affiliation to UR at any point between 2005 and 2012. Each column reports the estimated coefficients of a separate regression model. All regression include year and state fixed effects. Standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Background on opposition parties in Russia.

UR is the party of power in Russia. It has been holding the majority of Parliament seats since 2003. However, there exist a number of other parties, which are either minority parties represented in the Parliament, or are not represented in the Parliament at all. For the purposes of this analysis, all parties that do not hold the majority of Parliament seats are treated as opposition parties.

The opposition parties included in the analysis are
The Communist Party of the Russia Federation (KPRF)**
The Liberal Democratic Party of the Russia Federation (LDPR)**
The Union of Right Forces (SPS)*
The Agrarian Party of Russia (APR)*
The Democratic Party of Russia (DPR)*
The Motherland-National Patriotic Union (Rodina)*
A Just Russia (SR)**
The Russian Party of Social Democracy (RPSD)*
The Transformation of the Urals, (TU)*
The People's Patriotic Union of Russia (NPSR)*
The Russian People's Republican Party (RNR)*
The United Democratic Movement "Solidarity" (UDM)

*Parties do not exist as officially registered parties any longer. Nevertheless, they were relatively important members of political opposition in early and mid-2000's.
**Opposition parties that have been consistently maintaining their seats in the Parliament.

Appendix 2

Table 24: Metropolitan Atlanta charter schools with priority zones

Charter School Name	Year Opened	Grades	Priority zone 1	Priority zone 2	Priority zone 3
Atlanta Neighborhood Charter School – Elementary Campus	2001	K-5	Grant Park and Ormewood Park neighborhoods	Other NPU-W[1] neighborhoods	Other APS district neighborhoods
Atlanta Neighborhood Charter School – Middle Campus	2005	6-8	NPU-W[1]	APS district outside of NPU-W	
Charles R. Drew Charter School	2000	PK-12	Villages of East Lake	East Lake and Kirkwood neighborhoods	APS district attendance zones outside priority zones 1 and 2
DeKalb PATH Academy	2002	5-8	Buford Highway corridor[2]	Clarkston, Stone Mountain, Lithonia zip codes - 30021, 30032, 30034, 30035, 30083, 30088	DeKalb county school system outside priority zones 1 and 2
Kingsley Charter Elementary School[3]	1998 (1970)	PK-5	Kingsley Attendance Zone	DeKalb County School District attendance zones outside of priority zone 1	

KIPP South Fulton Academy	2003	5-8	Conley Hills, Holmes, Hapeville, Mt. Olive, Oak Knoll, Park Lane, Harriet Tubman, and Brookview elementary school attendance zones	Seaborn Lee, Heritage, Feldwood, Bethune, Gullatt, Love Nolan, and St. Lewis elementary school attendance zones	Fulton County Schools attendance zone
KIPP STRIVE Academy	2009	5-8	30310 zip code	30311 zip code	APS attendance zone outside priority zones 1 and 2
KIPP STRIVE Primary	2012	K-3[4]	30310 zip code	30311 zip code	APS attendance zone outside priority zones 1 and 2
KIPP VISION Academy	2010	5-8	30315 zip code	30354 zip code	APS attendance zone outside priority zones 1 and 2
KIPP VISION Primary	2013	K-2[5]	30315 zip code	30354 zip code	APS attendance zone outside priority zones 1 and 2
KIPP West Atlanta Young Scholars (WAYS) Academy	2003	5-8	30314 zip code	30318 zip code	APS attendance zone outside priority zones 1 and 2

North Springs Charter High School[6]	2007 (1963)	9-12	FCS designated North Springs attendance zone	FCS district	outside the Fulton County School district
The Museum School of Avondale Estates	2010	K-8	Avondale, Knollwood and Midway Elementary School attendance zones	DeKalb County School District attendance zones outside of priority zone 1	

Notes: Data compiled directly from charters on file with the Georgia Department of Education, with supplemental information provided by Georgia Department of Education annual reports, school websites and contacts (Patrick 2015).

1. Neighborhood Planning Unit W (NPU-W) includes the neighborhoods of Benteen, Boulevard Heights, Custer/McDonough/Guice, East Atlanta, Grant Park, Ormewood Park, North Ormewood Park, and Woodland Hills.
2. The Buford Highway Corridor is defined by I-85 as the southeast boundary, the Fulton-DeKalb county line as the west boundary, the Gwinnett-DeKalb county line as the north boundary, and a line one-half mile to the northwest and parallel to Peachtree Street/Peachtree Industrial Boulevard as the northwest boundary.
3. Kingsley Elementary Charter School is a conversion charter. The conversion occurred in August 1998.
4. KIPP STRIVE Primary opened in July 2012 with 100 kindergarten students, and will grow a grade per year until serving grades K-4. The school serves grades K-3 for 2015-2016 school year.
5. KIPP Vision Primary opened in July 2013 with a 100 kindergarten students, and will add one grade annually to reach full elementary school capacity in July of 2017. KIPP Vision Primary will serve students in grades K-2 during the 2015-16 school year. Additional grades will be added each year until the school serves grades K-4.
6. North Springs Charter High School is a conversion charter school, with conversion occurring in 2007. Fulton County became a Charter System in 2012. North Springs Charter High School began transitioning into the Fulton County Charter System in the 2014-2015 school year as part of Cohort 3. The charter sunsets June 2015, at which time governance transitions to the School Governance Council and the Fulton County Charter System. North Springs will continue to operate its two magnet programs, accepting students from outside the designated attendance zone based upon admissions criteria.

Appendix 3

Table 25: The effect of the NFA on the firearm death rate (fixed effects)

	(1)	(2)	(3)	(4)	(5)
Law	-9.755 (6.361)	-8.377 (4.771)	-12.69* (5.728)	-13.25 (8.914)	-0.749 (0.659)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.777	0.789	0.797	0.813	0.779

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 26: The effect of the NFA on the non-firearm death rate (DID)

	(1)	(2)	(3)	(4)	(5)
Post*1994-1996 Total Firearm Death Rate	-1.688** (0.675)	-1.544** (0.622)	-1.214** (0.465)	-1.208 (0.741)	-0.412 (0.573)
Implied Effect of NFA at Mean 1994-1996 Total Firearm Death Rate					
NFA	-6.117** (2.445)	-5.595** (2.256)	-4.400** (1.687)	-4.377 (2.684)	-1.384 (1.924)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.869	0.864	0.872	0.877	0.855

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 27: The effect of the NFA on non-firearm suicide rate (DID)

	(1)	(2)	(3)	(4)	(5)
Post*1994-1996 Total Firearm Death Rate	0.934	0.930	0.888	0.602	0.567
	(0.750)	(0.721)	(0.664)	(0.422)	(0.335)
Implied Effect of NFA at Mean 1994-1996 Total Firearm Death Rate					
NFA	3.385	3.369	3.217	2.181	1.905
	(2.717)	(2.614)	(2.407)	(1.531)	(1.125)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.602	0.622	0.628	0.682	0.636

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 28: The effect of the NFA on non-firearm homicide rate (DID)

	(1)	(2)	(3)	(4)	(5)
Post*1994-1996 Total Firearm Death Rate	-0.385	-0.360	-0.366	-0.372	-0.235
	(0.258)	(0.237)	(0.246)	(0.334)	(0.210)
Implied Effect of NFA at Mean 1994-1996 Total Firearm Death Rate					
NFA	-1.394	-1.304	-1.327	-1.350	-0.790
	(0.935)	(0.858)	(0.892)	(1.212)	(0.705)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.828	0.838	0.842	0.847	0.825

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 29: The effect of the NFA on non-firearm accidental and undetermined death rate (DID)

	(1)	(2)	(3)	(4)	(5)
Post*1994-1996 Total Firearm Death Rate	-2.237*	-2.114*	-1.736**	-1.437	-0.744
	(1.108)	(1.046)	(0.722)	(0.935)	(0.706)
Implied Effect of NFA at Mean 1994-1996 Total Firearm Death Rate					
NFA	-8.108*	-7.660*	-6.290**	-5.208	-2.499
	(4.015)	(3.790)	(2.617)	(3.389)	(2.372)
State Fixed Effects	YES	YES	YES	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES
Economic and regional controls	YES	YES	YES	YES	YES
Percent male population	NO	YES	YES	YES	YES
Percent young and old population	NO	NO	YES	YES	YES
More ages	NO	NO	NO	YES	YES
Flexible Time Trend	NO	NO	NO	NO	YES
Sample Size	261	243	243	243	243
R-squared	0.867	0.860	0.871	0.875	0.854

Notes: Heteroscedasticity robust standard errors clustered at the state level are in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

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Vita

Elena Andreyeva was born on November 13, 1988 in Syktyvkar, Komi Republic, Russia. She came to the United States to begin her studies in the fall of 2006. She attended Georgia College and State University in Milledgeville, GA, graduating in May of 2010 with B.S. in Economics.

Elena began her doctoral studies at the Andrew Young School of Policy Studies at Georgia State University in the fall of 2010. She worked as a research assistant for Dr. Carlianne Patrick, with whom she collaborated in studying the capitalization effect of charters schools. She also performed research for Dr. Mark Rider on minimum wages, and worked as a teaching assistant for Dr. Grace O.

During her graduate studies, Elena presented papers on the topics of public policies and regional amenities at the Southern Economic Association's annual meetings. In addition to her research activities, she served as an instructor to undergraduate courses in introductory macroeconomics and international trade as well as a graduate course in development economics. She was also a volunteer with the non-profit program Banking on Our Future. She earned her M.A. in Economics from Georgia State University in May of 2013.

Elena was awarded a Ph.D. in Economics by Georgia State University in summer of 2016. Upon graduation she will begin working as a postdoctoral researcher at the Leonard Davis Institute of Health Economics and the Perelman School of Medicine at the University of Pennsylvania.