Essays on Public Policy and Household Insecurities

By

Kaitlyn M. Sims

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The dissertation is approved by the following members of the Final Oral Committee:
Paul Dower, Assistant Professor, Agricultural and Applied Economics
Bradford Barham, Professor, Agricultural and Applied Economics
Jeremy Foltz, Professor, Agricultural and Applied Economics
Mariel Barnes, Assistant Professor, LaFollette School of Public Affairs
Jenna Nobles, Professor, Sociology, Population Health Sciences, and Obstetrics & Gynecology

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Contents

Introduction

In this dissertation, I ask how public policy does and does not support people at critical inflection points in their lives. Security and safety are necessary conditions for mental, physical, and emotional well-being. Chapters 1 and 2 explore how different forms of domestic insecurity – homelessness, poverty, and family violence – affect the way people live their lives and prepare for the future. The final chapter pivots to instead consider the public health implications of agricultural development and how policymakers might respond to support particularly vulnerable populations.

All of my work is particularly interested in the mechanisms behind behavioral responses to policy, which proves challenging when solely using the tools of economics. Although I am trained as an economist, my work lies in several interdisciplinary junctures. While I try to maintain humility about my own relative expertise in these spaces, I bring my training as an economist in causal inference and population-level data to these difficult questions. My scholarship is informed by knowledge and frameworks across psychology, demography, and sociology, especially as they relate to trauma and security.

In the first essay, I examine the effects of multidimensional property rights on investment in children's education. Homelessness, specifically doubling-up, has been a major concern on Chile for decades. The Chilean subsidized housing program offers access to homes and mortgages at low rates, but full housing rights are conditional on living in the home for five years. I take advantage of this conditional housing policy to disentangle the effects of housing use rights (the ability to live in the home) and transfer rights (the ability to sell the home) on investment in children's education. I develop a theoretical framework for understanding how housing transfer rights are distinct from the security effects offered by use rights.

I find that there are indeed distinct effects of housing use rights and transfer rights, with both increasing educational outcomes for children of beneficiaries. However, the effects of use rights are more salient for finishing secondary school, while transfer rights affect university education, likely reflecting the high financial costs of attending university in Chile. While this study considers the context of public housing in Chile, it has implications for housing and education policy globally. As urbanization increases across the globe, my research provides necessary understanding of the different dimensions of property rights in an urban context. Further, policymakers in urban contexts often turn toward rent assistance (such as the United States Section 8 housing voucher) to address housing insecurity for families in poverty. My findings in this paper suggest that such rent assistance programs – which offer use rights without the possibility of transfer rights – should improve high school graduation rates, but will not have effects on costly university attendance.

In the second essay, I consider an alternative public housing policy type – emergency shelter for victims of domestic violence (DV). Victims of domestic and intimate partner violence (IPV) frequently identify the need for safe and secure housing as one of their most pressing concerns, leading to a (sometimes) robust network of services across the country aiming to support victims' housing needs. Emergency shelters, which provide short term (usually 14-90 days) housing for individuals fleeing violence, are a popular type of support services for victims, yet their efficacy remains under-evaluated in light of insufficient data. Further, our ability to speak to the mechanisms by which emergency shelters may affect broader patterns of violence is limited by the current game theoretic literature on domestic violence.

In this chapter, I contribute to this literature through the collection of such a database and creation of such a theoretical framework. The theoretical framework underscores how complicated the causal links between increased shelter capacity and DV/IPV homicide are, and I argue that this ambiguous relationship in the aggregate is likely a function of community- and individual-level characteristics. I find a precisely estimated zero effect of changes to shelter capacity on both DV and IPV homicide, and this relationship is persistent across a series of robustness tests and alternative specifications. Informed by the model, I interpret this as suggestive that on the aggregate, shelter capacity limitations are not the binding constraint affecting homicide rates. Instead, it is possible that shelters are already efficiently allocating scarce bed spaces to those at the highest risk for homicide.

In the third essay, co-authored with Dr. Marin Elisabeth Skidmore and Dr. Holly K. Gibbs, we investigate indirect health effects of agricultural development in the Brazilian Amazon and Cerrado. Brazil has rapidly become the world's leading soy producer in recent decades. There has been much research on the effects of extensification of soy production (increased area under cultivation) but less on the effects of intensification (use of inputs like agrotoxins), despite the identified link between pesticide exposure and carcinogenesis. We estimate the effects of the expansion of soy production – and related community exposure to pesticides – on childhood cancer incidence using 15 years of publicly available data on disease mortality. We focus on acute lymphoblastic leukemia (ALL), a relatively rare blood-borne cancer that develops in young children. We find a statistically significant and positive effect of soy production, both in terms of the percentage of area in soy

and tons of soy produced, on pediatric ALL. These effects are larger when we consider soy production in the entire Ottobasin, demonstrating the effects of agrotoxin exposure via water supply. Our results are robust to a series of different empirical specifications. We consider these health effects in the context of public health policy and infrastructure in Brazil. This work speaks to the need for stronger regulation of agrotoxins as well as increased public health attention to cope with exposure in the broader community.

Chapter 1

Turning a house into a home: Delayed property rights and education investment decisions in urban Chile

Abstract

Housing insecurity and homelessness is a multi-faceted policy issue encompassing concerns over emotional and physical security, financial liquidity, and formal state recognition. However, most research on housing titling programs focuses either on housing rights in general or on a single element of these rights. I distinguish between the effects of property use and transfer rights on children's educational outcomes. I utilize a specific feature of government-subsidized housing programs in Chile that bars recipients from selling their home for five years. This allows me to separately identify the effects of use rights and transfer rights for the children of housing recipients. I develop a theoretical framework to understand the difference between the effects use and transfer rights for secondary versus university education. I then use nationally-representative household survey data from 1996-2009 to test the implications of the theoretical framework. I find that housing use rights lead to a 6 percentage point increase in the probability of finishing 12th grade, while use and transfer rights correspond with an up to 5 percentage point increase in the probability of finishing university. Housing security effects (via use rights) directly affect the probability of attending and finishing high school, while housing financial effects (via transfer rights) increase the probability of attending and finishing university. These results underscore the importance of housing security and financial security on children's educational outcomes.

1.1 Introduction

Subsidized housing programs are a popular policy tool to address housing insecurity, homelessness, and crowding across the globe and particularly in Latin America, with major housing reforms taking place in Brazil, Argentina, and Mexico, among others. The details of individual reforms vary, but traditionally governments offer some combination of access to credit, mortgages at government-set interest rates, and legal recognition. Beyond simply providing access to safe and secure housing, subsidized public housing programs are a first step toward overcoming the disparities posed by generational wealth and the historical, systemic discrimination faced by traditionally marginalized communities.

Many of these programs offer title to households that previously lacked formal title or offer a bundle of subsidized housing with title. As such, households are conferred some mix of *use rights* and *transfer rights* once enrolled the program. For the purposes of this paper, I define “use rights” as those rights which are conferred with the home. In the case at hand, use rights provide security in place and security from seizure. Conversely, “transfer rights” consist of the legal right to sell the home. These can be thought of financial rights and include the right to leverage house as an asset, thereby constituting a wealth effect for the household. In this context, use rights are implied by transfer rights, but not vice versa. Despite the popularity of these programs, the distinct effects of these two types of property rights remain unidentified.

To distinguish between the effects of property use rights versus property transfer rights, I exploit a Chilean prohibition on selling homes purchased using a government subsidy for the first five years of ownership. This conditional subsidy allows me to separately identify the effects of these different dimensions of home ownership: after the home is received and once the home can be sold. I examine the effects of these two dimensions of conditional property rights on investment in children’s education. To do so, I address the following questions. First, do households that received housing invest in children’s education more than households that have not received such housing? Do households that received homes with government subsidies invest less in children’s education when they have only use rights than when they have use *and* transfer rights? Do these effects continue on into investment in post-secondary education?

Property rights may have significant effects on children’s education because investment in education requires both financial security (transfer rights) and housing security (use rights). I hypothesize that housing title could affect education *specifically* through the following channels. First, use rights improve security for the household. This could mean increased state visibility and access to state-provided services as well as tying a household to a location (Alston et al., 1996; Field, 2005). Use rights may also lead to a change in future discounting or different investment decisions. This increased security may make future periods more salient as the household may have a reasonable expectation of what the future will look like for the next five years. Moving to a government-subsidized home may also (temporarily or not) reduce the size of beneficiaries' social networks. Second, transfer rights provide a shock to wealth (Alston et al., 1996; Banerjee et al., 2002; Carter and Olinto, 2003) via the value of a transferable asset (the home), increased land value, and access to credit markets. Education is costly, both directly (e.g. tuition, books, or bus fare) and indirectly (e.g. opportunity cost from lost wages, child care). It also requires an element of stability in place. Furthermore, educational outcomes are not the focus of housing policy. If there is an effect of housing policy on educational outcomes, this would suggest that policymakers should consider the unintended consequences of seemingly unrelated policy arenas. I develop a theoretical model to understand the distinct roles played by use rights and transfer rights in education decisions, differentiating between secondary school and university education.

I test the predictions of the model using data from six waves the Chilean National Socioeconomic Survey (CASEN) covering 1996-2009. This allows me to conduct analysis on both cross-section and panel data, as well as imputing a pseudo-panel. I test the effects of use and transfer rights on total educational attainment, educational attainment by age 18, and probability of dropout. I use a binned difference-in-differences empirical strategy where treatment is defined by the age that a school-aged child's household received housing.

In accordance with the predictions of the theoretical framework, I find that there is an effect of property rights on educational investment, both in levels and on the margin (attendance).Further, there are distinct effects for children who received only use rights while in school compared to peers who received both use and transfer rights. Having use rights by age 14 (the time a child enters secondary school) increase the probability of completing 12 years of schooling by 6-7 percentage points, while use rights in combination with transfer rights increase the probability of completing university by 2-4 percentage points. The effect of transfer rights is particularly significant for university attendance, suggesting that there is some channel to university opened by transfer rights that is not accessible via use rights alone. I show that these results are robust to a series of data restrictions and explore heterogeneity by family fertility and across crucial "pivot" years like entry to 9th grade and university.

The findings in this paper have direct policy implications. I show that housing use rights and transfer rights affect children's long-term educational investments for recipients of subsidized housing, and that these effects are different for secondary versus university education. These unintended consequences of housing policy underscore the importance of holistic approaches to addressing poverty. Families may not reap the full gains of education policy without the security and financial effects of property rights. Further, these findings have generalizability to housing programs that don't offer ownership. I show that use rights have specific effects distinct from transfer rights. Therefore, rent assistance programs that offer the same benefits as use rights in Chile may have effects on secondary education, but we would not expect to see those effects on costly higher education.

Further, much of the research on secure property rights focuses on asset transferability (transfer rights) or the ability to generate income on securely protected lands (use rights). In these cases, property rights are ultimately *land* rights, rather than rights over housing. In the context of study in this paper, it is much less likely that a household uses their subsidized property as a way of generating income (at least, not in the same way as a rural household of subsistence farmers would). As a result, this findings in this paper speak directly to the challenges and constraints faced by *urban* households, where property use rights are specifically over housing and related security concerns. As urbanization continues to expand, especially in contexts with rapid population growth rates, property rights concerns among the urban poor will become increasingly important.

While I exploit the Chilean housing sunset clause for empirical identification purposes, studying these types of conditional housing programs or property rights is of particular importance and and of itself. There is a long history of conditional housing or aid, especially as given to vulnerable populations such as indigenous communities (Anderson and Parker, 2009; Leonard et al., 2020). The context I study is directly applicable to subsidized housing programs elsewhere in the world that target multiply-marginalized populations, such as households in poverty, people of color, and survivors of domestic violence.

The remaining sections of the paper will proceed as follows. Section 1.2 outlines related literature, and in section 1.3, I describe the historical and policy context I look at in this paper. In section 1.4, I use this context to motivate a theoretical framework for understanding the distinct effects of use rights and transfer rights. This includes a model that explores the effects of multidimensional property rights on education. I then describe the data and methodology I use in this paper as well as challenges to identification in section 1.5. Results are presented in section 1.6, and section 1.7 concludes by discussing policy implications and potential future streams of research.

1.2 Related literature

This paper contributes to three main strands of literature: first, the literature on educational investments, especially for households in poverty; second, the broad literature on housing insecurity and secure property rights; and third, the much smaller literature exploring the relationship between housing, particularly subsidized programs, and children's educational outcomes.

There is a broad body of literature on the importance of education, particularly for individuals in poverty (Barro, 2001; Mincer, 1974). Policymakers and researchers are particularly concerned with how to incentivize poor households to invest in their children's education. There is a well-developed body of evidence, both experimental and non-experimental, showing that student attendance and performance can be improved via peer effects, financial incentives, and anticipated returns (Ganimian and Murnane, 2014; Jacob and Ludwig, 2008; Kremer, M., Moulin, S., & Namunyu, 2003; Kremer and Holla, 2009; Schultz, 2004; Shah and Steinberg, 2017; Valente, 2014).

Housing insecurity and homelessness is a widespread issue, though the specifics of what "homelessness" means varies widely by context. I contribute to this interdisciplinary literature by developing a theoretical framework that distinguishes between the consequences of housing security as it relates to security in place versus the financial elements. More specifically, this paper contributes to the literature on urban land titling by exploring the consequences of an urban housing program on beneficiaries' children. There has been a good amount of work done on the effects of transferable property rights on access to credit markets and household investment (including but not limited to Field (2005), Field and Torero (2008), Lanjouw and Levy (1998), and Piza and de Moura (2016)). Galiani and Schargrodsky (2010) show that titling for urban squatters in Argentina led to increased investment in property and human capital, but not via access to credit markets. Instead, they posit that the increased educational investment comes from a reduction in fertility post-entitlement alongside a reduction in presence of extended family members, allowing parents to invest more in each child. I build on this literature by exploring the effect of a program with an automatic sunset clause. Additionally, this work considers the indirect effects of property title by exploring educational effects for children in recently-received homes.

The property rights literature is deep, but the literature on multi-dimensional property rights and urban land title is somewhat thinner. There has been some work that has attempted to un-bundle property rights into its component elements, including security effects, collateralization, income rights, and transfer rights (Besley, 1995; Brasselle et al., 2002; Czeglédi, 2015; Liu et al., 1998; Markussen et al., 2011). This paper contributes to this literature by defining and empirically distinguishing between two groups of benefits: those that come with the legal right to sell the home and those that come with the home itself. These distinctions will be more relevant to an urban context than those provided by papers like Liu et al. (1998). Additionally, this paper is the first (that I know of) to empirically identify the effects of use rights versus transfer rights, especially as it relates to educational investments.

There is mixed evidence as to the effects of subsidized housing on childhood outcomes, and most of this evidence comes from analysis of voucher programs the United States (DeLuca and Dayton, 2009; Jacob et al., 2015; Kucheva, 2018; Mills et al., 2006). Some work has found positive effects of housing subsidies on educational outcomes in Chile (Dumas, 2007; Kast et al., 2009). I expand on this work in four ways. First, I use the CASEN panel to look at contemporaneous effects of title status changes on education investment. Second, I compare households which have received full housing title (use *and* transfer rights) in the past versus more recently, rather than comparing applicants to recipients. Third, I build a theoretical framework and model for understanding the mechanisms at play in affecting household childhood investment. Most importantly, I draw a distinction between the type of housing title that a family has (use rights vs. use *and* transfer rights).

1.3 Context

1.3.1 Housing policy

Housing shortages and housing insecurity in Chile have been a major concern since the early 20th century. This housing insecurity arose from limited supply of housing, inflation, and high poverty rates, all of which prevented households from securing safe and hygienic housing (Jirón, 2004; de Freitas et al., 2013). Mass migration into Santiago further strained the housing market, and to this day the Santiago Metropolitan Region remains the most populous part of the country, with one in three Chileans living in Greater Santiago (Cummings and Dipasquale, 1997).

In the second half of the 20th century, policymakers were specifically concerned with the prevalence of *allegados* and overcrowding. *Allegado*, meaning "close" or "drop-in," is a term used to describe poor individuals or entire households who live with non-nuclear family members or other close individuals (often referred to as "doubling-up" in the United States). *Allegados* tend to be constrained by the housing supply and/or income and often live in spare rooms or additions to the home or yard, resulting in overcrowding (Cummings and Dipasquale, 1997). Historically, overcrowding was exacerbated by large-scale migration to Chile's three largest cities – Santiago, Valparaíso, and Concepción – during the mid-twentieth century (Long (2016)]). Between 1930 and 1960, the proportion of the country living in urban areas grew from 48% to 61% (Valenzuela, 2008). Without sufficient housing stock, the problem has only persisted over time. In 1990, the government estimated that 42% of households were living as *allegados*.

Property ownership and overcrowding were not the only housing challenges faced by the Chilean government. Policy priorities also include increasing access to water, electricity, and sewage systems, as well as reducing the number of people living in informal and often crowded *campamentos*. Illegal land seizures, known as *tomas*, posed an additional challenge, though these seizures ceased during the 1980s (Jirón, 2004; Pérez Ahumada, 2016).

The first Chilean housing agency, the *Caja de Habitación Popular*, or *Caja*, was established in 1936 to address the growing housing crisis (Valenzuela, 2008). Between 1936 and 1942, the *Caja* built over 43,000 housing units. Later programs tested the limits of this early policy design. *Operación Sitio*, was established in 1965, the same year as the establishment of the ministry for housing an urbanism, known as *Ministerio de Vivienda and Urbanismo* (MINVU). *Operación Sitio* was commonly known as "Operation Chalk" because sometimes officials would literally outline property lines in chalk for beneficiaries (Long (2016)). The program was haphazard at best, with some beneficiaries receiving housing with plumbing and others receiving an undeveloped patch of land without utilities.

On September 11, 1973, a military coup unseated Marxist President Salvador Allende and installed Augusto Pinochet in a military dictatorship that would last until 1989. During the military dictatorship, housing policy took a markedly neoliberal turn focused on private ownership of housing and utilities. Under the advising of the "Chicago Boys," programs focused on "proper" home ownership (Murphy, 2015). The government hit back against illegal settlements and institutionalized socioeconomic segregation by forcing poor households to the periphery of Santiago (Richards, 1995). Public housing agencies were regionalized, and public ownership of housing production was dismantled (Kuznetzoff, 1987). These policies increased the prevalence of *allegados* (Richards, 1995).

Chilean housing policy consists of financing (mortgages), subsidies, and regulation in the housing construction sector. Home buyers are able to pay for their homes using a combination of 8-30 year mortgages, government subsidies, and using own savings (de Freitas et al., 2013). Because this subsidy program often involves households paying a subsidized mortgage, this program is distinct from property rights contexts where the asset is owned outright, as is often seen in parts of the economic development literature on property rights. However, housing can indeed be transferred (once transfer rights are conferred) as is the case in other political and financial contexts.

In this paper, I focus primarily on the subsidy program as it contains the conditionality that provides the quasi-experimental variation of interest. The modern housing subsidy program began in the 1970s and expanded in 1984 (de Freitas et al., 2013). Unlike pre-existing supply-side subsidy programs, the Chilean model focused on *demand*-side subsidies (Jirón, 2004). Beneficiaries are able to use the subsidy to (either fully or in part) purchase privately constructed homes, rather than moving to housing specially constructed for this purpose. This subsidy program laid the foundation for the subsidized housing market that exists to this day. The greatest difference has been the increase in government spending in the years since re-democratization (Ozler, 2012). According to de Freitas et al. (2013), 333,000 families received subsidies in the 1980s, and 515,000 families received subsidies in the 1990s, which demonstrates the massive scope of this program relative to the country's total population.

There are three main types of subsidies[1]: the basic housing program (*Vivienda Básicas*); subsidy certificates (*Sistema General Unificado*); and sites-and-services (*Programa de Vivienda Progresiva*). This paper focuses on the first two. The basic housing program is intended for the poorest households with an immediate housing need. The subsidy certificates are intended for middle-income families, and the size of the subsidy depends on the house price, with smaller homes receiving larger subsidies. In this paper, I do not distinguish between the two housing programs due to data limitations.

Beginning in 1984, the subsidy program began to operate using an application system that placed applicants in a "line" to receive benefits. The program has a long waiting period, with many applicants waiting up to ten years to receive a home (Dumas, 2007; Gilbert, 2002).

1.3.2 Chilean education system

During the period I study (post-1973 military coup and pre-2011 educational reforms), Chilean education consisted of parallel public and private schooling at the primary, secondary, and post-secondary levels (Bellei, 2009). Families were able to use subsidized school vouchers to attend the school of their choice, in the hopes that schools would be incentivized to compete for student enrollment, thus creating a "self-regulating" market (Mizala and Romaguera, 2000). In practice, private schooling (at all levels) became costly, with public schooling (at the primary and secondary level) remaining free or adopting marginal enrollment costs (Matear, 2007). Certain private schools remained non-subsidized, therefore ineligible to enroll students using subsidies, but these schools enrolled less than 10% of the student population in the 1990s-2000s (Bellei, 2009).

At the primary and secondary levels, there are substantial differences in educational quality between the public and private sector, with public schools seen as "shabby" and under-funded (Nadworny et al., 2019; Quaresma, 2017). As of 2000, only 15% of university students came from public secondary schools (Mizala and Romaguera, 2000). Both subsidized and non-subsidized private schools may impose admission

[1]The subsidy programs are detailed thoroughly in Appendix A of Richards (1995) as well as the main text of de Freitas et al. (2013).

criteria that further entrench socio-economic inequality (Matear, 2007). In this paper, I abstract away from questions of education quality and instead consider quantity (the choice to enroll). However, future work should address whether the perceived benefit of education, which is likely a function of education quality, affects the decision to enroll conditional on subsidy timing.

At the university level, there are high quality institutions in both the public and private sector (Mizala and Romaguera, 2000). However, high tuition at public universities and the resulting educational inequality became a major focus of public outrage in 2011 (Nadworny et al., 2019). Prior to 2011, education costs were substantial enough in Chile to require students to take on debt to pay for tuition and related costs.[2]

1.4 Theoretical Model

Next, I describe a theoretical model for understanding the distinct roles of different dimensions of property rights on educational investment, as well as the propositions I draw from this model.

Consider a household consisting of a parent and a child. The household is utility-maximizing over lifetime income. This utility is a function of consumption, c_t, subject to a discount factor, $\beta \in (0,1)$. The parent works for an income $\omega_t \sim U[0,\omega]$, with expected wage denoted $\underline{\omega}$ for brevity. The child can either attend school or earn a wage. The child's wage is an increasing function of the amount of schooling the child has completed up to that point.The child attends school in period t if the expected lifetime utility from attending school in period t is greater than the lifetime expected utility from working during period t. Attendance in secondary school or university is conditional on attending primary school. To simplify this, I assume that dropout is permanent and students are not able to re-enter schooling once they exit. The expected lifetime utility from attending school today must only be greater than the expected lifetime utility from never attending school in any future period (all future income is a function of the level of education already received).

In the baseline case, there is no saving or borrowing technology, so the household consumes its income. I first assume that education is costless in all periods. There are five periods, denoted $t \in \{0,1,2,3,4\}$. In period 0, the child is in infancy and only able to consume. In periods 1, 2, and 3, the child can either go to school or work for a wage equal to their human capital, e_t. In period 4, lifetime earnings are realized. Period 1 can be conceived of as primary school, 2 as secondary school, and 3 as university. The schooling decision

[2]Educational reforms beginning in 2011 reduced the cost of university tuition, making public university tuition free for households in the bottom 50% of income and capping tuition at private universities (Nadworny et al., 2019). For this reason, I omit survey data after 2009 from my analysis.

is denoted by $s_t \in \{0, 1\}$.[3] The household therefore maximizes

$$\max_{s_1,s_2,s_3 \in \{0,1\}} u_0(c_0) + \beta u_1(c_1) + \beta^2 u_2(c_2) + \beta^3 u_3(c_3) + \beta^4 V(e_4) \tag{1.1}$$

where $V(e_4)$ is the household's total payoff after all possible human capital is accumulated. For brevity and without loss of generality, I assume $V(e_4) = e_4$ throughout the paper. I assume that utility is strictly increasing in consumption, but the marginal returns to consumption are decreasing. Consumption is a function of schooling, human capital, and earned wages. Human capital is an increasing function of lagged consumption, lagged human capital, and education investment.[4]

I first consider the case where there is only primary and secondary school available. In period 3, the child can only work for a wage $e_3 = f_3(c_2, e_2, s_2)$.[5] There are three possible states of the world, denoted where necessary in subscripts in both the utility and human capital functions.[6] The child can go to school in both periods (denoted $U_{\{1,1\}}$), in only the first period ($U_{\{1,0\}}$), or never ($U_{\{0,0\}}$). [7] I compare the utility in each state of the world to find the necessary conditions for the household to prefer more schooling to less. The household will prefer to receive two periods of education over zero periods of education when:

$$\beta^2[e_{3,\{1,1\}} - e_{3,\{0,0\}}] > [u_1(\underline{\omega} + e_1) - u_1(\underline{\omega})] + \beta[u_2(\underline{\omega} + e_{2,\{0,0\}}) - u_2(\underline{\omega})]. \tag{1.3}$$

[3]Unlike Shah and Steinberg (2017), I consider the schooling choice to be binary as the CASEN survey does not include information on the amount of schooling a student receives or the quality of learning.

[4]Consumption follows

$$c_0 = \underline{\omega}$$
$$c_{t>0} = \underline{\omega} + (1 - s_t)e_t.$$

Human capital follows:

$$e_0 = 0$$
$$e_1 = f_1(c_0) = f_1(\underline{\omega})$$
$$e_{t>1} = f_t(c_{t-1}, e_{t-1}, s_{t-1}).$$

[5]Equation 1.1 simplifies to

$$\max_{s_1,s_2 \in \{0,1\}} u_1(c_1) + \beta u_2(c_2) + \beta^2 e_3. \tag{1.2}$$

[6]E.g. if a person attends in period 1 but not 2, they will maximize $U_{\{1,0\}}$ and have second-period human capital $e_{2,\{1,0\}}$. Because $e_1 = f(c_0)$, $e_{1,\{1,1\}} = e_{1,\{1,0\}} = e_{1,\{0,0\}}$.

[7]I rewrite the utility function for each possible state of the world:

$$U_{\{1,1\}} = u_1(\underline{\omega}) + \beta u_2(\underline{\omega}) + \beta^2 e_{3,\{1,1\}}$$
$$U_{\{1,0\}} = u_1(\underline{\omega}) + \beta u_2(\underline{\omega} + e_{2,\{1,0\}}) + \beta^2 e_{3,\{1,0\}}$$
$$U_{\{0,0\}} = u_1(\underline{\omega} + e_1) + \beta u_2(\underline{\omega} + e_{2,\{0,0\}}) + \beta^2 e_{3,\{0,0\}}$$

Note that e_2 is a function of e_1 and s_1. Therefore the period 3 functions are dependent on the education decision in period 1.

The household will prefer two periods of education to education only in period 1 when:

$$\beta^2[e_{3,\{1,1\}} - e_{3,\{0,0\}}] > \beta[u_2(\underline{\omega} + e_{2,\{1,0\}}) - u_2(\underline{\omega})]. \tag{1.4}$$

The household will prefer to send the child to school in both periods so long as the lifetime discounted utility premium to education is greater than the sum of the child's wage premium earned in periods 1 and 2 (discounted accordingly).

1.4.1 Secondary education: housing use rights *only*

Next, I introduce a government-subsidized housing program. The household has applied to the housing program before period 0, and they can receive housing in periods 0, 1, or 2. At the time housing is received and use rights are conferred, the household experiences a positive shock to their security. This can be thought of as reducing the disutility from housing insecurity, which may come in myriad forms. As one example, when the household lives with relatives, it is possible that they might have to vacate the home suddenly. This could force the family to relocate and therefore the parent to lose their job. Housing security reduces the probability of these zero-income, worst case scenarios by providing security in place. [8]

In this model, I operationalize this decrease in uncertainty as a change to the distribution of parental wages once the home is received, such that $\omega_t \sim U[\omega_0, \omega]$. The new expected wage is $\frac{\omega_0+\omega}{2}$, denoted $\bar{\omega}$ for brevity. If the household receives the home in period 0, they will prefer two periods of education to none if

$$\beta^2[e_{3,\{1,1\}} - e_{3,\{0,0\}}] > [u_1(\bar{\omega} + e_1) - u_1(\bar{\omega})] + \beta[u_2(\bar{\omega} + e_{2,\{0,0\}}) - u_2(\bar{\omega})]. \tag{1.5}$$

Because utility has decreasing returns to scale, the lower bound for a household to prefer schooling *with* housing (the right-hand side of (1.5)) is lower than in the baseline case (without housing). All else constant, after a household receives housing they prefer to invest in education more so than they would have without housing. It follows that if housing use rights are received in *any* period, the necessary lifetime expected utility for a household to prefer more schooling to less is lower than in the baseline case. In other words, the household is able to invest more in education because there is less of a wage premium to working today.

Hypothesis 1 (P1): *Use rights will increase school attendance.*

[8] The examples given above are particularly salient given the case at hand, however they are not the only ways to operationalize and parameterize housing insecurity. Drawing from the psychology literature, housing insecurity and related stress may decrease overall productivity, reducing income even in non-zero income situations. More simply, security in place may reduce household stress about the potential for moving, thereby increasing utility directly. These parameterizations of the effect of housing insecurity on utility are not substantively different, meaning the results of the model can generalize widely.

1.4.2 University education

Next, I consider the case where the household can invest in university education in period 3. The child can only attend university if they have invested in education in periods 1 and 2.[9] University education costs λ.[10] Without a borrowing or savings technology, the child can only go to university if the parent's endowment in period 3 is greater than the cost of education and some baseline level of consumption, $\underline{c}$. The household will only choose to invest in university education if lifetime expected utility from attending school in all three periods is greater than from attending only in period 1, period 1 and 2, and in no periods. The household will prefer three periods of education to no periods of education when:

$$\begin{aligned}\beta^3[e_{4,\{1,1,1\}} - e_{4,\{0,0,0\}}] >& [u_1(\underline{\omega} + e_{1,\{0,0,0\}}) - u_1(\underline{\omega})] + \beta[u_2(\underline{\omega} + e_{2,\{0,0,0\}}) - u_2(\underline{\omega})] \\ &+ \beta^2[u_3(\underline{\omega} + e_{3,\{0,0,0\}}) - u_3(\underline{\omega} - \lambda)]. \end{aligned} \tag{1.7}$$

In other words, the household will only invest in university education if the lifetime utility premium to university is greater than the utility premium to working in up to three periods plus university costs, λ. The lower bound on necessary lifetime expected utility to attend university is *increasing* in the number of periods worked. Because of this, I compare the cases of attending school in all three periods ({1,1,1}) to working for three periods ({0,0,0}) for the remainder of this subsection.

Housing use rights *only*

I begin by considering the case where the household is endowed with use rights at the start of the game. The household will prefer two periods of education to no periods of education when:

$$\begin{aligned}\beta^3[e_{4,\{1,1,1\}} - e_{4,\{1,1,0\}}] >& [u_1(\bar{\omega} + e_{1,\{0,0,0\}}) - u_1(\bar{\omega})] + \beta[u_2(\bar{\omega} + e_{2,\{0,0,0\}}) - u_2(\bar{\omega})] \\ &+ \beta^2[u_3(\bar{\omega} + e_{3,\{0,0,0\}}) - u_3(\bar{\omega} - \lambda)]. \end{aligned} \tag{1.8}$$

[9] The household maximizes according to

$$\max_{s_1,s_2,s_3\in\{0,1\}} u_1(c_1) + \beta u_2(c_2) + \beta^2 u_3(c_3) + \beta^3 e_4 \tag{1.6}$$

[10] The cost of university is generalized here, though university costs have varied considerably in Chile during the period of study. It is important to remember that university costs in Chile, especially around the turn of the millennium, were the source of acute social strife and nation-wide protests (Long, 2011; Nadworny et al., 2019). While the specifics of what this cost entails are different over time and contexts, they can be conceived of more generally as including opportunity costs from lost wages for the university student or costs of books and supplies.

Because of the diminishing marginal utility of consumption, we again see that the lower bound of the lifetime earnings premium is lower with housing use rights in (1.8) than without use rights in (1.7).

However, the household can receive housing use rights in period 0, 1, or 2. Next, I consider the cases when housing is received *after* the game has started. I denote the period housing is received via superscripts to the human capital term (e.g. e_t if housing is received in period 0, e'_t if in period 1, e''_t if in period 2, and e'''_t if in period 3.) Note that for all periods t, $e_t \geq e'_t \geq e''_t \geq e'''_t$. If the household receives housing in period 1, it maximizes the following:

$$\max_{s_1,s_2,s_3\in\{0,1\}} u_1(\bar{\omega} + (1-s_1)e'_1) + \beta u_2(\bar{\omega} + (1-s_2)e'_2) + \beta^2 u_3(\bar{\omega} + (1-s_3)e'_3) + \beta^3 e'_4. \tag{1.9}$$

The only difference between this and 1.8 comes from the nested effect of initial human capital accumulation, as $e'_1 = f(c_0) = f(\underline{\omega})$, while $e_1 = f(\bar{\omega})$.

Hypothesis 2 (P2): *Households that received housing use rights before the students were in school will attend university more than those who received housing use rights while students were already in school.*

If the household receives housing use rights in period 2 it prefers to invest in university education when:

$$\begin{aligned}\beta^3[e''_{4,\{1,1,1\}} - e''_{4,\{0,0,0\}}] >& [u_1(\underline{\omega} + e''_{1,\{0,0,0\}}) - u_1(\underline{\omega})] + \beta[u_2(\bar{\omega} + e''_{2,\{0,0,0\}}) - u_2(\bar{\omega})] \\ &+ \beta^2[u_3(\bar{\omega} + e''_{3,\{0,0,0\}}) - u_3(\bar{\omega} - \lambda)]. \end{aligned}\tag{1.10}$$

Observe that the lower bound in (1.10) is greater than that in (1.8). Finally, families who receive housing use rights in period 3 will maximize according to the following:

$$\max_{s_1,s_2,s_3\in\{0,1\}} u_1(\underline{\omega} + (1-s_1)e'''_1) + \beta u_2(\underline{\omega} + (1-s_2)e'''_2) + \beta^2 u_3(\bar{\omega} + (1-s_3)e'''_3) + \beta^3 e'''_4(c_3, e_3, s_3). \tag{1.11}$$

The child can only attend university if endowed income is greater than the lower bound for consumption plus tuition. Increased income will make this constraint less binding, but less so than in the case where housing use rights are endowed before the child would attend university.

Hypothesis 3 (P3): *University attendance will be lower for students whose households received housing use rights during secondary school or college years than for students who received use rights earlier.*

Transfer rights *only*

In the period *after* housing is received, the household receives transfer rights to the home, allowing the household to sell the home. This is modeled as providing access to a borrowing and saving technology. The borrowing and saving technology allows a household to save in each period once transfer rights are obtained. The household will only save if they intend to send the child to college. College has a nontrivial cost λ. I assume that there is no interest earned on savings, so without loss of generality I assume that the household will save λ/n each period they can save, where n is the number of periods that the household has transfer rights. Further, the household can only send the child to college if the child has completed primary and secondary education ($s_1 = s_2 = 1$).

First, I consider the case where *only* transfer rights have an effect. If the household receives transfer rights in period 0, they will prefer to send the child to college if

$$\begin{aligned}\beta_4[e_{4,\{1,1,1\}} - e_{4,\{1,1,0\}}] >& [u_0(\underline{\omega}) - u_0(\underline{\omega} - \lambda/4)] + \beta[u_1(\underline{\omega}) - u_1(\underline{\omega} - \lambda/4)] \\ &+ \beta^2[u_2(\underline{\omega}) - u_2(\underline{\omega} - \lambda/4)] + \beta^3[u_3(\underline{\omega} + e_{3,\{1,1,0\}}) - u_3(\underline{\omega} - \lambda/4)]\end{aligned} \tag{1.12}$$

Observe that the right-hand side condition is *less* than the right-hand side condition in the baseline case due to decreasing marginal returns to consumption. The household can only save up to its endowment less c_0 in a given period. Because the household can only save in those periods after transfer rights have been conferred, if the household receives use rights in period 1, then it can only save for college in period 2. Therefore, the more periods the household can save over, the more likely that the household can save λ/n.

Hypothesis 4 (P4): *Transfer rights will increase investment in university education, conditional on having attended school in periods 1 and 2. University attendance will increase with the number of pre-college periods where the household has transfer rights.*

Use *and* transfer rights

Finally, I consider the case where both use *and* transfer rights are conferred with the home. The household receives transfer rights in the period *after* it receives use rights. If use rights are conferred in period 0, the household will prefer to send the child to college over receiving no education if

$$\begin{aligned}\beta^3[e_{4,\{1,1,1\}} - e_{4,\{0,0,0\}}] >&[u_1(\bar{\omega} + e_{1,\{0,0,0\}}) - u_1(\bar{\omega} - \lambda/3)] + \beta[u_2(\bar{\omega} + e_{2,\{0,0,0\}}) - u_2(\bar{\omega} - \lambda/3)] \\ &+ \beta^2[u_3(\bar{\omega} + e_{3,\{0,0,0\}}) - (\bar{\omega} - \lambda/3]. \qquad (1.13)\end{aligned}$$

Similarly, if use rights are conferred in period 1, the household will prefer to send the child to college over receiving no education if

$$\begin{aligned}\beta^3[e_{4,\{1,1,1\}} - e_{4,\{0,0,0\}}] >&[u_1(\bar{\omega} + e'_{1,\{0,0,0\}}) - u_1(\bar{\omega})] + \beta[u_2(\bar{\omega} + e'_{2,\{0,0,0\}}) - u_2(\bar{\omega} - \lambda/2)] \\ &+ \beta^2[u_3(\bar{\omega} + e'_{3,\{0,0,0\}}) - (\bar{\omega} - \lambda/2]. \qquad (1.14)\end{aligned}$$

Next, I compare the lower bound if use rights are conferred in period 0 to that if use rights are conferred in period 1. The lower bound in the case where use rights are conferred in period 0 will be *lower* than if use rights are conferred in period 1 if

$$\begin{aligned}&[u_1(\bar{\omega} + e'_{1,\{0,0,0\}}) - u_1(\bar{\omega})] + \beta[u_2(\bar{\omega} + e'_{2,\{0,0,0\}}) - u_2(\bar{\omega} - \lambda/2)] + \beta^2[u_3(\bar{\omega} + e'_{3,\{0,0,0\}}) - u_3(\bar{\omega} - \lambda/2)] \\ &\quad > [u_1(\bar{\omega} + e_{1,\{0,0,0\}}) - u_1(\bar{\omega} - \lambda/3)] + \beta[u_2(\bar{\omega} + e_{2,\{0,0,0\}}) - u_2(\bar{\omega} - \lambda/3)] + \beta^2[u_3(\bar{\omega} + e_{3,\{0,0,0\}}) - u_3(\bar{\omega} - \lambda/3)].\end{aligned}$$

If use rights are conferred in period 2, then the family only has transfer rights in period 3, and is therefore unable to save for university costs. This is equivalent to the case of only use rights conferred in period 2.

The lower bound for lifetime expected utility gain if use rights are conferred in period 1 is lower than if conferred in period 2 (conditional on attending in all three periods) if[11]:

$$\begin{aligned}&[u_1(\underline{\omega} + e''_{1,\{0,0,0\}}) - u_1(\underline{\omega})] + \beta[u_2(\bar{\omega} + e''_{2,\{0,0,0\}}) - u_2(\bar{\omega})] + \beta^2[u_3(\bar{\omega} + e''_{3,\{0,0,0\}}) - u_3(\bar{\omega} - \lambda)] \\ &\quad > [u_1(\bar{\omega} + e'_{1,\{0,0,0\}}) - u_1(\bar{\omega})] + \beta[u_2(\bar{\omega} + e'_{2,\{0,0,0\}}) - u_2(\bar{\omega} - \lambda/2)] + \beta^2[u_3(\bar{\omega} + e'_{3,\{0,0,0\}}) - u_3(\bar{\omega} - \lambda/2)]\end{aligned}$$

This is *true* if the cost of university education are sufficiently low or the premium to human capital accumulation for an extra period of housing is sufficiently small. This leads to the following proposition:

Hypothesis 5 (P5): *Students whose families receive financially secure assets while in elementary school will have an ambiguous change in university education attainment compared to students whose families*

[11]Note that, in the case where use rights are conferred *after* period 0, $e'_1 = e''_1$. In period 1, $e_1 = f(c_0)$. If use rights are conferred after c_0 is realized, then the first-period human capital accumulation should be the same.

received secure assets while in high school.

This ambiguity comes from the fact that the inequality only holds for a certain range of parameters. We would expect this to be true in contexts with relatively low education costs, as is seen in Chile. In cases with substantial education costs, as in the United States, we would not expect this inequality to hold.

Previously, it was assumed that consumption and income are unchanging. However, it is possible that transfer rights may change income (e.g., if beneficiaries are able to rent out). In this case, income is higher *and* savings pathways open when the family receives transfer rights, which further increases the likelihood that beneficiaries have sufficient savings to pay for university.

1.5 Data

I use data from the Chilean National Socioeconomic Survey (CASEN). CASEN is intended to evaluate the efficacy of government programs aimed at reducing poverty and aiding traditionally underrepresented and marginalized groups. The survey is administered by the Ministry of Social Development. The repeated cross-sectional survey is done every 2-3 years, and data is publicly accessible beginning with the 1990 wave. Beginning in 1996, the survey has information on the year of home purchase and whether or not the home was purchased with a government subsidy. In this paper, I use the 1996, 1998, 2000, 2003, 2006, and 2009 waves of the repeated cross section. Additionally, a follow-up panel was done annually for three years using a subset of the 2006 cross-section, creating a panel data set covering 2006 to 2009. Note that the sample ends in 2009, prior to significant changes to the educational policy environment in Chile, and for that reason I do not include survey waves after 2009. These changes make including the more recent data more challenging and should be analyzed separately.

1.5.1 Methodology

I use the CASEN data in three main ways. First, I pool the CASEN cross-sections from 1996 to 2009. I do this to look at the effect of total years of treatment (defined as school-aged years since the housing subsidy was received) on total educational attainment in levels. I limit the sample to individuals who were at least 25 years old at the time of observation. This prevents students who are still making education investment from being included, instead looking at effects for individuals who have already completed their education investments. I further limit the sample to only include those individuals whose families received housing between 1984 (the first year of the housing subsidy program) and 2009. This leaves in all individuals that report education data and the year the housing subsidy was received. Looking at completed education in levels allows me to explore the cumulative effects of multidimensional housing title over the educational life-span.

However, the levels do not offer a completely satisfactory answer to the effect of multidimensional housing title on education as they do not give a sense of what causes a student to stop attending school. As a result, I use the repeated cross-sections to impute a pseudo-panel covering 1984 to 2009. This pseudo-panel compares years of housing to the probability of dropout in a given year. This pseudo-panel requires strict assumptions about patterns of education, including that education is continuous (dropout is permanent). For the pseudo-panel, the sample is limited to individuals whose households 1) received housing between 1984 and 2009, 2) are under age 25 in the pseudo-panel year, and 3) attended school at during at least one year in period of study.[12] Further details on the construction of the pseudo-panel are available in section 1.6.2. The pseudo-panel approach allows me to account for correlation between the cohort effect and treatment timing. Additionally, secondary school and university attendance are conditional on primary and secondary school attendance. If an individual does not finish primary school, they cannot continue on to secondary and (eventually) post-secondary education. If transfer rights do affect the decision to go to college, perhaps by ensuring the ability to finance college education, then I would hypothesize that this would affect *current* educational investments (e.g. attendance).

This pseudo-panel does not provide complete insight into whether or not security effects from use rights improve educational outcomes at the time use rights are conferred. Additionally, the pseudo-panel makes strict assumptions about continuity of attendance. To relax the assumptions necessary in the pseudo-panel, I use panel data to look at contemporaneous school attendance as a household's title status changes.[13] This panel consists of school-aged individuals (under age 25) from 5,874 households who reported data on whether or not an individual attended school in the last year. I again restrict the panel sample to those households that received housing between 1984 and 2009.

1.5.2 Challenges to identification

Empirical identification relies on the assumption that, conditional on control variables and fixed effects, children in households that received housing before the relevant cut-off age are not statistically different from those in households that received housing after the cut-off age in a way that would correlate to educational outcomes. Housing benefits therefore only affect children's educational outcomes through housing rights,

[12] As a robustness test, I conduct the pooled cross-section and pseudo-panel analysis using the intersection of the two samples.
[13] I use the first year of reported age to calculate birth years and age at housing.

rather than an unobserved channel from housing to education. In specifications with household fixed effects, the identification draws on variation between children within the same household, thereby mitigating concerns over differences between households.

One concern a reader may have is whether or not timing is endogenous. If households can influence when they will receive a home (or where the home they receive will be), then the results would be subject to nontrivial bias. However, this program often has a several year wait-list, preventing families from applying with a strong expectation of when they will be offered housing. Further, the program penalizes individuals who turn down a unit they are offered by putting them back at the bottom of the list (Cummings and Dipasquale, 1997). In a context where the wait-list for housing can be several years long, this decreases the probability that families are strategically applying to get specific houses at specific times and makes the receipt of a home more plausibly random.

It should be noted that the period covered in the data includes a compulsory education reform (2003) requiring students to complete 12 years of schooling (over the previous 8 mandatory years). However, there is still attrition across the sample, reducing total schooling. Further, the timing of this reform should be exogenous to individual households' receipt of housing. As a result, this should bias my results down by making individuals in the control group (those who did not yet receive housing by the appropriate cut-off age) more likely to complete schooling, attenuating any effect spurred by housing rights.

Second, it is not unlikely that households that applied for the housing subsidy may behave differently in the years preceding compared to following their application for housing. For example, a household may worry about the effect of an arrest on their chances of receiving housing. As a result, there is reason to worry that the time before the home is received is actually broken up into multiple time periods with significantly different behavior. The focus on educational outcomes should limit the effect of behavioral changes in the pre-housing years. Further, the data limitations of the CASEN panel work to address this. The panel data only includes behavior prior to home receipt for up to three years, limiting the window of observation. Waiting times for housing could be several years, increasing the probability that the entire observation period is during the post-application period (Cummings and Dipasquale, 1997).

Even if households cannot choose where their subsidized home is, there will still be problems if families make housing decisions based on school availability and vice versa. This would instead capture effects of neighborhoods, rather than housing itself. This problem is mitigated by the structure of education in Chile. Families can choose where to send their children to school irrespective of their place of residence (for public schools) (Berthelon, Matias; Kruger, Diana; Vienne, 2016).

Finally, recent work by Abraham and Sun (2019), Borusyak and Jaravel (2017), Goodman-Bacon (2018), and others have called into question the validity of difference-in-differences with two-way fixed effects, particularly in cases (as in this paper) where cohort effects are not constant over time. This comes from the difference in treatment timing across the sample. I do several things to address these concerns. First, I use a binned specification, which prevents the under-identification issue as described in Borusyak and Jaravel (2017). However, it is likely that a flexible specification could yield important insights into patterns of educational investment. To look at a flexible specification of those presented in the next section of this paper, I first use an event-study framework as recommended by Goodman-Bacon (2018). The fully flexible specification is available in appendix (A.2.3). Future research could consider including cohort-average treatment effects as described in Abraham and Sun (2019).

1.6 Results

In this section, I draw on a differences-in-differences empirical strategy to better understand how changes to household property rights affect educational outcomes. I first do this with binary treatment variables representing whether an individual had only use rights, use rights and transfer rights, or no rights at all at the time they entered secondary school or university. Here, the control group are students whose households received housing *after* the relevant age cut-off.

1.6.1 Pooled cross-section

I first look at the effects of years of treatment on whether or not an individual finished secondary school or university. In the sample, 31% of individuals have 12 or more years of education, whereas only 6.4% of individuals have 16 or more years of education. In this section, I use a binned approach[14]:

$$\text{Education Level}_{i,h} = \alpha + \beta_1 \mathbb{1}\{\text{Use rights only}_{i,h}\} + \beta_2 \mathbb{1}\{\text{Use + transfer rightst}_{i,h}\} + \delta X_i + \lambda_t + \epsilon_{i,h} \tag{1.15}$$

where Education $\text{Level}_{i,h}$ is a binary variable for whether or not individual i has either 12 or more or 16 or more years of education; Years of $\text{Education}_{i,h}$ is the number of years of education reported by an individual in household h (limited to individuals over age 25); and $\mathbb{1}\{\text{Use rights only}_{i,h}\}$ is a dummy variable equal to 1 if the individual i was been treated for less than 5 years at the time of school entry (age J), and

[14]Flexible specifications are available in Appendix A.2.

$\mathbb{1}\{\text{Use + transfer rights}_{i,h}\}$ takes a value of one if the individual had housing for at least five years before school entry. J is 14 when the outcome variable is secondary school and 18 when the outcome variable is university. So, for secondary education, a student who received a house before they were 9 would be indicated as having both use *and* transfer rights, while a student who was between 9 and 14 would be marked as only having use rights. For university education, these cut-offs are instead ages 13 and 18.[15] I include individual controls for birth year (as a series of dummies), birth order, and sex in X_i, and λ_t controls for survey year.

In the pooled cross-section, I am unable to use individual fixed effects because there is only one observation for each individual. I present the results in this section both without any fixed effects as well as with household fixed effects. However, the household fixed effect is based on the household reported in the year of the survey. As a result, if a student has moved out of their childhood home, this fixed effect would compare them to other adults in the new household (e.g. comparing spouses).

The results are reported in columns (1) and (3) of table 1.1. I also present the same specification using household fixed effects and the same individual-level controls for birth order, birth year, and sex, reported in columns (2) and (4) of the same table. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

Table 1.1: Years of treatment on educational attainment (pooled cross-section)

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling
Use Rights	0.0685***	0.0631***		
	(0.00788)	(0.0110)		
Use + Transfer Rights	0.0512***	0.0113		
	(0.00988)	(0.0139)		
Use Rights			0.0135***	0.0196***
			(0.00344)	(0.00487)
Use + Transfer Rights			0.0434***	0.0246***
			(0.00546)	(0.00699)
Constant	0.0575***	0.460***	-0.00171	0.0911***
	(0.00852)	(0.0117)	(0.00396)	(0.00607)
Observations	241,834	241,834	241,834	241,834
R-squared	0.116	0.727	0.022	0.669
Household FE	No	Yes	No	Yes
Clustering	Household	Household	Household	Household
Outcome mean	0.315	0.315	0.0571	0.0571

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample includes individuals at least age 25 at the time of observation. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

[15] The same analysis using the same age cut-offs for both secondary school and education is reported in appendix table 1.4.

The effects of both sets of rights are positive on the probability of high school completion (12 or more years of education). The results show that having use rights only increases the probability of graduating high school by roughly 7 percentage points, while having use *and* transfer rights by age 14 increase the probability by roughly 11 percentage points. These results are consistent if high school attendance is not contingent on the financial effects of housing. Older students (those who only achieve use rights) should be more likely to drop out than younger students as the opportunity cost of education is higher. Therefore, changing housing security has a larger effect on their educational outcomes.

The opposite is seen for university graduation. Students who have only use rights by age 18 are between 1-2 percentage points more likely to graduate from college, while students who had use *and* transfer rights by age 18 are 2-4 percentage points more likely to graduate college. This is consistent with the intuition that college financing requires more advanced planning. These results suggest that educational frictions play an important role in university education investments post-subsidy, but that these frictions play different roles in secondary school versus university education. Students who will have transfer rights by the time they would enter college are more able to make the initial investments (namely, not dropping out of earlier years of school) necessary to go to college.

Next, I will explore three different sets of robustness tests: changes to fertility, age restrictions, and differences across pivot years.

Fertility changes

The theory model I outline in section 1.4 assumes that decisions are made by parents about their single child's education. However, it is likely that these households have more than one school-aged child at any time, meaning that they are forced to make decisions about the family as a whole rather than a single child's education. If schooling is costly, parents might prefer to invest more in the youngest child, sending the older child into the workforce rather than going to college. While these household equilibria fall beyond the scope of the theory I develop in this paper, it is worth examining whether household fertility dynamics affect my main findings. Future work should expand on the theoretical framework in this paper to consider whole-household decision-making.

Here, I address the concern that household fertility changes post-treatment, changing the relative resource share of each student. Previous work has indicated that this may be the case and may improve educational outcomes for children already born at the time housing is received (Galiani and Schargrodsky, 2010). As a first robustness check, I drop households that did have children post-housing. This limits the sample to those households where no member was born after the year housing was received. As a result, households resources (and the subsequent increase from the subsidized home) are divided between the same number of children before and after the subsidy. The results are presented in table 1.2.

Table 1.2: Years of treatment on educational attainment (fertility robustness test I)

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling
Use Rights	0.0966***	0.0567***		
	(0.0124)	(0.0127)		
Use + Transfer Rights	0.0587***	0.0181		
	(0.0158)	(0.0167)		
Use Rights			0.0631***	0.0437***
			(0.00707)	(0.00694)
Use + Transfer Rights			0.0479***	0.0268***
			(0.0107)	(0.00978)
Constant	0.0608***	0.455***	-0.00474	0.105***
	(0.0116)	(0.0127)	(0.00565)	(0.00692)
Observations	137,333	120,009	137,333	120,009
R-squared	0.122	0.718	0.035	0.655
Household FE	No	Yes	No	Yes
Clustering	Household	Household	Household	Household
Outcome mean	0.288	0.292	0.0597	0.0607

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample includes individuals at least age 25 at the time of the survey where no household member was born after the year housing was received. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

Next, I do the same for households who *did* have more children post-housing, presented in table 1.3. For secondary school graduation, the effects of use rights only compared to use and transfer rights become more similar for households that did more children post-housing. This follows if students who are born after housing are able to receive the greatest benefit from the subsidized home because they have had the housing for their whole lives. Students in these growing families are between 5-6 percentage points more likely to graduate high school than students who did not have housing by age 14.

The results do not change significantly for university graduation, which follows if university education is more contingent on financial rights. The large and significant effects for children who achieve use and transfer rights (both for secondary and university education) imply that children who are able to realize the financial effects of transfer rights are able to invest in education at both levels. Overall, students who only have use rights are 1 percentage point less likely to graduate from college, however these effects become positive within households.

Table 1.3: Years of treatment on educational attainment (fertility robustness test II)

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling
Use Rights	0.0498***	0.0579***		
	(0.0103)	(0.0105)		
Use + Transfer Rights	0.0628***	0.00344		
	(0.0127)	(0.0129)		
Use Rights			-0.0102***	0.0131***
			(0.00363)	(0.00398)
Use + Transfer Rights			0.0548***	0.0356***
			(0.00579)	(0.00566)
Constant	0.0132	0.244***	-0.00140	0.0423***
	(0.0132)	(0.0143)	(0.00568)	(0.00668)
Observations	104,501	98,931	104,501	98,931
R-squared	0.104	0.680	0.014	0.620
Household FE	No	Yes	No	Yes
Clustering	Household	Household	Household	Household
Outcome mean	0.351	0.352	0.0538	0.0544

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample includes individuals at least age 25 at the time of the survey where at least one household member was born after the year housing was received. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

Age cut-offs

Next, I test for robustness by using the same cut-off ages for both university and secondary school completion. I use ages 13 and 18 (the cut-offs from the university analysis in section 1.6.1) for both secondary school and university education because 18 is both the age of entry and exit for university and secondary school, respectively. The results remain statistically significant and positive. In appendix A.2, I consider further age restrictions and find that my results are robust to a variety of specifications.

Pivot years

It is intuitive that there are certain levels of education that are likely to have greater completion rates than others. These "pivot" years will have significantly higher completion rates than the subsequent year, as the cost to dropping out before completing a pivot year is significantly higher than dropping out in the next year. First, I show that there are pivots in the data. Figure 1.1 reports a histogram of total years of schooling in the pooled cross-section data. There are obvious pivots at 6th, 8th, and 12th grade.

In table 1.5, I regress binned use and transfer rights on a binary variable for completion of 6th, 8th, and 12th grades. I do the same with the subsequent years of schooling (7th, 9th, and 13th) on the left-hand side. The results are positive and significant for use rights at 8th and 9th grade and positive and significant for

Table 1.4: Years of treatment on educational attainment: age cut-offs 13 and 18

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling
Use Rights	0.0353***	0.0629***		
	(0.00614)	(0.00871)		
Use + Transfer Rights	0.0705***	0.0286***		
	(0.00777)	(0.0108)		
Use Rights			0.0135***	0.0196***
			(0.00344)	(0.00487)
Use + Transfer Rights			0.0434***	0.0246***
			(0.00546)	(0.00699)
Constant	0.0616***	0.456***	-0.00171	0.0911***
	(0.00852)	(0.0117)	(0.00396)	(0.00607)
Observations	241,834	241,834	241,834	241,834
R-squared	0.116	0.727	0.022	0.669
Household FE	No	Yes	No	Yes
Clustering	Household	Household	Household	Household
Outcome mean	0.315	0.315	0.0571	0.0571

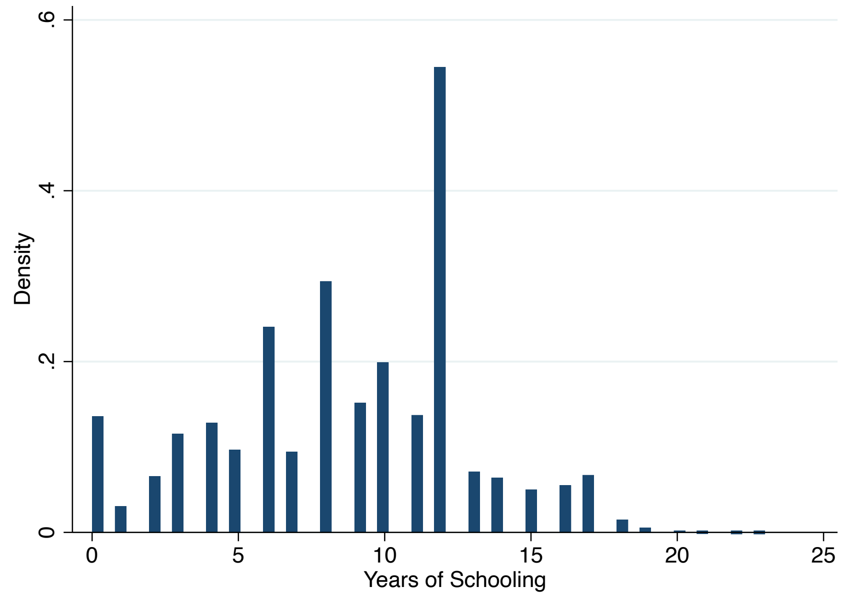
Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample includes individuals at least age 25 at the time of the survey. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

both sets of rights for 12th and 13th grade. The effect of use *and* transfer rights is insignificantly different from 0 for 8th and 9th grade, suggesting that transfer rights do not impact the decision to drop out at the 8th grade level. This is consistent if the barrier to staying in 8th grade is not the ability to pay for schooling (which would be eased by transfer rights).

It is interesting to note that housing use rights have a statistically greater effect on completion of 9th grade than on 8th grade. This suggests that having housing incentivizes students to remain in school (and enter secondary school) where they may have dropped out without housing. Conversely, the impact of housing *transfer* rights is statistically greater for completing 13th grade than for 12th. This suggests that transfer rights do help students (and their families) to overcome barriers to entry specific to university education.

Further, none of the coefficients are significant for 6th or 7th grade. This suggests that there is some barrier in place preventing students from dropping out between 6th and 7th grade. It is possible that this is institutional (mandatory schooling for students of a certain age) or that the outside option (having these children contribute to income or childcare) is not cost-effective. This is intuitive if the opportunity cost of lost wages for students in 8th-12th grade is higher.

Figure 1.1: Histogram of years of schooling (pooled cross-section)



1.6.2 Pseudo-panel

The cross-section analysis presented above does not allow for analysis of behavioral changes during the course of a child's education, instead considering only *ex post* total educational attainment. In this section, I present the results from an imputed pseudo-panel dataset covering 1984-2009. The imputed panel allows me to (under a set of relatively strong assumptions) identify behavioral changes in the probability of dropping out (that is, the year a student would have stopped attending school relative to the year the family received housing). This imputed panel data comes from the CASEN cross-sectional data from 1996-2009. To impute the pseudo-panel, I use self-reported years of education, age, and year of subsidy receipt.[16] I use these variables to create a series of dummies for whether an individual attended school in a given year from 1984 until the year of the survey. The panel is therefore unbalanced, as the only observations in 2009 are observations from the 2009 survey.

The pseudo-panel construction requires more strong assumptions than the pooled cross-section. The cross-section data does not ask when an individual did or did not attend school. Instead, it asks for the number of years of schooling an individual received and (in some years) whether they attended school in the last year. I therefore must assume that recall is accurate for both educational attainment and housing data. Additionally, I must make assumptions about continuous attendance, the age of first attendance, and

[16]This treats every observation in the CASEN cross-sections as unique, which is likely to be untrue for some of the sample.

Table 1.5: Pooled cross-section: pivot years

VARIABLES	(1) 6th Grade	(2) 7th Grade	(3) 8th Grade	(4) 9th Grade	(5) 12th Grade	(6) 13th Grade
Use Rights	-0.00940	0.0117				
	(0.00922)	(0.0100)				
Use + Transfer Rights	-0.00599	-0.00673				
	(0.0129)	(0.0140)				
Use Rights			0.0300***	0.0527***		
			(0.0102)	(0.0109)		
Use + Transfer Rights			-0.0106	-0.000496		
			(0.0125)	(0.0135)		
Use Rights					0.0625***	0.0361***
					(0.00869)	(0.00653)
Use + Transfer Rights					0.0287***	0.0420***
					(0.0107)	(0.00869)
Constant	-0.122***	-0.205***	-0.250***	0.0900***	0.452***	0.186***
	(0.0114)	(0.0118)	(0.0119)	(0.0123)	(0.0118)	(0.00813)
Observations	242,758	242,758	242,758	242,758	242,758	242,758
R-squared	0.704	0.733	0.735	0.733	0.727	0.696
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Household	Household	Household	Household	Household	Household
Outcome mean	0.732	0.627	0.588	0.453	0.318	0.115

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample includes individuals at least age 25 at the time of the survey. "Use rights only" is defined as individuals who received housing by age 12 for 6th grade, age 14 for 8th grade, and age 18 for 12th grade. "Use + Transfer Rights" is defined as individuals who received housing by age 7 for 6th grade, age 9 for 8th grade, and age 13 for 12th grade. Note that the three sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

the permanence of drop-out. For this analysis, I assume that individuals enter school at age 5, and once an individual drops out of school they cannot re-enter.

I use a binned difference-in-differences strategy following:

$$\begin{aligned} \text{dropout}_{i,t} =& \beta_1 \mathbb{1}\{\text{Housing Age } \leq 18\} + \beta_2 \mathbb{1}\{\text{Housing Age} \leq 13\} + \beta_3 \mathbb{1}\{\text{Housing Age} \leq 18\} * \text{Post}_{i,t} \\ & + \beta_4 \mathbb{1}\{\text{Housing Age} \leq 13\} * \text{Post}_{i,t} + \delta X_i + \lambda_h + \gamma_t + \epsilon_{i,t} \end{aligned} \tag{1.16}$$

Here $\text{Post}_{i,t}$ is an indicator variable equal to 1 if the house has been received as of year t and 0 otherwise. This specification includes both household and year fixed effects, with controls X_i for birth year, order, and sex. The results are presented in table 1.6. An additional specification with individual fixed effects is presented in the same table.

Table 1.6: The effect of housing on the probability of dropout (model 1.16)

	(1)	(2)
VARIABLES	Dropout Probability	Dropout Probability
Use Rights	-0.0236***	
	(0.00182)	
Use + Transfer Rights	-0.0223***	
	(0.00150)	
Use Rights * Post-Housing	0.00894***	0.00817***
	(0.00263)	(0.00263)
(Use + Transfer Rights) * Post-Housing	-0.0230***	-0.0223***
	(0.00275)	(0.00276)
Constant	1.028***	0.328***
	(0.0375)	(3.45e-05)
Observations	2,606,947	2,600,040
R-squared	0.660	0.692
Household FE	Yes	No
Individual FE	No	Yes
Year FE	Yes	Yes
Clustering	Household	Household
Outcome mean	0.327	0.327

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample includes individuals under age 25 who received housing after 1984 and who attended school at some point between 1984 and 2009.

With household fixed effects, individuals who had use rights only were roughly 1 percentage point less likely to drop out in a given year post-housing. Individuals who had both use and transfer rights were almost 5 percentage points less likely to drop out post-housing. Further, the probability of dropping out before housing is received is lower, relative to individuals who are at least 18 at the time of the housing subsidy. This suggests that families increase education in the years before housing is received for both older and younger students (who are still under 18). This is consistent with the results found in table 1.1.

After using individual fixed effects, the probability of dropout is higher for individuals who only had use rights than students who did not have housing by age 18. This suggests that gaining use rights disrupts education, possibly by moving schools or if older students need to earn income so the family can move. Those students who achieved both use and transfer rights by age 18 had a reduced probability of dropping out than students who had not achieved either by the time they were 18.

1.6.3 Panel

Next, I use the panel data to look at the contemporaneous effects of switches in title status on attendance. Because I am able to observe attendance (or non-attendance) in each year, I am able to relax the continuity assumptions necessary in the previous sections. The binned difference-in-differences strategy follows that conducted in section 1.6.2:

$$\begin{aligned} \text{dropout}_{i,t} =& \beta_1 \mathbb{1}\text{Housing Age } \leq 18 + \beta_2 \mathbb{1}\text{Housing Age} \leq 13 + \beta_3 \mathbb{1}\text{Housing Age} \leq 18 * \text{Post}_{i,t} \\ &+ \beta_4 \mathbb{1}\text{Housing Age} \leq 13 * \text{Post}_{i,t} + \delta X_i + \lambda_h + \gamma_t + \epsilon_{i,t}. \end{aligned} \tag{1.17}$$

Again, I conduct the same analysis with individual fixed effects. Both sets of results are presented in table 1.7. The effects of both sets of rights are negative for students post-housing receipt. However, students who have use rights only do better pre-housing than post-housing. This suggests that there is some friction caused by housing subsidies for older students within household that inhibits school attendance. This could come from the disruptive effect of moving or changing schools on educational attainment. Within individuals, there is a positive effect of use rights only on non-attendance and a negative effect of use *and* transfer rights. This again suggests that there is some friction facing students who get housing later in their academic careers, preventing them from attending school.

Table 1.7: Years of housing on non-attendance (panel data)

VARIABLES	(1) Non-Attendance (PreK-12)	(2) Non-Attendance (PreK-12)
Use Rights	-0.215***	
	(0.0677)	
Use + Transfer Rights	0.0853*	
	(0.0500)	
Use Rights * Post-Housing	0.0950**	0.184***
	(0.0481)	(0.0504)
(Use + Transfer Rights) * Post-Housing	-0.210***	-0.340***
	(0.0516)	(0.0580)
Constant	0.681**	0.376***
	(0.299)	(0.0252)
Observations	10,081	9,596
R-squared	0.577	0.699
Individual FE	No	Yes
Household FE	Yes	No
Year FE	Yes	Yes
Clustering	Household	Household
Outcome mean	0.261	0.254

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level.

1.7 Concluding remarks

This work seeks to understand how the different benefits of land titling (housing security and financial rights) lead to different education investment decisions. I do this using data from a national, household socioeconomic survey in Chile, where government-subsidized housing is offered conditionally. I show that there are distinct effects of use rights and transfer rights for school-aged children who received housing. Housing security effects (via use rights) directly affect the probability of attending and finishing high school, while housing financial effects (via transfer rights) increase the probability of attending and finishing university. I also find evidence that the effects of use rights and transfer rights are different for secondary school versus university education, with transfer rights having a greater impact on the probability of completing 16 years of education. The results support the hypothesis that transfer rights increase access to university education by opening financial channels. I show that the results are robust to a series of sample restrictions including age, family fertility, and cohort effects.

This work contributes to a deep body of work on the effects of property rights by providing insight into the effects of multiple dimensions of housing rights that are particularly salient to urban contexts. Whereas much of the work on housing/land use rights focuses on land as an income generator, here I identify a case where housing use rights confer housing security and stability. Further, I empirically distinguish between housing as a transferable asset and housing as a source of stability, showing that both are important for children's educational outcomes, but have different effects.

My findings underscore the importance of considering the myriad barriers to accessing education, particularly higher education, including housing security and access to collateral. For students to be able to reap the full benefits of education policy, they must have secure housing and the knowledge that they will be able to afford continuing education. It is particularly important to consider the relative difference in costs for attending different stages of education. Housing use rights may be sufficient to initiate change in primary and secondary school, but financial barriers are a driving constraint against university education.

The insights in this paper can be generalized to a broad set of contexts. I show theoretically that multidimensional property rights have distinct effects on household education investment and provide empirical evidence that this is the case using the case of Chile. Going forward, researchers should test this generalizability in contexts with differing barriers to education. Brazil is a particularly interesting case for this as higher education is free but students must pass a high-stakes national exam (the Examen Nacional do Ensino Médio) in order to be sorted into universities and majors (for details see Melo and Suzuki (2021)). Do housing use rights alleviate stress in such a way as to change ENEM scores? How would this compare to other contexts with costly higher education?

1.7.1 Avenues for future research

Future work could look at the effects of subsidies and title on girls' education. If more, older girls are able to go to school after a subsidy is received, this would suggest that the household margin of substitution is on older girls. Specifically, households are using older girls as "cheaper" home or child care. Similarly, if women's labor force participation increases (for women over 18), this suggests that the household's margin of substitution is working-age women (i.e., mothers). Additionally, future work could explore if families are re-incentivized to enroll students who had dropped out, or if the costs to dropping out have already been internalized.

Attendance is only one element of education attainment. Future research could look at the *quality* of education received rather than the quantity. This would speak to the effect of housing title on overall learning. It is not obvious *a priori* whether housing title can affect learning outcomes. However, the analysis proposed in this paper assumes that attending school in that year is monolithic – students either attend or don't. This assumption comes from a lack of data on daily attendance. If there are changes to learning outcomes following the receipt of housing title, this could suggest that either students are able to attend more schooling *or* that they are able to learn more while they are at school. One potential avenue to do this analysis would be using Chilean national standardized exams or data on college applicants to public universities.

Additionally, research could look into whole-household investment, including investment in property and receipt of other government subsidies, as a way of looking at contact with the state. In contexts with recent histories of of non-democracy and political violence, it is reasonable to believe that there will be an absence of trust in the state both in terms of what is can and will provide and individuals' willingness to be observed by the state. Similarly, if households that received housing are more likely to receive other subsidies, it is possible that changes in education investment come via these other subsidies rather than the financial and security effects of the home itself. If households receiving housing are more likely to get other subsidies, it's possible this is another way of affecting the education changes. This is not a threat to the estimation provided in this paper if we don't expect there to be a change before/after the five year mark, meaning that potential other subsidies are plausibly exogenous to timing. However, if being in a fixed location is what increases the likelihood of receiving these other subsidies, then this is a mechanism of use rights. In this case, this would be yet another effect of use rights increasing visibility to the state.

Chapter 2

Seeking safe harbors: Emergency domestic violence shelters and family violence

Abstract

How do changes in the provision of services for domestic violence (DV) victims change the incidence of violence, both between intimate partners (IPV) and in the family more broadly? I examine how changes to locally available DV emergency shelter capacity affect rates of IPV and DV homicide, the most extreme form of family violence. I collect a novel data set of US emergency DV shelters from 1984 to present, which allows me to look separately at the effects of the opening and closing of shelters (the extensive margin) and the magnitudes of changes in capacity (the intensive margin). In contrast to traditional intra-household bargaining models of domestic violence, I develop a theoretical model outlining how changes to local shelter availability may affect victims' willingness to seek shelter and their post-shelter outcomes. I test empirically how changes to shelter presence versus shelter capacity affect IPV/DV homicide. Although the presence of a shelter (the extensive margin) is associated with reduced incidence of such homicides, this finding does not support strong causal inferences. I find no strong evidence of a causal relationship between changes in shelter capacity (intensive margin) and IPV or DV homicide. I use a variety of econometric methods, including difference-in-differences and instrumental variables designs, to show that the relationship remains a precisely estimated zero effect across time horizons and specifications. I find suggestive evidence that supportive services offered at shelters, including assessment

of the risk of death that the victim faces, may be more effective in preventing homicides. In sum, this study suggests that, given the presence of a shelter, increasing the quality and quantity of services instead of bed capacity may be a cost-effective approach to reduce lethal DP/IPV.

2.1 Introduction

Domestic violence (DV) and intimate partner violence (IPV)[1] is a pervasive problem affecting one in three women and one in four men in the United States. In a given day, over 20,000 phone calls for support services are made to DV hotlines (National Network to End Domestic Violence, 2017, 2019, 2020). DV is a major cause of homicide; from 1980 to 2008 one in every five homicides and 72% of all murder-suicides involved an intimate partner, with women the overwhelming majority of victims (National Coalition Against Domestic Violence, 2020; Cooper and Smith, 2013). Despite extensive work across disciplines to understand the causes and effects of DV, there has remained insufficient data to empirically compare the relative effectiveness of different victim service program types (Sullivan et al., 2018). Funding for DV prevention and response services is limited, requiring organizations and policymakers to make difficult decisions about which programs (and program types) to fund (Iyengar, 2009; National Network to End Domestic Violence, 2020). These decisions are made more challenging due to a lack of rigorous empirical evidence on the effectiveness of different programs.

This paper contributes to this policy discussion by using novel data to explore how DV emergency shelter capacity affects the incidence of lethal violence within households and relationships. DV emergency shelters provide short-term housing (usually between 15 and 90 days) to individuals fleeing violence. While their primary objective is to ensure the safety of clients, these 24-hour, 365 day-a-year shelters offer a host of related services intended to meet the specific needs of DV victims, making them unique in the emergency shelter market. These complementary services may include case management, group therapy, childcare, and legal support, though the quality and availability of these services vary. These shelters are often grossly under-funded relative to need, resulting in a high frequency of individuals seeking shelter with one organization and receiving services elsewhere, or being turned away entirely (Danis et al., 2019; Iyengar et al., 2008; Iyengar and Sabik, 2009; National Network to End Domestic Violence, 2007). DV shelters are

[1] Unlike most economics research on household violence, I draw a clear distinction between DV and IPV. I follow existing definitions of DV as violence among close relatives (e.g., parents, siblings, children, current/former spouses and dating partners), and IPV specifically restricted to current and former dating partners and spouses (Breiding et al., 2015). While there is some overlap in these categories, the constraints faced by a current spouse are very different than those of a dependent child or parent. This distinction allows me to estimate the relationship between service provision and violence for different types of victim/abuser dynamics.

particularly important given the strong links between violence, housing insecurity, and homelessness. DV victims identify safe and secure housing as one of their most pressing concerns (Clough et al., 2014), and 38% of DV victims are housing-insecure at some point (National Network to End Domestic Violence, 2003). In a single representative day in 2018, 42,494 adult and child victims received housing services (e.g., shelter or transitional housing) from local DV programs (National Network to End Domestic Violence, 2019). Given victims' overwhelming need for safe and secure housing, it is vital to understand how to optimize shelters' limited resources to not only provide safe shelter but also improve outcomes once victims leave shelter.

I develop a novel theoretical framework for understanding how service availability may affect the incidence of violence. This framework deviates from traditional household bargaining models of violence as a rational choice by abusers that can be prevented via increased bargaining power (e.g., higher incomes or better outside options) (Card and Dahl, 2011; Chen and Woolley, 2001; DeRiviere, 2008; Farmer and Tiefenthaler, 1997). In these traditional models, when individuals match with a partner, they get a meaningful signal about the potential for future violence that has the potential to change future behavior. However, if these initial signals are not very informative about lethality risk, or if victims (such as dependent children) do not have a credible threat to leave, then these models are unsatisfactory. To address these and other limitations in the existing theoretical literature, the model I develop instead describes victims' crisis management via the stay/leave decision. As such, this model is broadly applicable to all household violence (including violence against children and parents) because it accounts for victims' decisions in the face of known violence, regardless of how individuals arrived in the violent relationship dynamic.

In this framework, victims must choose whether to stay with an abuser (with a given likelihood of experiencing violence in the current period) and leaving (knowing that if they return there is a possibility of retaliatory violence that may be more extreme). If shelter is not available, victims choose to leave so long as discounted future retaliatory violence, plus the cost of temporary homelessness, is outweighed by current-period violence. If victims are instead able to seek shelter, they are more likely to leave, and even more so when shelters have higher quality victim services on-site, which may increase their chance of avoiding future violence.[2] It is reasonable to assume that the homicide rate decreases when the number of individuals in shelter who return to their abuser decreases, which is a function of service quality. However, if there is a mismatch between service targeting and individual homicide risk (i.e., if those at the highest risks for lethal violence do not use either a shelter bed or complementary services) then changes to capacity will have no effect on the homicide rate.

[2]Note that leaving the relationship permanently does not remove the risk of future lethality in some cases, especially given the potential for stalking after separation.

The second major contribution of this paper is the data collected and used in the empirical analysis. Despite the importance of emergency DV shelters, there is a lack of high-frequency data on shelters over space and time, and therefore a lack of rigorous and causal empirical evidence on the effects of shelters on their communities. This data gap is a result of confidentiality, logistical hurtles, and high researcher cost to collecting data from individual shelters.

In this paper, I fill this gap by constructing a novel data set of shelter presence (including opening and closing of shelters over time) and county-level bed capacity in over 400 DV emergency shelters between 1984 to 2020. I identify 1,661 probable shelters and contact each with a survey on their organizational structure, funding sources and affiliations, and history, including any changes to location or capacity since they opened. The resulting data set is an unbalanced panel of 419 shelters from 1984-2020 (434 unique shelter-counties) in 49 states and the District of Columbia. The data set includes information on bed capacity, or the number of individuals who can stay in the shelter at any given time. I use bed capacity rather than measures such as number of bedrooms or number of employees because this is the most comparable way of measuring total services across organizations.

I use this novel data to analyze the effect of shelter capacity on DV and IPV homicide rates, which is a priority concern for DV/IPV service providers. Additionally, homicides offer an empirical advantage as they are less subject to measurement error or reporting bias than other forms of DV/IPV (DeLeon-Granados and Wells, 2003). These data are available from the FBI's Uniformed Crime Reporting Supplemental Homicide Reports (UCR-SHR). I control for county- and state-level characteristics that may affect the local incidence of violence. I supplement this analysis of lethal violence with an assessment of the effects of shelter on non-lethal DV and IPV assault using data from the National Incident-Based Reporting System (NIBRS), though these data are limited.

I assess the short- and long-run effects of shelter capacity changes using a number of empirical strategies. I first demonstrate a substantial, negative relationship between a binary indicator for shelter presence and IPV/DV homicide using a two-way fixed effects estimation strategy. I follow this with a similar strategy where the independent variable of interest is the number of beds available and instead find a precisely estimated zero effect of changes to shelter capacity on lethal violence. To address concerns that it takes time for victims to adjust their behavior in response to shelter capacity changes, I estimate the relationship between the lagged net change in bed counts over the previous five and ten years, as well as estimating effects over time among counties that experienced their first positive change in bed capacity. I find a consistent and precisely estimated zero effect of capacity changes across empirical specifications. I further compare the efficacy of shelter capacity changes to other mechanisms for reducing homicide including lethality assessment programs (Koppa, 2018). The relative effect sizes and program costs suggest that while shelters are an important piece of the DV service network, other programs and policies may be better suited to specifically addressing the issue of DV and IPV homicide.

I make three primary contributions to the literature. First, the data used in this paper is, to date, the only long-run, high-frequency (annual) data on changes to DV emergency shelter bed capacity in the United States. Second, I conduct the first long-run assessment of the effect of bed capacity changes at these shelters on lethal DV and IPV. By differentiating between these two different types of violence, I speak to the growing understanding of how violence affects individuals both within relationships and within the household more broadly. Finally, I develop a novel conceptual framework to improve our understanding of the mechanisms by which DV programming affects lethal DV/IPV.

Given the widespread and pervasive nature of family violence, researchers have spent considerable time and resources trying to understand why DV happens, how to prevent it, and how to support survivor/victims. There is a rich interdisciplinary literature exploring risk factors for DV including social isolation (Lanier and Maume, 2009), prior victimization (Costa et al., 2015; Dutton et al., 2006; Morrissey, 2003), controlled substance use (Campbell et al., 2003), employment and wages (Anderberg et al., 2016; Benson et al., 2003; Carr and Packham, 2020; Fagan and Browne, 1994), fertility (Anderberg et al., 2018), proximity to abusers (Ivandic et al., 2020), and unexpected emotional distress (Benson et al., 2003; Card and Dahl, 2011; Gibson et al., 2001), or the "aggression-frustration" hypothesis (Barlett and Anderson, 2013). Risk factors for homicide specifically include relationship separation (Morrissey, 2003) and access to controlled substances and firearms (Campbell et al., 2003; Dobash et al., 2007). Risk is mitigated via exposure reduction (e.g divorce), access to DV victim services (Dugan et al., 1999) and social networks (Dutton et al., 2004; Kirst et al., 2015), and employment (Anderberg et al., 2016). Researchers have identified myriad reasons why victims stay with their abusers, including economic dependence (Kim and Gray, 2008; Johnson, 1992; Strube and Barbour, 1983), emotional attachment (Strube and Barbour, 1984), and psychological effects of abuse (Choice and Lamke, 1997; Kim and Gray, 2008). These barriers to leaving result in the "revolving door" phenomenon, where victims leave and re-enter the violent relationship (DeRiviere, 2008). Research in economics on why DV victims leave has yet to differentiate between leaving the home to seek temporary safety and permanent separation (e.g., divorce). In this paper I begin to fill this gap through the creation of a conceptual framework distinguishing between these two outcomes of leaving.

Researchers have also been interested in understanding the efficacy of programs intended to reduce violence (and of programs that reduce violence indirectly). Aizer and Dal Bó (2009) find that no-drop orders (which prevent prosecutors from dropping charges at the victims' request) increase DV reporting but decrease homicide. There is mixed evidence on the effects of mandatory arrest policies (which require responding officers to arrest the alleged aggressor), with Chin and Cunningham (2019) finding no effect on IPV homicide and Iyengar (2009) finding that such policies increase IPV homicide. Finally, Koppa (2018) finds that lethality assessment programs in police precincts significantly decreased homicides of women perpetrated by men.

The empirical literature on DV shelters is relatively thin, in favor of small-scale and qualitative methodologies (Grossman et al., 2010; Ham-Rowbottom et al., 2005; Panchanadeswaran and Mccloskey, 2007; Stylianou et al., 2018). Much of this work is intended to support DV organizations and service providers (Constantino et al., 2005; Danis et al., 2019; Gordon, 1996; Jarvis et al., 2005; Lyon et al., 2008). The two pieces of work that most closely tie into this paper are Dugan et al. (1999) and Schechter (2021). In Dugan et al. (1999), the authors estimated the effect of shelter bed spaces using four survey waves between 1976 and 1992 covering 29 large US cities (n = 116). The authors did not find a statistically significant effect of shelter bed capacity on DV homicide. In the second paper, Schechter (2021) uses a binary indicator of shelter presence to estimate the effects of shelters on IPV homicide, divorce, and child maltreatment. Consistent with my findings, Schechter finds a negative effect of binary shelter presence on IPV homicides of women, but negligible effects for the other outcomes. My work expands on those two papers in three main ways. First, my paper uses novel, long-run, high-frequency data on shelter presence *and* capacity, allowing me to estimate the effect of changes on both the intensive and extensive margins. This data set also allows me to estimate a causal relationship between shelters and homicide, thereby expanding on Dugan et al. (1999). Second, by addressing IPV and DV as distinct categories of violence, I speak to the growing understanding of how violence affects individuals both within relationships and within households. Finally, I pair this with a theoretical framework that accounts for both retaliatory violence and program quality.

This work has direct implications for both policymakers and service providers. Evaluation of both the efficacy *and* cost-effectiveness of DV programs is important both for policy makers, to determine how to allocate funding, and for service providers, to choose how to spend allocated dollars. I find that marginal changes in shelter beds do not affect the rates of DV/IPV homicide. However, I want to underscore that these findings *do not* imply that emergency DV shelters are not important pieces of the network of social services for survivors and their families. Instead, this suggests that changes to capacity in isolation are not an efficient way of reducing the local IPV homicide rate, and policymakers should consider how capacity changes affect complementary service availability in-shelter.

The rest of the paper proceeds as follows. In section 2.2, I provide background information on DV emergency shelters and discuss how these organizations complement other programs and policies such as gun control and homeless shelters In section 2.3, I draw on existing literature to develop a theoretical framework and formal model for understanding the relationship between shelter capacity and lethal DV incidence. Section 2.4 describes the processes for collecting the data set on DV emergency shelters and provides summary statistics for original and secondary data. I then discuss methodology, with results in section 2.5. Section 2.6 contextualizes the results and discusses limitations. Section 2.7 concludes by describing policy implications and avenues for future research, using both this data set and data on DV shelter utilization more broadly.

2.2 Background and context

Emergency DV shelters are unique in the emergency shelter market. Clients are able to stay in shelter 24/7 for anywhere from a few days to several months (Ben-Porat and Sror-Bondarevsky, 2018). Shelters often partner with other social service organizations including police departments and housing placement and job assistance programs. Many work with pro-bono local attorneys to help clients file temporary protective orders (TPO) or advise on custody issues (Ben-Porat and Sror-Bondarevsky, 2018; Glenn and Goodman, 2015; Grossman et al., 2010). Shelters often work with 24-hour crisis lines (staffed by volunteers or phone-answering services) to quickly respond to calls for assistance. Some are able to take walk-ins to the shelter itself, while others conduct screenings at a separate location such as an administrative office. Emergency DV shelters have to adapt to the unique risks their clients face. Many shelter locations are confidential and strictly protected. Shelters often have security cameras, locked doors, or other methods of keeping clients safe from abusers(Glenn and Goodman, 2015).[3] During their stay, clients often have to follow “house rules” such as abstaining from drug and alcohol use, attending group therapy sessions, working or searching for work, maintaining shelter confidentiality, or meeting a curfew.[4] Shelter procedures are highly varied and likely a result of differences in funding, staffing, and accessibility (Hughes, 2020).

The first emergency DV shelter in the United States opened in St. Paul, Minnesota, in 1974 (Twin Cities PBS, 2019). Like many of the shelters that followed in the 1970s and 1980s, the shelter was open to

[3]During informal conversations with shelter employees and volunteers, I was told of an instance when a shelter’s previously confidential location was accidentally published in a local phone book. The shelter had to quickly react to increase safety on-site to protect its clients. The shelter now operates at a publicly disclosed location with other security measures.

[4]Many researchers are interested in how strict cultures in-shelter affect clients’ well-being and sense of agency as they process the trauma of their abuse. For further information, see Bergstrom-Lynch (2018) and Glenn and Goodman (2015).

women and their children fleeing violence. Emergency DV shelters began as an attempt to separate resource allocation from programs specifically targeting homelessness. Advocates claimed that facilitating separation of abusers from the abused partners would be a better use of limited funding (Aratani, 2009; Berk et al., 1986). While also trying to curb chronic homelessness, practitioners and social service providers attempted to isolate the issue of housing insecurity from DV-specific concerns. It has since been found that the profound overlap of populations served across the two different types of shelters makes it nearly impossible to deny participation in an emergency DV shelter to an individual who is not homeless based on wanting to preserve space (National Network to End Domestic Violence, 2017). Indeed, Clough et al. (2014) found that 80% of women with children seeking shelter had experienced DV, while 57% of women seeking shelter were fleeing DV.

DV shelters have to balance numerous, often countervailing objectives for their clients. These goals include – but are in no way limited to – things like securing safe, long-term housing; providing immediate safety; creating opportunities for growth and improved well-being; securing employment and enrolling clients in food assistance programs; facilitating safe childcare for clients' children; and helping clients to secure temporary restraining orders or custody of any shared children. Unlike traditional "homeless" shelters,[5] DV shelters place a premium on security and anonymity, which are high priorities for clients at many DV shelters (Solari et al., 2017). Shelter employees often describe having to wear numerous "hats" to give their clients the assistance needed to accomplish some or all of these goals. To do this, shelters offer a wide range of complementary services, including providing case managers and triage services on a shelter-by-shelter basis (Nuzhat and Sompura, 2018).

Notably, conversations with shelter employees indicated that shelters do not consider avoiding future violence or avoiding returning to their abusers as a "goal" for their clients. In part, this is because shelters know that this is beyond their control and does not fit under the tenets of "trauma-informed care," which advocates for clients making their own choices about their own lives. This consists of a three-pronged

[5]The closest program type comprises shelters targeted toward the general population of individuals experiencing temporary or chronic homelessness, estimated at 553,742 individuals on any given night (National Alliance to End Homelessness, 2021). Homeless shelters are much more prevalent than DV shelters, but are often only open during the night and require clients to leave during the day. Many large-scale shelters are also first-come, first-served, preventing clients from having continuity of care. Homeless shelters traditionally focus on basic needs (safety, warmth, food, and shelter). Many are segregated into shelters for women and children and shelters for men. Homeless shelters offer a wide range of services, including providing case managers and triage services on a shelter-by-shelter basis (Nuzhat and Sompura, 2018). In this way, emergency DV and homeless shelters are similar. However, homeless shelters frequently offer less safety than DV shelters (Solari et al., 2017). Homeless shelters historically charged a fee (set by the shelter) to use the facility and access further resources, though this practice has largely ended (Clough et al., 2014). Clients at homeless shelters are often limited to 3-5 consecutive nights based on shelter capacity, with a few exceptions offering 30 nights of shelter (Milby et al., 2005). The number of nights spent in a homeless shelter annually is also tracked and limited in many larger cities, with the exception of during conditions like cold nights or extreme weather (Nuzhat and Sompura, 2018).

approach to working with victims of trauma, addressing the need for 1) safety, 2) connection, and 3) emotional management (Bath, 2008). In the context of emergency DV shelters, this includes such policies as giving family units their own room (rather than having strangers room together); therapy that focuses on affirmation and collaborative problem-solving; and including residents in decisions made by the shelter. Researchers in social work, sociology, and psychology have found that trauma-informed care improves the well-being of clients in shelters (Danis et al., 2019; Glenn and Goodman, 2015; Hughes, 2020; Stylianou and Pich, 2019; Sullivan, 2012; Sullivan et al., 2018).

Here, I focus on how changes to the capacity of the shelter – the number of clients who are able to receive services in-shelter – affect aggregate outcomes. Informal conversations with shelter representatives during the course of data collection indicated that shelter bed capacity may change for a number of reasons. Small-scale changes may include purchasing additional cots or converting a bedroom into an office for shelter staff, while larger changes may include purchasing a new or secondary site or undertaking an expansion on current property. Small changes may be relatively quick to accomplish, while larger changes and new shelter construction can cost millions of dollars in addition to regular operating costs(Erickson, 2014; Morgan & Morgan Business Trial Group, 2016; The Family Center, 2021).

In this paper, I focus on marginal changes to bed capacity (rather than alternative measures of capacity such as number of employees or total victims served per year) because this metric is (1) relatively comparable across time and contexts and (2) able to be reported retrospectively. Additional bed spaces also represent access to a (varied and) broad set of complementary services, as residents in shelter will be able to take advantage of other services offered by the organization. Therefore, measuring total bed capacity captures how many individuals a shelter is able to serve in ways beyond simply offering safe housing. However, limiting attention to bed capacity may mask alternative services offered by shelters – such as non-residential counseling and group therapy, legal services, and childcare – that also improve outcomes for survivors who seek support from these organizations. Further, this prevents me from understanding the broader network of services beyond residential DV shelters such as local food banks, homeless shelters, and rent assistance programs.

Emergency DV shelters exist alongside complementary programs such as DV transitional housing. These programs provide long-term housing (usually between six months and two years) for victims and their children fleeing violence. As transitional housing is beyond the scope of the survey instrument used, I exclude transitional housing shelters from my analysis. Future work should look into how transitional programs specifically affect the revolving door phenomenon and violence incidence. Another program type uses temporary placement in housing such as a scattered site model (multiple houses or apartments operated by the shelter organization) or hotel vouchers. I do not include scattered site or hotel-based shelter programs in this analysis.

2.2.1 Shelters and homicidality

Because shelters are only able to offer a given number of individuals space in-shelter (due to capacity constraints, employee and service constraints, or other limitations), shelters must make difficult choices over which potential clients are able to access shelter. Conversations with shelters indicated that homicidality and lethality[6] are of particular concern, especially when it comes to making decisions about serving clients immediately versus placing them on a wait list. As studied in (Koppa, 2018), similar homicidality assessments are used by police departments, corrections departments, shelters, and other DV service providers (for a few examples of such assessments, see Campbell (2003); Idaho Coalition Against Sexual & Domestic Violence (2012); Maryland Network Against Domestic Violence (2005); Mental Health Centre Penetanguishene Research Department (2005)). These metrics include many of the known risk factors for DV/IPV homicide, including prior choking by the abuser, suicide attempts or threats of suicide attempts by the abuser, firearm access, and abuser unemployment.

In order to be referred to immediate, next-level care, respondents must score above a given threshold, though this threshold varies by assessment. To avoid strategic answering by clients seeking shelter or further support (especially in the context of DV shelters), these assessments may not be public knowledge.[7] The use of these assessments underscores the difficult trade-offs between choosing to allow someone into shelter today versus holding a space for tomorrow. When faced with a risk of homicide today, shelters often prioritize minimizing the risk of immediate lethal violence over serving individuals not at risk for homicide.

2.2.2 Shelter accessibility

There is a longstanding debate over who should be allowed to stay in emergency DV shelters. Some advocates take issue with housing anyone other than cisgender women and their children. This includes debate about the age at which male children will not be allowed to stay in-shelter with their mothers (Côté et al., 2018). Norms have shifted over time, and, in 2012, the U.S. Department of Housing and Urban Development

[6]In general, homicidality refers to the risk that the person seeking shelter may be killed, while lethality is more broad.

[7]During confidential correspondence with a shelter employee, I was informed that employees at that shelter triage potential shelter clients using a homicidality risk assessment. A potential client's score determines whether that client is served in the shelter immediately or placed on a wait-list and provided with alternative services. The shelter was unwilling to share the metrics used for fear that making the metric public would allow for strategic abuse by certain individuals seeking shelter.

(HUD) issued the "Equal Access to Housing in HUD Programs Regardless of Sexual Orientation or Gender Identity" rule requiring (among other things) that emergency shelter providers receiving HUD funding serve clients without regard for gender or sexual orientation. The 2016 "Equal Access in Accordance with an Individual's Gender Identity in Community Planning and Development Programs Rule" further required federally funded shelters to provide similar services to transgender, non-binary, and gender non-conforming clients (U.S. Department of Housing and Urban Development, 2021). While many shelters had already expanded their services to individuals who are not cisgender women, federally funded "women's shelters" were required to update their policies and practices to serve all potential clients.

2.2.3 The broader policy environment

State policy unrelated to shelters may also affect victims' ability and willingness to interact with local support services. In jurisdictions where an officer "may arrest" or "is authorized to arrest" in response to a DV call, responding officers have a significant level of discretion. Such laws allow law enforcement to make an arrest if there is probable cause that an act of domestic violence has occurred, but do not mandate arrest.

Some states have aimed to reduce this discretion by adopting "pro-arrest" or "preferred" arrest policies, where an officer "should arrest" or "is encouraged to arrest". As the name suggests, these do not require that law enforcement officers make an arrest. Since the 1980s, many states have adopted mandatory arrest laws which require that responding officers arrest abusers, though the details vary widely (for example, an officer "shall arrest" or "must arrest"). These require law enforcement to make an arrest if there is evidence that an act of domestic violence has taken place or that there is an imminent threat of physical or sexual harm. Chin and Cunningham (2019) find no effect of mandatory arrest on IPV homicide, in contrast to the findings in Iyengar (2009).

Appendix table B17 describes mandatory arrest laws by state. If the state did have a mandatory arrest law, it lists the first year that the mandatory arrest law was codified. These laws are even further complicated by limitations in scope. As an example, California has a mandatory arrest policy specifically for violations of protective orders, but allows officers discretion otherwise. Iowa allows for discretion except in cases where a deadly weapon is used or a physical injury has occurred. For the purposes of this paper, state-level mandatory arrest laws will be subsumed in the state-by-year fixed effects. I do estimate a model interacting bed counts with a state-level indicator for whether arrest is mandatory, provided in the appendix.

Given the interconnectedness of family violence and firearms, there have been both federal and state efforts to limit access to firearms by individuals convicted of DV. The 1996 Lautenberg Amendment to the

Federal Gun Control Act of 1968 (Title 18, United States Code, Section 922(g)(9)) prevents individuals convicted of misdemeanor DV charges from owning firearms. Some states have passed more stringent laws, including requiring individuals convicted of misdemeanor DV to surrender their weapons or to be registered on the Instant Criminal Background Check System (NICS). Appendix table B19 describes restrictions on purchasing and owning firearms and laws authorizing or requiring individuals to surrender firearms to local authorities. Similarly to arrest discretion policies, these federal and state-level laws will be subsumed by the fixed effects. Future work should consider how to best assess the effects of firearms restriction laws on lethal DV.

2.3 Theoretical framework

In this section, I describe existing work modeling family violence and the "stay/leave" decision. I then describe where the model I develop deviates from existing work and the reasons for these deviations. Finally, I outline the formal model and the hypotheses it yields.

Existing economic theory on DV has traditionally approached family violence using non-cooperative household bargaining models.[8] These models take a rational actor approach wherein the victim and the abuser are both trying to optimize over a series of different constraints. Violence is modeled as either instrumental, representing an intrinsic utility to perpetrating violence, or expressive, as a response to salient emotional cues (Card and Dahl, 2011). Often, the threat of violence is used as a tool to extract some behavior from the victim. Such models are aimed at understanding why violence occurs in the first place and treat violence as an "optimal strategy" on the part of the abuser. However, the existence of any violence is sub-optimal, and, from the perspective of abused persons, violence is often effectively random and non-preventable. Research has demonstrated that abusers should not be considered as rational actors. While some violence may be instrumental, as proposed in a household bargaining framework, other violence is reactive to outside forces (such as unemployment) or even random.

Alternatively, models focus on the effect of income on potential victims' partner selection *before* entering into a relationship that will become violent in the future (DeRiviere, 2008). Using assortative matching frameworks allows the economist to identify the switch point where individual bargaining power (e.g., income) leads to a non-violent outcome. This is unsatisfactory for three reasons. First, it puts the onus for violence prevention on the victim to anticipate the potential for future violence. Second, violence may be

[8] For some examples, see Chavas and Klein (2020); Chen and Woolley (2001); Farmer and Tiefenthaler (1997); Tauchen et al. (1991). For an overview of classical models of DV and the shortcomings of such models, see (DeRiviere, 2008).

unpredictable at the time the match is made. By contrast, the model I develop instead operates as a model of crisis management conditional on violence in the match. Finally, such models inherently exclude DV that affects those who have no say in the match. Unlike intimate partners, children and dependent adults do not enter into these relationships voluntarily. As such, the implications of such models are limited when considering the choices made by individuals (particularly dependents) experiencing violence.

In sum, the nature of DV requires economists to rethink common assumptions used in household bargaining models. First, victims are trying to optimize under a series of extreme constraints, requiring that they balance avoiding harm and staying alive, protecting and providing for their children, and avoiding community and cultural stigma, to name only a few of their objectives(Strube, 1988). These goals do not translate easily to the traditional economic framework of utility maximization. This is compounded when we consider cases where the victim must choose between a high risk of perceived homicide and an unknown outside risk. The literature on the value of a statistical life emphasizes that individuals are unable to efficiently estimate the value of their own lives in monetary terms, meaning that bargaining games cannot effectively model cases where the potential outcome is homicide (Banzhaf, 2014). I circumvent this issue by modeling an individual's choice in high-stress circumstances with a strong probability of present and future violence.

Further, DeRiviere (2008) highlights features of abusive relationships that call into question traditional models of the family in the context of violence, specifically the "revolving door phenomenon," whereby victims of domestic abuse leave an abuser and return more than once. (Gordon et al., 2004) find that often forgiveness and a desire to "move on" is the key determinant of whether a victim returns to their abuser. To account for this, Hamby and Gray-Little (2007) uses a "a model of risk-based coping, which posits that most victims of violence are making realistic appraisals of their life situation. . . ." In this paper, I directly account for the revolving door phenomenon by modeling the stay/leave decision as one that is predicated on a given probability of future return, allowing victims to leave their abusers multiple times.

Additionally, victims of DV think about the future differently (and optimize differently) than individuals who have not been abused. Aizer and Dal Bó (2009) argue that this manifests as time-inconsistent preferences. Those authors show that strong commitment devices (such as no-drop orders, which require prosecutors to maintain charges against abusers even if the victim wishes to drop them) do increase DV reporting.[9] Shelter instead provides a low commitment device for leaving – victims are able to go to the shelter and leave at will (though their behavior in-shelter may affect the likelihood they will be taken in by the same shelter in the

[9]Interestingly, no-drop orders decrease the rate of DV homicide of men. The authors hypothesize that this reflects a decrease in the likelihood of victims of abuse killing their abusers.

future). In the game-theoretic literature, shelter is traditionally considered to serve as an (imperfect) outside option for victims of violence. Indeed, Berk et al. (1986) posits that, for shelters to be an effective deterrent of domestic abuse, abused partners must have a credible threat that they will use a shelter (e.g., the victim must know shelter exists and be willing and able to communicate this to the abusing partner). However, these models have their limitations, including the assumption that the threat to leave will temporarily stem abuse. On the contrary, a threat to leave may actually increase abuse before or after the shelter stay via retaliation effects (Berk et al., 1986). Interpreting this risk of retaliatory violence in the context of the stay/leave decision is important because this is the decision that ultimately leads a victim to seek shelter. I account for this in the model by considering a case where the decision to seek shelter itself can affect the magnitude of retaliatory violence, and where the decision to leave and seek shelter is a function of the relative cost of entering shelter (such as restrictive or uncomfortable shelter conditions).

Building a comprehensive model of the stay/leave decision is beyond the scope of this paper. The data set I use does not include any data on shelter use by actual victims of violence. As such, the framework I develop creates a basic model of the stay/leave decision in the absence and presence of shelter. This can be thought of as a model of crisis management, which describes how individuals cope with highly stressful events when facing extreme behavior, rather than day-to-day behavior. I use the stay/leave decision framework to interpret the effects of changes to shelter capacity (and changes in service availability in-shelter) on the county-level lethal DV rate. While lethal DV is the outcome variable of interest in this paper, the results of the framework (and the hypotheses it generates) should generalize to all forms of retaliatory household violence.

Additionally, the model I develop can be thought of as a partial equilibrium model of a single individual's choice to seek shelter. However, the shelters themselves are likely solving a general equilibrium problem, choosing how to prioritize who gets shelter and when. Due to the concerns about confidentiality outlined above, it is difficult to access information about how shelters are making these strategic decisions, the information they have, the constraints they face, and so on. Because the individual is effectively unable to manipulate the likelihood of getting into a shelter, my model takes the probability of receiving services as given from the perspective of the individual. Therefore, I make a reduced-form simplification holding capacity constraints constant at any given time. However, a general equilibrium model of shelter choices is a direction for future research.

2.3.1 Formal model

An individual i is involved in an abusive relationship. In a given period, there is a probability that the individual will be subject to violence by their partner. The level of violence experienced in a given period is a function of the maximum level of violence under the current violence paradigm.[10] For some victims of DV, this maximum level of potential violence will result in death of both the abused partner and the abuser (e.g., "murder-suicide"). Violence is randomly drawn from a distribution over $[0, \bar{\nu}]$, generalized as $f(\bar{\nu})$. Here, violence is a multidimensional parameter representing many types of violence an individual can experience. By assuming that violence is orthogonal to consumption (i.e., the utility from consumption is not a function of violence), this becomes effectively a violence-minimization problem (or, equivalently, cost-minimization subject to a budget constraint) instead of a traditional utility-maximization problem.

It is important to note that, while the magnitude of abuse on any day is modeled as stochastic, in reality abusers are often extremely manipulative and intentional in their abuse. From the perspective of the victim, however, there is an unpredictability to what will "set off" an abuser, and with what result. For this reason, the lower bound of the distribution stays at 0 because some days there will be no abuse. The unpredictability of what each day will bring adds to the victim's trauma.

Each period, the victim faces the choice of staying and experiencing the random draw of violence or leaving and attempting to find a safe place to stay. The victim maximizes over expected violence today and tomorrow, subject to a discount rate $\delta \in (0, 1)$. The victim faces a cost to leaving, β, which can be thought of as the cost of leaving the relationship, even temporarily, or from having to spend the night without housing. This cost of leaving varies by individuals and accounts for several of the constraints victims face when leaving. While some victims are able to stay with friends and family, others (such as immigrants) may be more isolated from the community. Further, victims with children may face a higher β if they risk disrupting their child's schooling or potentially even losing custody after leaving the abuser.

If the victim returns to the abuser after leaving, they are subject to daily violence $f(\bar{\nu})$ as well as an additional retaliatory violence penalty, such that the draw of violence comes from $g(.) \geq f(.)\ \forall \nu$.[11]

I begin with the simplest case, where there is no retaliation effect, no shelter, and return is certain. In

[10] A more complex functional form would include the path dependence of violence, where current period violence is a function of violence experienced in the past. In such a model, violence today is a function of the maximum level of violence experienced *ever*, reflecting that once the "bell has been rung" of a certain magnitude of violence, it is possible that that level of violence can be seen again in the future. In such a functional form, $\bar{\nu} = h(\bar{\nu}_t)$, where $\bar{\nu}_t$ is the maximum level of prior violence and $g(\bar{\nu})_t > \bar{\nu}_t$.

[11] This is consistent with the findings in Harding and Helweg-Larsen (2009), where those authors find that women in shelters often have an accurate belief about the probability of future violence if they return to their abuser.

this case, the victim will choose to leave rather than stay so long as

$$
\begin{aligned}
f(\bar{\nu})(1+\delta) &> \beta + \delta f(\bar{\nu}) \\
f(\bar{\nu}) &> \beta. \qquad (2.1)
\end{aligned}
$$

In other words, the victim will leave so long as the cost of leaving (β) is less than the expected violence the victim faces today.

If instead return is uncertain, then the victim has a probability $q \in (0,1)$ of not returning and a probability $(1-q) \in (0,1)$ of returning. This probability of return is a function of the individual's outside options and characteristics, meaning that some individuals have a high risk of return regardless of the cost of leaving and potential for assistance from outside options.[12] Conversely, others will have a low risk of return, indicating that leaving is most likely leaving for good. For the remainder of the model, I will assume that the risk of return is at the level of the individual, but different factors may shift this risk up or down.

In this case, the victim chooses to leave rather than stay so long as

$$
\begin{aligned}
f(\bar{\nu})(1+\delta) &> \beta + \delta(\beta * q + (1-q) f(\bar{\nu})) \\
f(\bar{\nu}) &> \beta.
\end{aligned}
$$

Here, the necessary condition for leaving is the same as in equation 2.1. In other words, regardless of the probability of return, the victim will choose to leave under the same trade-off between violence today and costs of leaving in the absence of shelter and retaliatory violence.

If instead there is a retaliation effect and the victim faces certain return, the victim will leave if

$$
\begin{aligned}
f(\bar{\nu})(1+\delta) &> \beta + \delta g(\bar{\nu})) \\
f(\bar{\nu}) &> \beta + \delta(g(\bar{\nu}) - f(\bar{\nu})). \qquad (2.2)
\end{aligned}
$$

In other words, the victim will leave so long as today's certain violence exceeds the cost of leaving plus the discounted retaliation penalty. This could be the case if a victim has family to stay with, making the cost of leaving small, or if the victim has a belief that retaliation will be small (as when $g(\bar{\nu}) \to f(\bar{\nu})$).

[12] This may be represented as $q = h(\beta, X)$, where X is a vector of individual-level characteristics. For brevity, I will generalize this probability as q throughout the paper.

Combining the cases of retaliation and uncertain return with absence of shelter, the victim leaves if

$$f(\bar{\nu})(1+\delta) > \beta + \delta(\beta * q + (1-q)g(\bar{\nu}))$$
$$f(\bar{\nu})(1+\delta) - (\delta - \delta q)g(\bar{\nu}) > \beta(1+\delta q). \tag{2.3}$$

Here, the victim leaves if the difference between the expected violence if they stay and the discounted violence expected upon returning exceeds the cost of leaving over this period and the next.

Suppose instead that there is an emergency DV shelter that can house the victim for the night. First, I consider the case where shelter changes the relative cost of leaving, such that $\beta_s \neq \beta$. If $\beta_s < \beta$, then the victim will prefer to go to the shelter rather than the alternative outside option, and the victim will choose to leave for more values of $f(\bar{\nu})$. This may be seen as representing a case where a DV shelter is more appealing than a homeless shelter. Conversely, if the cost of leaving is higher with respect to the shelter than the outside option (which may be the case if the victim has supportive family living nearby or if shelter rules are restrictive and uncomfortable), then the victim will never choose to go to a shelter, even if the victim chooses to leave.

If the shelter does not change the probability of return (e.g., $q_s = q$), then there is no change to the condition necessary for the victim to leave. If instead $q_s > q$, such that going to the shelter reduces the probability of return relative to another non-shelter option, then the victim will choose to go to the shelter so long as the threshold to go to the shelter is greater than the threshold to leave in the absence of shelter. In the model's notation, this is represented as

$$f(\bar{\nu})(1+\delta) - (\delta - \delta q_s)g(\bar{\nu}) - \beta(1-\delta q_s) > f(\bar{\nu})(1+\delta) - (\delta - \delta q)g(\bar{\nu}) - \beta(1-\delta q)$$
$$q_s(g(\bar{\nu}) - \beta) > q(g(\bar{\nu}) - \beta)$$
$$q_s > q.$$

Note that this is always true so long as $g(\bar{\nu}) > \beta$, which is inherent in the assumption that $g(\bar{\nu}) \geq f(\bar{\nu}) > \beta$.[13] Therefore, if there is an option to go to a shelter and the magnitude of the retaliation penalty is not a function of the choice to go to the shelter, then more victims will leave in the presence of shelter than in the absence of shelter.

[13] Here, I assume that $g_s(\bar{\nu}) = g(\bar{\nu})$, meaning that from the perspective of the abuser, retaliatory violence is not a function of the choice to go to the shelter versus another outside option. This assumption is reasonable, as many shelters prioritize client confidentiality, even going so far as to make their locations confidential and only accessible to clients accepted into the shelter.

Proposition 1 (P1): *DV victims are more likely to leave (even temporarily) when there is a local shelter available.*

However, it may be the case that the level of retaliatory violence is endogenous to the choice to go to a shelter. This may result from the abuser wanting to further punish the victim for attempting to seek help from services that are specifically for DV victims. In this case, the level of retaliatory violence from seeking shelter exceeds that from leaving to stay with a family member or friend ($g_s(\bar{\nu}) > g(\bar{\nu})$). The victim will then choose to seek shelter rather than leave without going to a shelter so long as

$$f(\bar{\nu})(1+\delta) - (\delta - \delta q_s)g_s(\bar{\nu}) - \beta(1-\delta q_s) > f(\bar{\nu})(1+\delta) - (\delta - \delta q)g(\bar{\nu}) - \beta(1-\delta q).$$

If $q_s = q$, then this constraint holds so long as $-(\delta - \delta q)[g_s(\bar{\nu}) - g(\bar{\nu})] > 0$. This is never true for $g_s(\bar{\nu}) > g(\bar{\nu})$ and $\delta, q \in ()0, 1)$. If instead $q_s > q$, then the inequality reduces to

$$(\delta - \delta q)g(\bar{\nu}) - (\delta - \delta q_s)g_s(\bar{\nu}) > \delta\beta(q_s - q). \tag{2.4}$$

In other words, if the relative risk of return outweighs the additional retaliation penalty incurred due to going to a shelter, then the victim will choose to go to a shelter over the alternative of leaving the abuser but not going to a shelter.. This leads to the following proposition:

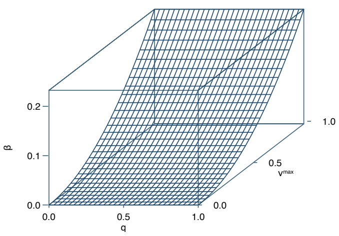
Proposition 2 (P2): *DV victims are more likely to leave (and go to a shelter) when they anticipate shelter will reduce the risk of return (e.g., when shelter "quality" is higher).*

Figure 2.1 demonstrates how the presence of shelter changes the area over which a victim will choose to leave their abuser for the night with and without shelter.[14] Here, individuals with parameters below the map will prefer to leave over staying, while individuals on the mapping will be indifferent between leaving and staying. Retaliation effects decrease the area of leaving when there is certain return (sub-figure 2.1b), but this is partially offset by decreased probability of return upon leaving (sub-figure 2.1c) as a result of seeking shelter. However, the area of leaving to go to shelter decreases further when retaliation effects are higher in the event of leaving (sub-figure 2.1d). Sub-figure 2.1e shows the area of leaving when shelter is available changes both the probability of return and expected retaliation violence.

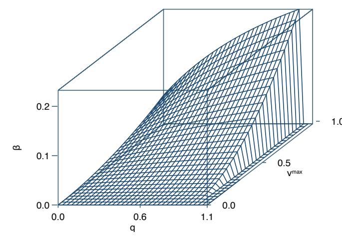
Figure 2.1 assumes that the population is distributed uniformly among the three parameters. However,

[14]For these demonstrative figures, I assume a discount rate $\delta = 0.75$; $q, \bar{\nu}, \beta \in [0, 1]$; $q_s = q^{1/3}$; $f = 0.25\bar{\nu}^{1/2}$; $g = 0.5\bar{\nu}^{1/2}$; and $g_s = \bar{\nu}^{1/2}$.

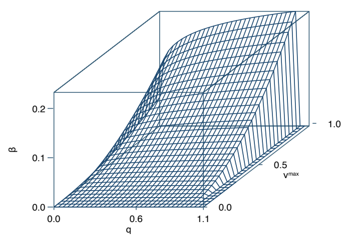
Figure 2.1: Mapping the area over which a victim will choose to leave as the area captured underneath the three-dimensional surface mapped. Illustrative coefficients and functional forms assumed.



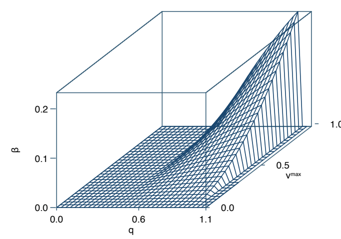
(a) There is no retaliation and no shelter



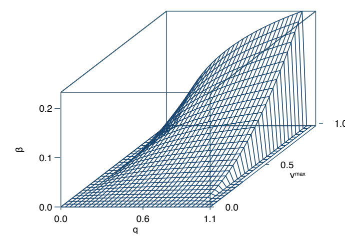
(b) Retaliation is possible, but victims cannot seek shelter



(c) Shelter decreases the probability of return $q_s > q$, $g(\bar{\nu}) = g_s(\bar{\nu}) > f(\bar{\nu})$



(d) Shelter increases retaliation $q_s = q$, $g_s(\bar{\nu}) > g(\bar{\nu}) > f(\bar{\nu})$



(e) Shelter increases retaliation but decreases the probability of return $q_s > q$, $g_s(\bar{\nu}) > g(\bar{\nu}) > f(\bar{\nu})$

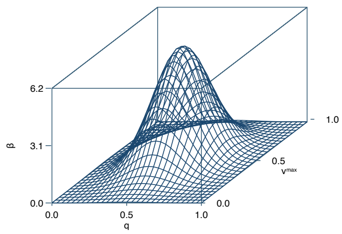
there are certain realizations of the $\{q, \bar{\nu}, \beta\}$ triplet that are less likely than others.[15]

In figure 2.2, I map the area of leaving relative to an example population distribution. For the purposes of this example, I assume q and $\bar{\nu}$ are normally distributed with mean 0.5 and standard deviation 0.16, represented in sub-figure 2.2a. Where the population distribution is above the mapping from figure 2.1, the victim will not choose to leave because the relative costs of leaving today are too high. Observe that the area of leaving remains the same for high values of q and $\bar{\nu}$, as would follow intuition.

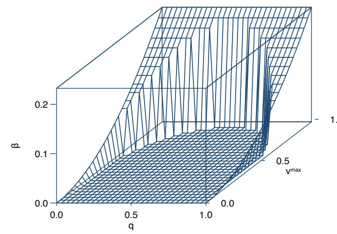
While the chosen shape of the distribution of the three parameters is relatively arbitrary for the purposes of illustration, it is useful to consider how changes to that underlying distribution affect who chooses to stay with the abusive partner. In the previous example, the parameters are normally distributed around the mid-point of the parameter intervals. However, it is quite possible that in reality, the distribution would be centered toward one of the corners of the three-dimensional space. As an example, consider the case where the distribution is centered toward a higher value of $\bar{\nu}$, meaning that the maximum level of daily violence is high across the population. In this case, a larger mass of individuals would choose to leave, as their draw of

[15] For example, the probability of returning is likely higher for individuals with children who might struggle to get legal custody, meaning they have a very high β.

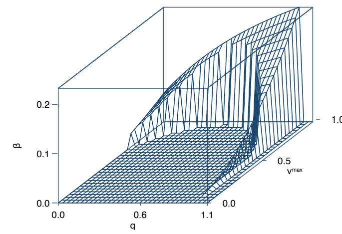
Figure 2.2: Mapping the area over which a victim will choose to leave as the area captured underneath the three-dimensional surface mapped. Illustrative coefficients and functional forms assumed.



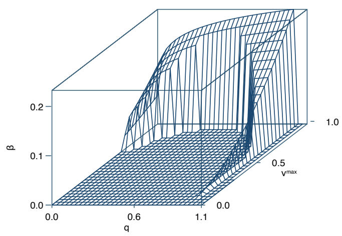
(a) Normally distributed population parameters



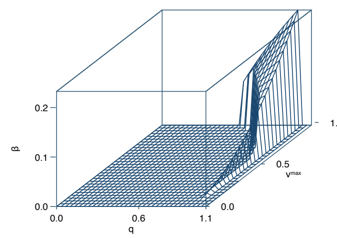
(b) There is no retaliation and no shelter



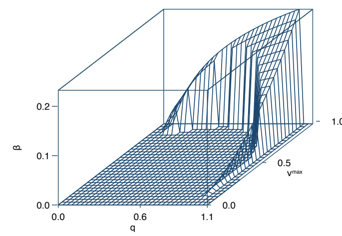
(c) Retaliation is possible, but victims cannot seek shelter



(d) Shelter decreases the probability of return $q_s > q$, $g(\bar{\nu}) = g_s(\bar{\nu}) > f(\bar{\nu})$



(e) Shelter increases retaliation $q_s = q$, $g_s(\bar{\nu}) > g(\bar{\nu}) > f(\bar{\nu})$



(f) Shelter increases retaliation but decreases the probability of return $q_s > q$, $g_s(\bar{\nu}) > g(\bar{\nu}) > f(\bar{\nu})$

the three parameters would fall under the mapping.

This begs the question, what are the characteristics and constraints of individuals who are near the "corners?" How are these individuals affected by shelter capacity changes differently than people near the center? I will compare two distinct corner cases for purposes of exposition to compare the relative differences. If we first consider the back right and bottom-most corner, an individual with a draw at that point on the distribution has a high maximum daily violence level $\bar{\nu}$, a low probability of returning to their abuser $(1-q)$ and a low cost of leaving β. This might describe someone who lives in a home with firearms (a risk factor for lethal violence) but who is well-connected to their social circle and community and therefore can reach out for help. Conversely, a person with a high probability of returning to the abuser, a low maximum daily violence, and a high cost of leaving might be someone without strong social ties or employment options, making it more likely they will stay with the abuser in the short-run.

2.3.2 Changes to capacity

Next, I consider a case where a shelter already exists in the area, but this shelter has changed its capacity and service availability. The most simple case is that where complementary service availability/quality is unchanged (i.e., increases to services are proportionate to the increase in capacity). If capacity increases and there is no change to per-client service availability, then there is no change to the individual's choice to go to a shelter, as $q_s^0 = q_s^1$.

How does this change lethal DV in the aggregate? Following anecdotal evidence from shelter employees, I assume that the representative shelter is operating at capacity both before and after the change. If more individuals go to the shelter, but the probability of returning to the abuser does not change, then more victims will return to their abusers.

Proposition 3 (P3): *Increases in capacity without changes in per-victim service availability will increase the DV homicide rate.*

Increases in capacity may reduce per-client service availability. This could take the form of a shelter buying a new bed, but not increasing staffing or counseling hours. If capacity increases but per-client services decrease, victims will be less likely to use the shelter. Victims who do use the shelter will be more likely to return to their abusers ($q_s^0 > q_s^1$) and experience high-violence events. These changes may make shelters more uncomfortable for victims, yielding higher costs to leaving ($\beta_s > \beta$).

Proposition 4 (P4): *Increases in capacity that decrease per-client service availability will increase the DV homicide rate.*

Finally, increases in capacity may come with an increase in per-client service availability. This could occur if a shelter organization moves to a new location that has more bed space as well as office space for an on-site counselor. Capacity increases will therefore lead more victims to come in and out of shelter in a given time period. However, improvements to shelter services will reduce the likelihood that a given client returns to their abuser ($q_s^0 < q_s^1$). These improvements may also yield a more comfortable experience for victims in the shelter, reducing the costs of leaving such that $\beta_s < \beta$.

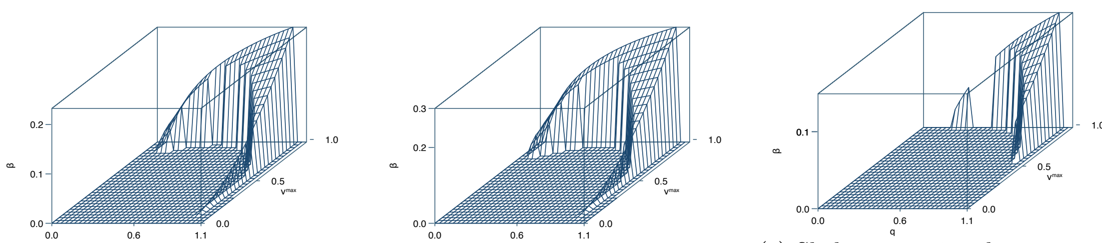
Proposition 5 (P5): *Increases in capacity that increase per-client service availability will have an ambiguous effect on the DV homicide rate.*

Note that the relationships in this subsection should also hold for capacity *decreases*, albeit in the opposite directions. If a shelter reduces its capacity and proportionately scales down its services, then fewer victims will come in and out of shelter. If a decrease leads to improved per-head services (e.g., a shelter employs a trauma-informed model that does not house multiple family units in the same room), then the effect would be ambiguous, as fewer victims would move through shelter, but those who did would have a lower probability of returning to the abuser.

2.3.3 Discussion of the model

Figure 2.3 shows the same mapping of the area of leaving under the case where capacity changes affect per-head service quality via a changed probability of return.[16] Compared to the benchmark case of no change to capacity/services (sub-figure 2.3a, replicated from sub-figure 2.2f), the area of leaving increases as per-head services increase (sub-figure 2.3b) and decreases as services decrease (sub-figure 2.3c).

Figure 2.3: The effects of shelter service capacity on the area of leaving. Mapping the area over which a victim will choose to leave as the area captured underneath the three-dimensional surface mapped. Illustrative coefficients and functional forms assumed.



(a) Benchmark shelter case, no change in capacity $q_s > q$, $g_s(\bar{\nu}) > g(\bar{\nu}) > f(\bar{\nu})$

(b) Shelter capacity change increases per-head services $q_s^1 > q_s^0$, $g_s(\bar{\nu}) > g(\bar{\nu}) > f(\bar{\nu})$

(c) Shelter capacity change decreases per-head services $q_s^0 < q_s^1 > q$, $g_s(\bar{\nu}) > g(\bar{\nu}) > f(\bar{\nu})$

Shelter-victim matching

The prior hypotheses assume that shelter targeting is efficient for victims at high risk of IPV and DV homicide. However, it is quite possible that there exists a mismatch between risk of lethality and shelter usage. In the framework described so far, this may take several different forms. First, if lethality is used as a means of control for individuals who have high costs to leaving (β), such as the case for both domestic and international migrants who may be far from family support networks or for victims who are unemployed and therefore cannot support themselves, then the constraint to leave will bind only for sufficiently high expectations of current and future violence. If the victim faces a high risk of return (low q) with or without

[16] Here, I assume $q_s^1 = .75q_s^0$ if $q_s^1 < q_s^0$, and $q_s^1 = 1.25q_s^0$ if $q_s^1 > q_s^0$.

shelter, as may be the case if the victim is unable to secure custody of children or in the case of an underage victim of abuse, then these victims may not seek shelter or leave in general due to fear of retaliation. Finally, if lethal IPV is less progressive than other forms of violence – namely, if lethal violence is not preceded by escalating non-lethal violence through repeated iterations – then the victim's expectation of the risk of violence and the risk of retaliatory violence ($f(\bar{\nu})$ and $g(\bar{\nu})$, respectively) will not be sufficiently high to prompt leaving.

If the individuals at the highest risk for homicide are not using shelter, then changes in local shelter capacity will have no effect on the incidence of homicide:

Proposition 6 (P6): *If individuals at the highest risk of homicide are not likely to use shelter, then changes to the locally available shelter capacity will have no effect on the local DV homicide rate.*

2.4 Data

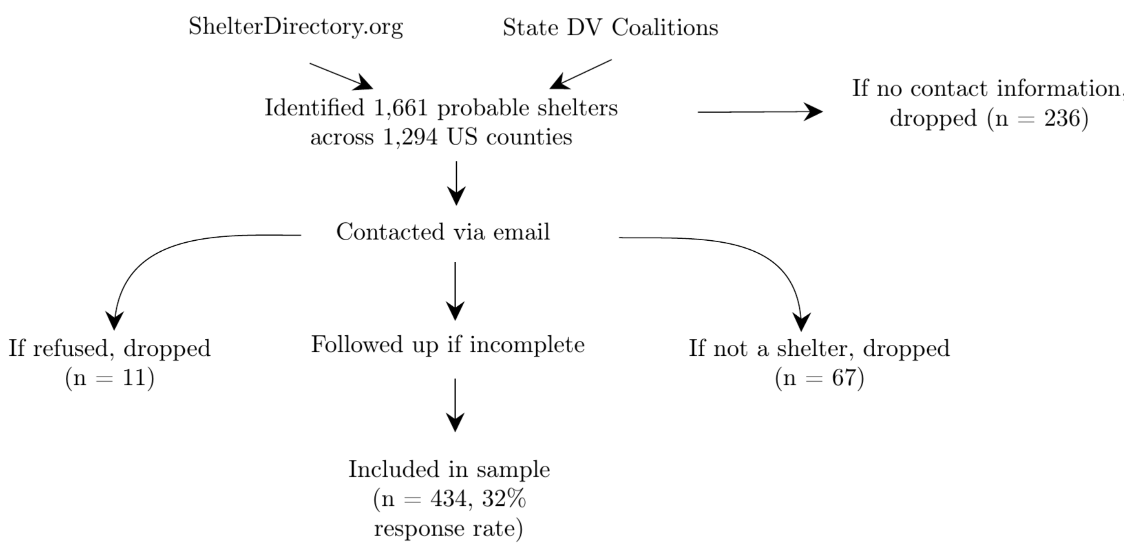
2.4.1 DV shelter data collection

Since the first US women's shelter was established in 1974, there has been an explosion in DV service providers nationwide (Twin Cities PBS, 2019). However, there is a lack of high-quality, longitudinal, fine-grain data on DV service providers, including emergency shelter providers. Incomplete data coverage of DV shelter services is often a result of funding source bias, data collection purpose, and data management issues (DeLeon-Granados and Wells, 2003). Collecting such data is difficult and requires contacting individual organizations, which is a time-intensive process.

In these sorts of resource-scarce data environments, empirical social scientists have a toolbox of ways to construct panel data after the fact. However, many of these common strategies are inappropriate for this particular question. As a few examples, researchers have found evidence of conflation of DV shelter bed space and homeless shelter bed space for women. This prevents economists and other empirical social scientists from using resources like tax records or business licenses to specifically identify DV emergency shelters. Further, organizing groups such as state domestic violence coalitions vary widely in the information they provide on in-state providers. The National Coalition Against Domestic Violence, the largest DV advocacy group, purges shelter directories every few years, preventing long-run analysis of shelter service availability (DeLeon-Granados and Wells, 2003). Organizations like DomesticShelters.org provide information on local shelter availability, but this information is often out of date or includes incorrect contact information.

As a result, there has been no unified data set describing changes in DV shelter capacity over time and

Figure 2.4: Data collection process



space. A major contribution of this project is the creation of such a database. Figure 2.4 describes the data collection process and relevant sample sizes. I manually collected contact information for 1,661 DV shelters across the United States from DomesticShelters.org and state domestic violence coalitions.[17] If contact information was listed on either of these two sites, I confirmed that the contact information listed was accurate. If no contact information was listed, I searched out this information. Any organization which I was unable to reach via email or phone was dropped (n = 236). When it was unclear whether or not the organization operated shelter services, I took the conservative approach and included the organization. Based on the information I was given by professionals in the industry, this exhaustive list of shelters is likely the most complete and updated list of probable DV shelters available.

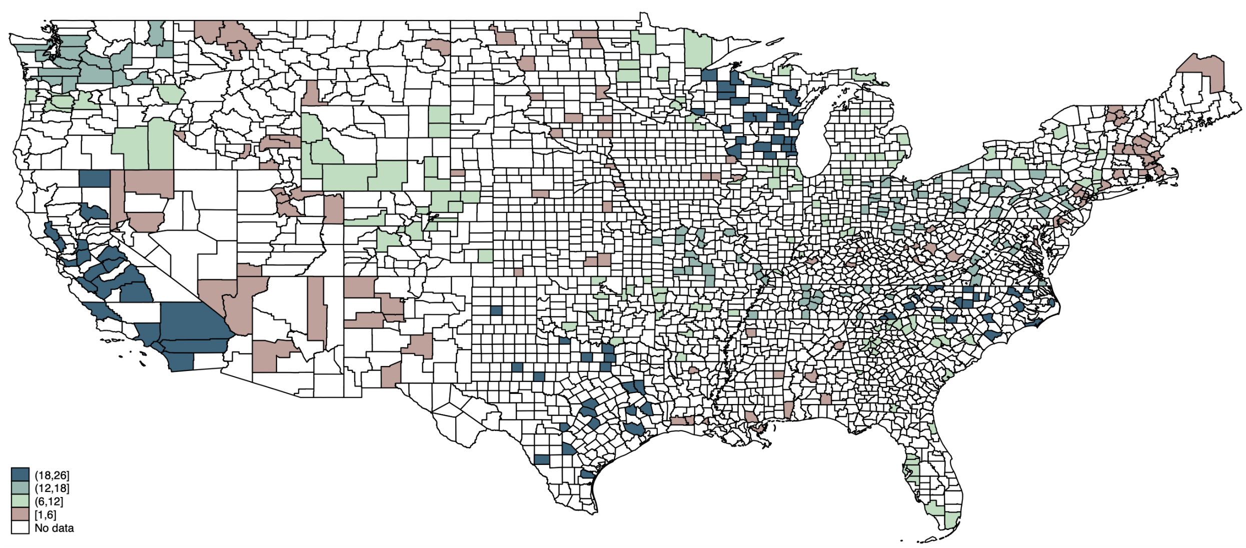
Shelters were contacted initially in February 2020, with follow-ups occurring through August 2020. Shelters were asked for information on the overseeing organization (including funding sources, target populations, and services offered), the history of shelter in the area (including the number of beds available, maximum number of days clients can stay in shelter, and shelter location), and how shelter can be accessed. Shelters were also asked for information about other current or former shelters in their area. This serves to both

[17] As a validation test, during April 2020, I compiled information on local public health departments for each of the 73 counties and 11 American Indian/indigenous groups in the state of Wisconsin. Departments were then emailed or called and asked what, if any, domestic violence shelters operated in their counties. Of the 84 counties/tribes contacted, 83 responded to this request for information. Only one public health department reported an operating shelter in their county that was not included in the original sweep of shelter organizations I identified (New Hope Shelter and Transitional Housing in Forest County, WI). This is suggestive evidence that the process by which shelters were identified was relatively thorough.

describe each shelter's effective network and to provide a pseudo-snowball sample of shelters that may have been missed in the initial search.

Of the contacted shelters, 556 responded to requests for information about their shelter's history by November 1, 2020, when I stopped actively collecting this data set. Of those, 419 provided complete information and are therefore included in the analysis. Eleven organizations contacted me to refuse to participate. I do not know whether these organizations do or do not offer shelter, so they are dropped from the analysis. Finally, 67 organizations contacted me to say that they were not and had never been a shelter. My analysis occurs at the level of the county, and some shelters have either moved counties or operate multiple shelter sites across different counties. As a result, the final data set includes 434 unique organization-counties. Among shelters that did have accessible contact information, the resulting sample represents roughly a 32% response rate, which is close to the mean response rate in the literature on web-based surveys (Shih and Xitao, 2008). Figure 2.5 maps respondents by county and state, where the color ramp is darkest for states with the most shelters. Respondents were distributed across the entire country, with the largest number of respondent organizations from California, North Carolina, Texas, and Wisconsin.

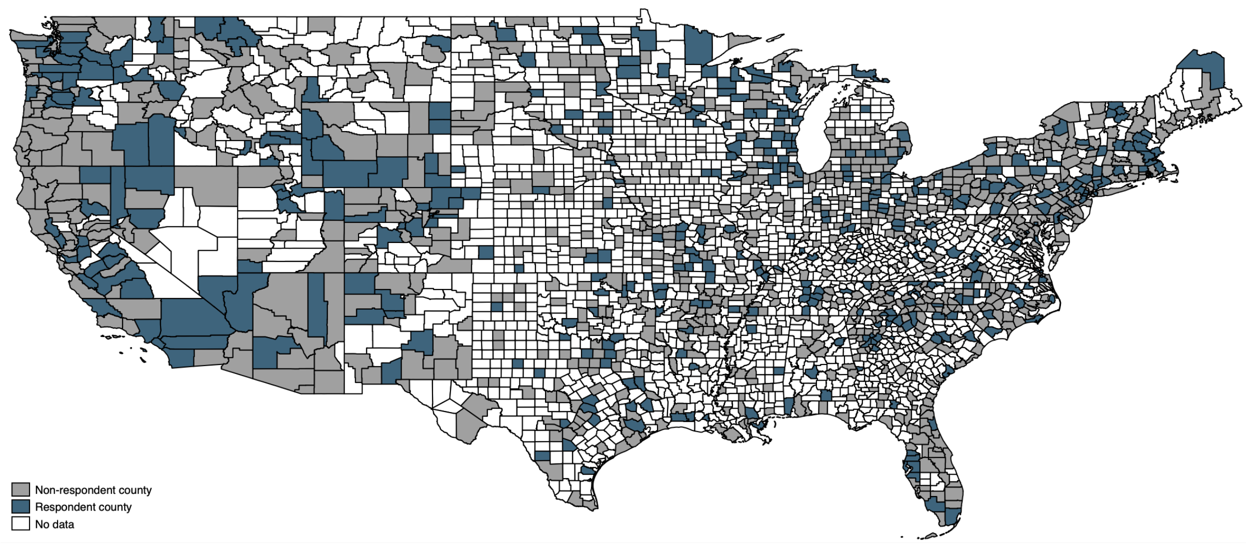
Figure 2.5: Map of counties included in the sample



Notes: Respondent counties included in the sample are shaded using the total number of respondent organizations in the state. Seven shelters in Alaska responded. Of the five identified shelters in Hawaii, none responded. Shelters in US territories were not included in the original shelter sample.

Sample selection, and response bias in particular, is a major concern in this work, and in this section I document the ways selection occurred and discuss strategies for accounting for this selection. Figure 2.6 maps counties identified as probably having shelter where a shelter did respond (dark blue), counties identified as probably having shelter where no shelter responded (dark gray), and counties for which I had no data on probable shelters (white). Some counties, such as Los Angeles County, had more shelters identified than actually responded. For the purposes of this analysis, I consider this as a within-sample county.

Figure 2.6: Map of counties with respondent shelters versus non-respondent counties identified as possibly having shelter



Notes: Respondent counties included in the sample are shaded in using the total number of respondent organizations in the state. Of the 21 county and county-equivalents in Alaska, 19 were identified as possibly having shelters. Of those, eight counties had a respondent organization and 11 did not have any respondent organizations. Of the five identified shelters in Hawaii, none responded. Shelters in US territories were not included in the original shelter sample.

Table 2.1 provides basic summary statistics on the respondent shelter organizations including the year they opened, funding sources and affiliations, services offered, and capacity as of spring 2020. The overwhelming majority of shelters were publicly funded (defined as receiving any funding from local, county, state, or federal sources), offered case management services, and operated a hotline. Few shelters that responded were or ever had been affiliated with American Indian/Indigenous organizations or religious groups.

It should be noted that this sort of retrospective survey is likely subject to recall error. (For a thorough review of the literature on short- and long-term recall bias, see Beckett et al. (2001).) DeLeon-Granados and Wells (2003) specifically find that self-reports from shelter employees to researchers carry substantial bias in terms of supporting documents that are retained over time and limitations on institutional memory. The data collected in this project is subject to such bias, but I account for it in the following ways. First, I reached out to organizations via email and allowed shelters to have the most appropriate person fill out the survey. Additionally, the survey was able to be closed and re-opened, allowing time for research. Shelters that filled

Table 2.1: Characteristics of respondent shelters

	Count	Mean	St. Dev.	Min.	Max.
Year opened	419	1992.31	10.41021	1984	2019
Publicly funded	419	.9594272	.1975342	0	1
Tribally affiliated (ever)	419	.0453461	.2083107	0	1
Religiously affiliated (ever)	419	.0286396	.1669908	0	1
Counseling services offered	419	.7947494	.4043675	0	1
Legal services offered	419	.5608592	.4968756	0	1
Case management offered	419	.9379475	.2415393	0	1
Operates a hotline	419	.9594272	.1975342	0	1
Transitional housing offered	419	.4319809	.495944	0	1
Capacity (2020)	419	26.65394	23.7319	0	181

Notes: Summary statistics are done at the organization level rather than the organization-county as the survey did not distinguish between shelter sites when asking what services the shelter offered.

out the survey with inconsistencies, incomplete information, or unclear information were contacted directly via phone or email to clarify the information provided. While this does not remove the possibility of the bias that DeLeon-Granados and Wells (2003) and Beckett et al. (2001) identify, it provides an improvement on previously existing data.

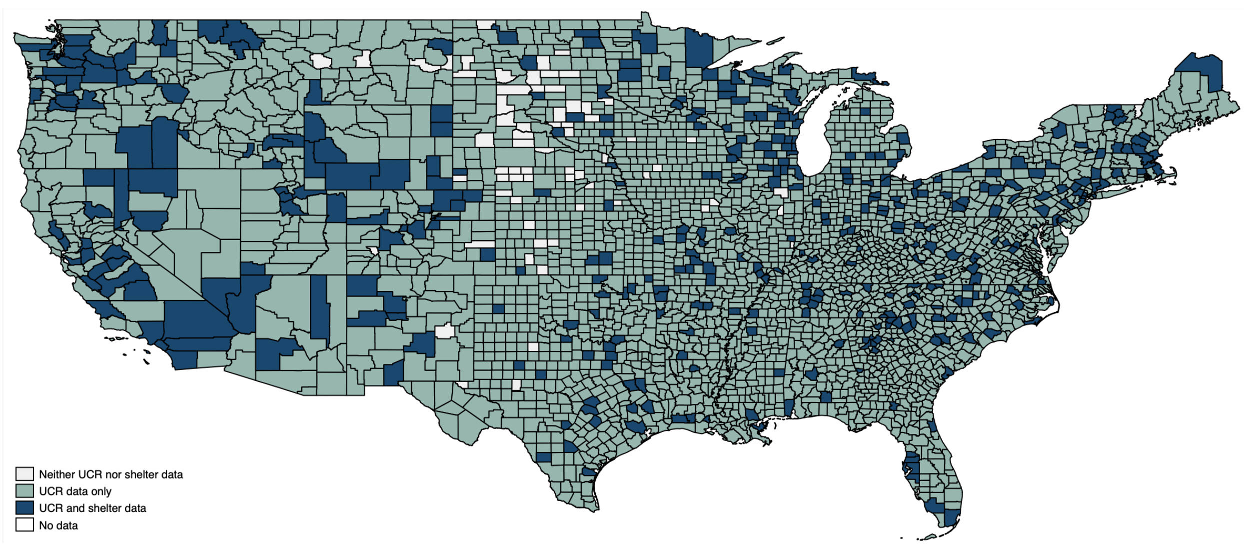
2.4.2 Outcome variables

IPV (and DV more broadly) encompasses a wide variety of behaviors and tactics, meaning that researchers must be intentional in choosing which measure of DV they study depending on the mechanism they are investigating. Self-reporting on DV and IPV requires a level of identification on the part of the survivor/victim that may complicate measurement (Ellsberg et al., 2001). On the other hand, a large fraction of calls made to police regarding domestic abuse are made by third parties, including neighbors. As a result, data on calls to police may over-represent households living in close proximity to others, such as those in apartment buildings (Ivandic et al., 2020). Following the recommendations in DeLeon-Granados and Wells (2003), in this work I use IPV/DV homicide as the primary outcome variable.

I use homicide reports from the FBI's Uniformed Crime Reporting Supplemental Homicide reports (UCR-SHR), aggregated by Kaplan (2019). The UCR-SHR data covers homicides reported between 1984 and 2019. Most relevant to this work, the UCR-SHR reports include detailed information on the relationship between victim and perpetrator, allowing me to define a homicide as having occurred among first- or second-degree family members (siblings, parents, grandparents, current/former spouses/partners). I use this broad definition when classifying homicides as being DV. I limit the scope to current and former dating partners/spouses when defining IPV. I am unable to determine the nature of the abuse dynamic between the victim of homicide and the offender. It is possible that the victim of homicide was the abuser of the homicide perpetrator, and thus the homicide may have been a form of self-defense. These events are therefore considered DV or IPV homicides, as they are homicides related to the perpetration of DV and IPV.

Figure 2.7 maps the overlap between counties included in the shelter sample and counties reported in the UCR-SHR data. Counties in dark blue are in both data sets, those in teal are only in the UCR-SHR data, and those in gray are missing from both data sets. There are no counties included in the shelter data that are missing from the UCR-SHR data.

Figure 2.7: Map of data intersection between shelter data and lethal violence data (UCR)

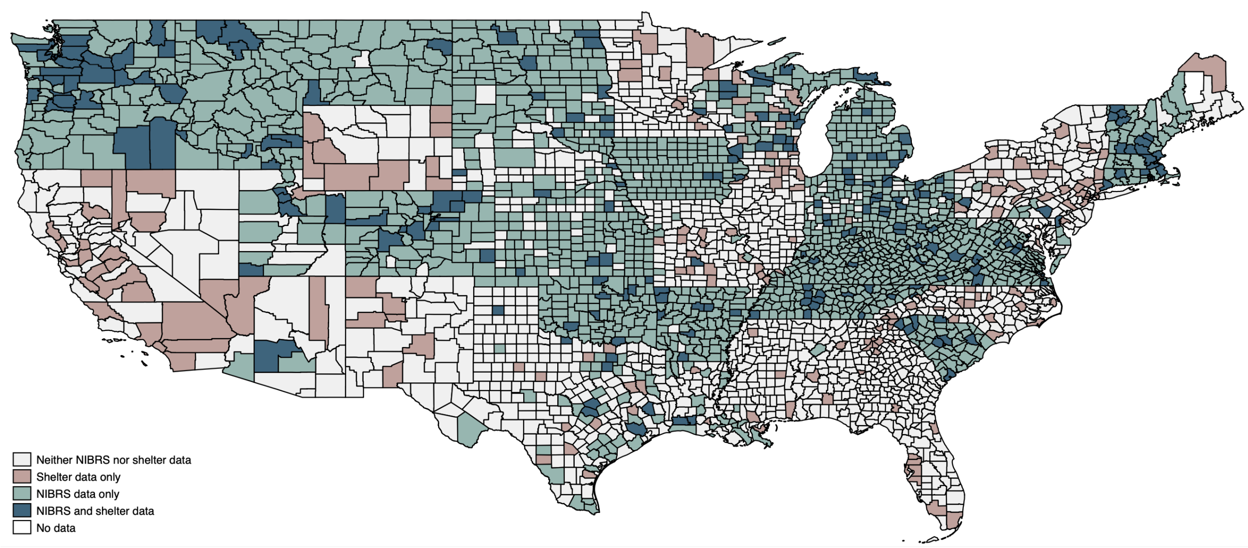


As discussed further in section 2.6.2, there are likely cases of DV and IPV that are not caught using this strategy. One particular concern would be IPV homicides of gender and sexual minorities whereby social stigma may prevent respondent officers, family members identifying bodies, or other individuals involved after the fact from classifying the event as having involved intimate partners. Further, I am unable to determine whether a homicide was the result of DV/IPV if the relationship between victim and offender does not meet the definition of DV/IPV. This will result in under-measurement of homicide deaths because I do not observe bystander deaths or police officer-involved deaths in the IPV/DV victim counts.

Though I focus on lethal forms of DV/IPV, I also present results using four types of non-lethal assault (all assault, aggravated assault, simple assault, and intimidation). These data are collected by the National Incident-Based Reporting System (NIBRS), which is an FBI-collected voluntary reporting system that covers up to 37 states. I only include agency-year observations where the agency reported to NIBRS in all 12 months of the calendar year. In the event that two or more agencies in a single county have different patterns of nonreporting, county aggregates of crime rates are weighted by the service population of the included agencies. Again, I am able to use the description of the relationship between victim and offender to define assault incidents as being IPV, DV, or neither. There are state-level differences in what constitutes different types of assault, but in general intimidation involves no physical harm whereas aggravated and simple assault may involve physical harm. In some jurisdictions, assault is upgraded to aggravated when a deadly weapon (e.g., a firearm) is involved.

Unfortunately for the purposes of this paper, the voluntary nature of the NIBRS data limit statistical power for analyzing the effect of bed capacity on different types of assault. Figure 2.8 maps the overlap between counties in the data I have collected on shelter capacity and counties included in the NIBRS data with all 12 months of the year reported. Counties which are in the shelter sample but not the NIBRS sample are in rose, counties in the NIBRS data but not the shelter sample are in green, counties in both the NIBRS data and shelter sample are in navy, and counties without NIBRS or shelter data are in light gray. The overlap is much smaller than with the UCR Supplemental Homicide Reports (figure 2.7), and so I cannot rule out insufficient power or sample selection as driving the (null) effects I find.

Figure 2.8: Map of data intersection between shelter data and non-lethal violence data (NIBRS)



2.4.3 Control variables

I include a series of control variables at both the state and county levels. Due to the extensive time coverage in the data collected for this paper, there is some difficulty in getting estimates of control variables going back as far as 1984. Because some control variables are only available starting in 1990, and because I control for the lagged homicide rate (excluding the IPV/DV homicide rate), the final sample includes 420 organization-counties between 1990-2019. Control variables were selected using identified risk factors in the literature on DV/IPV.

I use data from the Quarterly Census of Employment and Wages (collected by the US Bureau of Labor Statistics) to control for county-level average annual pay and county-level unemployment rate. County-level total SNAP issuance comes from USDA Food and Nutrition Service. State-level unemployment and political climate controls (including an indicator for whether the governor is a Democrat, the share of the state house that is Democratic, and the share of the state senate that is Democratic) come from the University of Kentucky Center for Poverty Research. I use the UCR data described above to control for lagged homicide rate less IPV and DV homicide. Klarner (2019) provided data for FIPS county matching. County-level population estimates come from the US Census Bureau. FIPS to CBSA matching was made possible by NBER. Finally, I use the Woods & Poole Complete Economic and Demographic Data Source (CEDDS) county-level annual projections. I include estimates of the white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than \$10,000; households with income \$10,000 to \$19,999; households with income \$20,000 to \$29,999; and households with income \$30,000 to \$44,999. For details on how these projections are estimated, see Woods & Poole Economics Inc. (2020).

2.4.4 Sample selection

Table 2.2 shows t-tests for the differences between mean outcome variables as well as controls by whether or not the county had a responding shelter. Respondent counties tended to be larger, have lower poverty rates, have higher annual average pay, and have higher homicide rates (normalized per 100,000 population) than non-respondent counties identified as having probable shelters. The differences reported in this table indicate that the generalizability of the results for the sample included in this paper to out-of-sample counties is limited. However, the shelters included within the sample do represent a substantial portion of the US population, and therefore I interpret these results in the context of the within-sample counties.

Table 2.2: Sample balance test (respondent counties vs. non-respondent counties)

	(1) Non-Respondent		(2) Respondent		T-test Difference
Variable	N	Mean/SE	N	Mean/SE	(1)-(2)
IPV Homicide	13605	0.903	5996	0.817	0.086***
		(0.016)		(0.021)	
DV Homicide	13605	1.459	5996	1.368	0.090**
		(0.021)		(0.030)	
IPV assault	3086	44.406	1460	39.719	4.686
		(2.332)		(3.023)	
DV assault	3086	62.719	1460	54.847	7.872
		(2.923)		(3.796)	
Population	13605	1.86e+05	5996	3.84e+05	-1.98e+05***
		(2140.888)		(11807.005)	
Poverty Rate	13605	13.922	5996	13.469	0.453***
		(0.028)		(0.043)	
County unemployment rate	13599	6.299	5994	5.987	0.312***
		(0.024)		(0.034)	
State unemployment rate	13605	5.826	5996	5.739	0.087***
		(0.015)		(0.023)	
Annual Average Pay	13605	31767.560	5996	33050.554	-1282.995***
		(91.684)		(141.479)	
Governor is Democrat (1=Yes)	13605	0.446	5996	0.435	0.011
		(0.004)		(0.006)	
Total SNAP issuance	13605	1.92e+06	5996	4.07e+06	-2.16e+06***
		(32000.748)		(1.57e+05)	

The values displayed for t-tests are the difference in means across groups. Single asterisk (*) represents significance at the 10% level; two asterisks (**)represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. The sample size for IPV/DV assault is smaller than the sample with homicide data, for reasons discussed in section 2.5.5.

2.4.5 Summary statistics

Table 2.3 presents a similar balance table comparing the outcome and control variables means for those counties which had capacities above the median number of beds per 100,000 population. (Medians per capita are defined by observation year to adjust for potential trends in shelter size over time.) Counties with above median per capita beds are in larger counties with higher incomes and more unemployment. With the exception of DV assault, counties with more beds per 100,000 population tend to have less IPV/DV crime, though this relationship is not statistically significant in general.

The variation of interest is changes in shelter capacity over time. Figure 2.9 plots the magnitude of different changes in shelter capacity observed in the data. (This figure does not include observations where there was no observed change in shelter capacity.) As shown, there are many changes to capacity in the data I have collected, from small, marginal changes of less than five beds to major increases of over 100 beds that

Table 2.3: Sample balance test (counties above and below the median beds per 100,000 population)

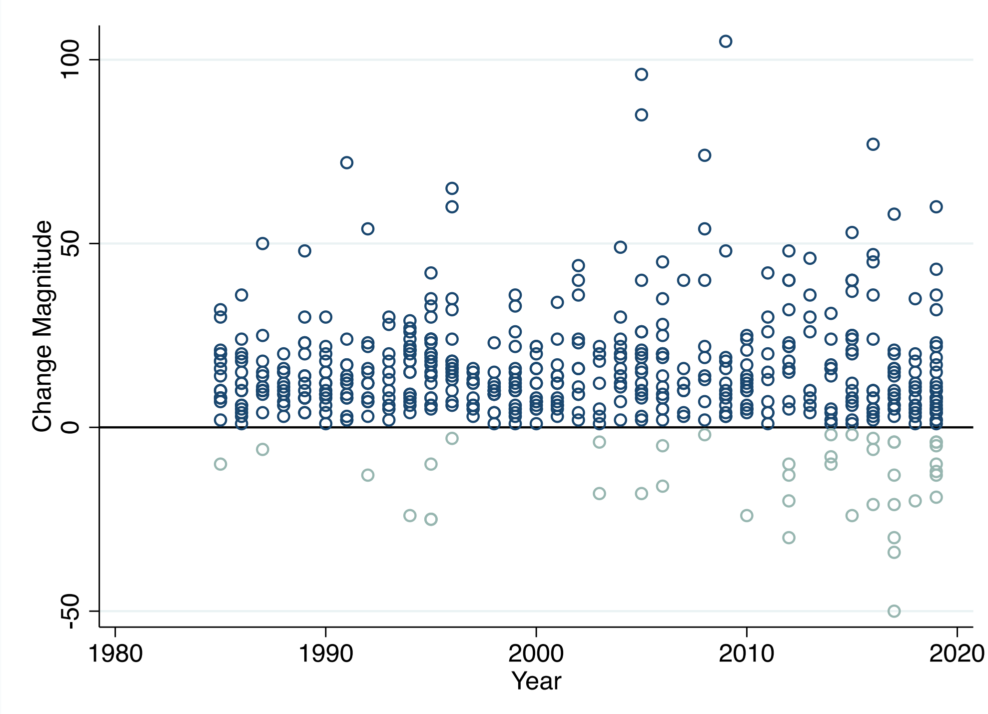
	(1) Above the median		(2) Below the median		T-test Difference
Variable	N	Mean/SE	N	Mean/SE	(1)-(2)
IPV Homicide	2989	0.804	3005	0.831	-0.027
		(0.030)		(0.029)	
DV Homicide	2989	1.317	3005	1.419	-0.102*
		(0.045)		(0.041)	
IPV assault	746	39.365	714	40.089	-0.724
		(4.374)		(4.164)	
DV assault	746	55.349	714	54.322	1.026
		(5.441)		(5.288)	
Population	2989	6.55e+05	3005	1.15e+05	5.40e+05***
		(22495.726)		(2438.241)	
Poverty Rate	2989	13.527	3005	13.408	0.119
		(0.059)		(0.062)	
County unemployment rate	2989	6.160	3005	5.815	0.345***
		(0.049)		(0.046)	
State unemployment rate	2989	5.957	3005	5.522	0.435***
		(0.034)		(0.031)	
Annual Average Pay	2989	35781.263	3005	30334.439	5446.825***
		(226.731)		(154.661)	
Total SNAP issuance	2989	7.07e+06	3005	1.09e+06	5.98e+06***
		(3.03e+05)		(30641.673)	
Governor is Democrat (1=Yes)	2989	0.448	3005	0.421	0.026**
		(0.009)		(0.009)	

The values displayed for t-tests are the difference in means across groups. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. The sample size for IPV/DV assault is smaller than the sample with homicide data, for reasons discussed in section 2.5.5.

likely involved a significant expansion or move to a new site. There are many more cases of shelter capacity increases than decreases, which likely reflects survival bias, as shelters that have closed are less likely to be identified with these sampling strategies (and much less likely to respond to a web survey). Note also that there are more shelters entering the market in the earlier part of the sample and more shelters exiting the market after 2000 (represented by negative changes). This is consistent with patterns of shelter openings and closures in the literature, as there was a major push to increase shelter availability in the 1980s and following the Violence Against Women Act.

However, this figure also reflects one of the major data limitations in my sample: survival bias. It is quite likely that there are unobserved shelters in the counties I study that entered and exited without appearing in the data. The figure demonstrates that I am unable to identify shelter closures, especially early in the sample. This data limitation arises because there is not an easily identifiable contact person to offer information about the history of shelters that have been closed for years or decades. In section 2.5, I discuss the plausibility of exogeneity given this data limitation in more detail.

Figure 2.9: Magnitudes of capacity changes by observation year



2.5 Methodology and results

In this section, I describe a series of empirical estimation strategies to test for a causal relationship between shelter bed capacity and lethal DV/IPV. I first show the results of a straightforward ordinary least squares (OLS) estimation using two-way fixed effects (subsection 2.5.1). I supplement my primary specification of interest with a model using a longer time horizon (subsection 2.5.3), which allows me to look at how patterns of change in service availability over time (including multiple changes in a short period of time) affect the incidence of lethal DV/IPV. To further disentangle the effects of changes in capacity over time, I estimate a model using a limited sample of shelters that experienced increases in capacity (subsection 2.5.2). I then discuss the applicability of these methods to measures of non-lethal violence (subsection 2.5.5). I close by describing a series of robustness tests, provided in appendix B.3.

2.5.1 Baseline OLS estimation

I first estimate a baseline, naive OLS regression of binary shelter presence on DV and IPV homicides using the following difference-in-differences specification with two-way fixed effects.

$$\mathrm{Y}_{c,t} = \alpha + \beta_1 \mathbb{1}\{\text{Shelter present}\}_{c,t} + X_c\delta + \gamma_c + \lambda_t + \epsilon_{c,t}. \tag{2.5}$$

Here, $\mathrm{Y}_{c,t}$ is the outcome variable of interest, either IPV or DV homicides per 100,000 population in county c and year t. The primary variable of interest is an indicator equal to 1 if the county had a shelter in that year. X_c is a vector of county- and state-level controls, including lagged homicide excluding the measure of homicide of interest (e.g., all homicides in a given county-year minus all IPV homicides in that county year, normalized for county population). I include county (γ_c) and year fixed effects (λ_t). I also provide specifications with state-by-year fixed effects. Following Abadie et al. (2017), standard errors are clustered at the level of treatment, in this case the county.

The identification strategy rests on the assumption that, after controlling for state and county characteristics (including the lagged non-IPV or non-DV homicide rate) as well as county and year (or county and state-by-year) fixed effects, the error term is orthogonal to the IPV or DV homicide rate. One potential violation of this assumption would be if DV/IPV homicide clusters spurred construction of new shelters (resulting in reverse causality). However, because these homicides are relatively rare events (with a dependent variable mean of 0.8-1.3 incidents per 100,000 population), it is unlikely that there is a contagion effect among lethal IPV/DV whereby prior IPV/DV homicide both instigates a shelter's construction (or expansion) and future violence. I discuss the plausibility of different threats to causal interpretation further in section 2.5.7.

Results are presented in table 2.4. I find that shelter presence is negatively associated with both IPV and DV homicide, on the order of a roughly 25% decrease in IPV homicide incidents. The relationship is inconsistently statistically significant. This estimation indicates that there is a relationship between DV service availability and homicide, but mechanisms are unclear. Using this variable allows me to estimate an easily interpreted relationship (e.g., counties with shelters had up to 25% fewer IPV homicides). I use a similar approach including those counties where I was unable to identify any probable shelters as a control group (excluding co-linear county fixed effects) and find similar effect sizes (see appendix table B5). Using that sample, effects remain statistically significant across IPV and DV.

However, there are several limitations to this estimation strategy that preclude causal interpretation. While it is plausible that shelter entry and exit are exogenous to the decisions made by other shelters in

Table 2.4: OLS regression of shelter presence on IPV or DV homicide rate

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) DV homicide	(4) DV homicide
Shelter in county	-0.191**	-0.153	-0.166	-0.149
	(0.0909)	(0.102)	(0.116)	(0.137)
Homicide (not IPV) = L,	0.0858***	0.0696***		
	(0.0248)	(0.0177)		
Homicide (not DV) = L,			0.182***	0.146***
			(0.0673)	(0.0439)
Observations	4,890	4,738	4,890	4,738
R-squared	0.296	0.442	0.379	0.519
County FE	X	X	X	X
Year FE	X	-	X	-
State x Year FE	-	X	-	X
Controls	X	X	X	X
Standard Errors	County	County	County	County
Outcome mean	0.790	0.794	1.311	1.320

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

the area (because there is excess demand), survival bias means that I am unable to definitively say that the effect identified here is a direct result of a single shelter's entry or exit, rather than the marginal shelter's entry or exit. This estimation also relies on variation in shelter openings and closings, which is biased toward the early part of the sample. Finally, it is unclear whether this result is an effect of the housing piece of shelter (that is, the ability of victims to stay overnight) or the general effects of having an organization that provides DV services in the community.

To improve the causal interpretability of the estimation and begin to assess mechanisms, I next look at the intensive margin, that is, changes in shelter capacity in beds per 100,000 population. This independent variable allows me to identify variation in both shelter openings/closures and changes to capacity during the shelter's life cycle. Because the survey is retrospective and aggregated to the year, I am unable to distinguish between a shelter capacity change on January 1 of a given year and December 31 of the same year. I therefore control for a one-year lag of per capita bed counts.

Here, the identification rests on the assumption of orthogonal error terms after controlling for state/county characteristics and fixed effects. Further, it assumes that, if there are unobserved shelters operating in the county during the sample period, then changes to capacity in the observed shelters are orthogonal to the behavior of unobserved shelters, again after including controls and fixed effects. The estimation strategy is as follows:

$$\mathrm{Y}_{c,t} = \alpha + \beta_1 \mathrm{Beds}_{c,t} + \beta_2 \mathrm{Beds}_{c,t-1} + X_c\delta + \gamma_c + \lambda_t + \epsilon_{c,t}. \tag{2.6}$$

Results for this estimation strategy are presented in table 2.5. I find a small, negative relationship between bed counts and IPV homicide. A contemporaneous single per-capita bed count increase corresponds to a per-capita IPV homicide rate decrease of roughly 0.005 at the county level. The coefficient for DV homicide is similarly small but statistically indistinguishable from zero. Further, this estimate is relatively precise, with standard errors tightly clustered near zero and comparable in size to (or smaller than) the estimated coefficients themselves. If I take an extreme approach and consider the potential true effect as outside of the 95% confidence interval, these results are still not economically significant. I discuss this further and compare the effects to other effects identified in the literature in section 2.6.1. The inclusion of the one-year lagged independent variable (Bed count = L) substantially improves the R-squared, but does not meaningfully change the coefficient estimates or the statistical significance.

2.5.2 Effects over time

The prior identification strategy assumes a linear relationship between bed counts and DV homicides, which may not be true. Particularly, there is reason to believe that the effects of increases are different from closures/capacity decreases, and that this effect largely depends on the scale of the change. Additionally, they do not consider the long-term effects of these changes. It is likely that, over time, individuals who might seek shelter will adjust their behavior to utilize the new shelter space.

To address this, I next estimate a model that draws on the intuition of regression discontinuity and event studies. This model estimates separate trends before and after the observed change in capacity, thereby allowing for a non-linearity in the pattern of homicide at the time of the discontinuity. I am therefore able to conserve statistical power while looking at a broader time horizon before and after the capacity change. This strategy is distinct from an event study as it does not estimate individual coefficients for each year bin, but simply non-linear before and after trends. This is to account for the relatively limited sample size in this restricted sample.

Table 2.5: OLS regression: per-capita bed counts on homicide (county-level)

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) IPV homicide	(4) DV homicide	(5) DV homicide	(6) DV homicide
Bed count	-0.00482		-0.00481	0.00473		0.0153
	(0.00398)		(0.00618)	(0.00417)		(0.0101)
Homicide (not IPV) = L,	0.0861***	0.0860***	0.0699***			
	(0.0248)	(0.0248)	(0.0175)			
Bed count = L,		-0.00324	0.000489		0.00234	-0.0113
		(0.00397)	(0.00631)		(0.00415)	(0.0105)
Homicide (not DV) = L,				0.182***	0.182***	0.145***
				(0.0676)	(0.0674)	(0.0435)
Observations	4,986	4,986	4,834	4,986	4,986	4,834
R-squared	0.296	0.295	0.439	0.377	0.376	0.515
County FE	X	X	X	X	X	X
Year FE	X	X	-	X	X	-
State x Year FE	-	-	X	-	-	X
Controls	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County
Outcome mean	0.794	0.794	0.798	1.315	1.315	1.324

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts, lagged bed counts, lagged dependent variables, and lagged homicide, are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

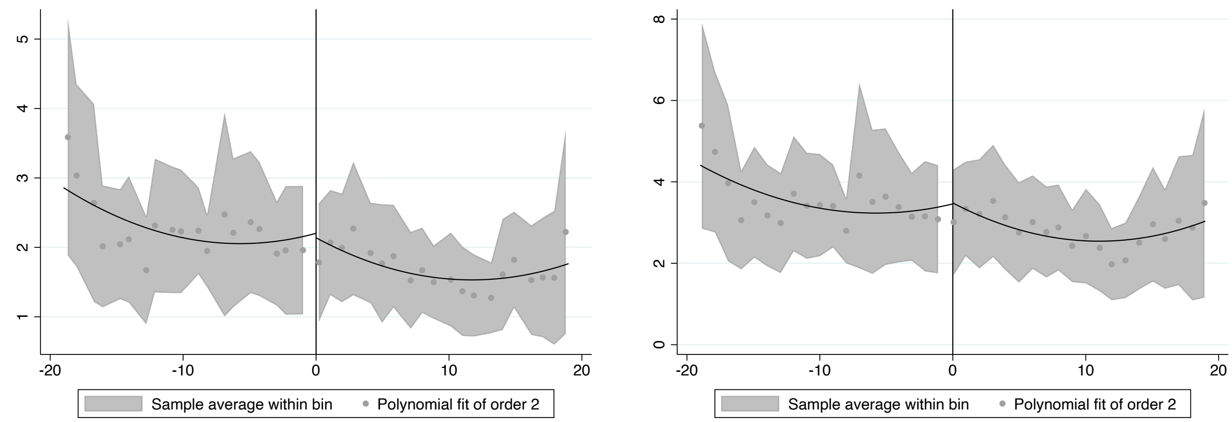
I limit the sample to those shelters that experienced a positive change in shelter capacity and look at only the first observed change in my data. I estimate the following:

$$\begin{aligned} \mathrm{Y}_{c,t} =& \alpha + \beta_1 \mathbb{1}\{\text{Change year}_{c,t}\} + \beta_2 f(\text{Running Var.}_{c,t<0}) + \beta_3 f(\text{Running Var.}_{c,t>0}) \\ &+ \beta_4 \text{Beds}_{c,t} + X_c\delta + \gamma_c + \lambda_t + \epsilon_{c,t} \end{aligned} \tag{2.7}$$

where the running variable is the number of years since the increase in shelter capacity, and the trends for years prior and years after changes are different. Rather than looking at increases, decreases, and small changes to shelter capacity (and thereby coming up against statistical power limitations by splitting the effects of large and small changes), this approach uses a single type of change (increases) and a single occurrence of changes in a given county. The functional form of the running variables is a second-order polynomial. For this specification, I dropped counties with more than one change in service capacity. I limit the bandwidth to 20 years before and after the change.

The primary specification of interest is shown in figures 2.10a and 2.10b. Here, there is a negative effect on the outcome variable of interest at the discontinuity (the year of the shelter's capacity change). This estimation strategy assumes that the year of change is the relevant break to estimate. Further, it opens the door for increased omitted variable bias relative to the model presented in table 2.5 because it includes a longer time horizon. When considered in light of the two-way fixed effects specification (table 2.5) and the instrumental variables specification (table 2.7), the results of this specification suggest that there is little to no relationship between contemporaneous bed count changes and IPV/DV homicide.

Figure 2.10: Discontinuity over time specification (baseline): county-level IPV and DV homicide



(a) IPV homicide (county-level)

(b) DV homicide (county-level)

2.5.3 Net change in bed counts

To allow for multiple changes in a shelter's capacity over time, I look next at the effect of the net change in bed counts over time on DV and IPV homicide rates. This specification improves on that in equation (2.6) by accounting for long-term trends in capacity change, rather than simply the most recent change. Further, Cunningham et al. (2019) find that the strongest effects of a local change to service access (in their case, local roll-out of the Craigslist Erotic Services section, an improvement to sex worker safety) on female homicide victimization occurred over five years after the change. This suggests that the market may take time to find a new equilibrium following changes to the supply of safety-improving services. To address the idea that shelters and clients may be re-equilibrating after the change in bed capacity, I estimate the following regression:

$$\mathrm{Y}_{c,t} = \alpha + \beta \mathrm{Beds}_{c,t} + \mu \mathrm{Net\ change}_{c,t} + X_c\delta + \gamma_c + \lambda_t + \epsilon_{c,t} \tag{2.8}$$

where I further control for the running net change in bed counts over the preceding five or ten years. This sums up all changes made in this window, rather than simply accounting for contemporaneous changes or changes in the prior year. Results are presented in table 2.6. Again, the relationship between net changes in bed counts and homicide is inconsistent in both sign and magnitude, but remains close to zero with very small standard errors. There is no evidence that long-term equilibration is occurring such that the homicide rate decreases over time in response to earlier changes in the absolute bed count.

Table 2.6: 5- and 10-year running net change in bed counts on homicide (county level)

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) DV homicide	(4) DV homicide
Bed count	-0.00515	0.000383	0.00562	0.00232
	(0.00435)	(0.00401)	(0.00446)	(0.00520)
Net change in beds (5yr)	0.000570		-0.00312	
	(0.00174)		(0.00206)	
Homicide (not IPV) = L,	0.0863***	0.0832***		
	(0.0247)	(0.0250)		
Net change in beds (10yr)		-0.000852		-0.00169
		(0.00128)		(0.00211)
Homicide (not DV) = L,			0.182***	0.188***
			(0.0675)	(0.0678)
Observations	4,914	4,082	4,914	4,082
R-squared	0.296	0.313	0.378	0.413
County FE	X	X	X	X
Year FE	X	X	X	X
Controls	X	X	X	X
Standard Errors	County	County	County	County
Outcome mean	0.795	0.736	1.320	1.242

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts, lagged bed counts, and lagged homicide, are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; county-level unemployment; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

However, it is possible that bed counts are endogenous to prior changes in bed counts. For this reason, I leverage the net change variable as an instrument using the following first and second stage estimation strategy:

$$\text{Y}_{c,t} = \alpha + \beta_1 \text{Beds}_{c,t} + X_c\delta + \gamma_c + \lambda_t + \epsilon_{c,t} \tag{2.9}$$

$$\text{Beds}_{c,t} = \pi_1 + \pi_2 \text{Net Change}_{c,t-1} + X_c\pi_3 + \gamma_c + \lambda_t + \epsilon_{c,t}. \tag{2.10}$$

Here, I instrument for the current-period bed count (per-capita) with the lagged net change in bed counts (absolute). Net changes in bed counts are a running sum of capacity changes over the preceding five or ten years. Here, the exclusion restriction requires that net changes in bed counts (lagged) have no effect on DV or IPV homicide outside of the effect through contemporaneous bed counts. This assumption may be violated if increases in prior years allowed a shelter to hire another staff member, improving the quality of services in year t and decreasing DV/IPV homicide. Results are presented in table 2.7.

Columns (1) and (4) report the first stage (equation 2.10) and demonstrate the strength of the net change in bed counts as an instrument for contemporaneous bed counts. Columns (2) and (3) report the IV results using the 5-year net change as the IV, and columns (5) and (6) use the 10-year net change as the IV. When instrumenting for contemporaneous bed counts, the relationships between contemporaneous bed counts and the two kinds of lethal violence are small, inconsistent, and statistically insignificant. Again, the standard errors on bed counts when instrumented are tightly clustered around zero.

2.5.4 Heterogeneity analyses

I test for heterogeneity by state policy environment and contemporaneous shelter characteristics, but I am unable to test for heterogeneity by victim/offender characteristics. Because IPV and DV homicides are relatively rare events at the level of the county-year, and because NIBRS/UCR data on individual characteristics is inconsistently reported, I am unable to estimate heterogeneous treatment effects for homicide against women, people of color, or former intimate partners. Future work should consider other avenues for assessing how changes to shelter capacity differ across victim/offender characteristics.

Table 2.8 reports results of a regression interacting the primary independent variable of interest (beds per capita) with an indicator variable equal to 1 if a shelter in that county offered legal services (columns 1-4) or counseling services (columns 5-8) as of 2020. For there to be an effect of contemporaneous shelter service characteristics on homicide rates as far back as 1990, this specification assumes that there is some characteristic of shelter organizations that is constant over time. Otherwise, this interaction should attenuate

Table 2.7: IV regression of bed counts on homicide

VARIABLES	(1) Bed count	(2) IPV homicide	(3) DV homicide	(4) Bed count	(5) IPV homicide	(6) DV homicide
Net change in beds (5yr) = L,	0.372***					
	(0.0468)					
Homicide (not IPV) = L,	0.0941	0.0861***		0.0915	0.0850***	
	(0.0775)	(0.0249)		(0.0683)	(0.0247)	
Bed count		0.00257	-0.00107		-0.00199	-0.00211
		(0.00509)	(0.00712)		(0.00482)	(0.00698)
Homicide (not DV) = L,			0.182***			0.194***
			(0.0673)			(0.0670)
Net change in beds (10yr) = L,				0.343***		
				(0.0469)		
Observations	5,833	4,897	4,897	4,653	3,891	3,891
R-squared	0.812	0.103	0.163	0.887	0.113	0.185
Model	OLS	IV	IV	OLS	IV	IV
County FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County
Outcome mean	19.16	0.796	1.320	20.61	0.724	1.222

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. In the first-stage regressions in columns (1) and (4), the outcome variable is current-period bed counts. In the IV regressions in columns (2), (3), (5), and (6) the outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. In the IV specifications, the variable of interest, bed counts, is instrumented using either the lagged 5- or 10-year net change in bed counts. Bed counts and lagged homicide are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

to zero. The coefficient on per capita bed count remains statistically insignificant and small, on the order of the effects found in table 2.5. However, the presence of both legal services and counseling services are associated with reductions in the IPV and DV homicide rates, with larger effects for DV homicide. The interaction term between bed counts and service indicators are small and statistically insignificant. This is suggestive evidence that complementary services (or shelter "quality") are important correlates of IPV and DV homicide, but these effects do not increase in the size of the shelter.

Similarly, table 2.9 tests for heterogeneity by state laws/policy regarding DV offenses, including mandatory arrest laws, primary aggressor arrest laws, TPO gun ownership bans, and TPO gun relinquishment. Effects are small and statistically insignificant.

Table 2.8: OLS regression interacting bed count with shelter services offered

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) IPV homicide	(4) DV homicide	(5) DV homicide	(6) DV homicide
Bed count	-0.00592	-0.00643	-0.00243	0.00875	0.00607	0.0133
	(0.00587)	(0.00544)	(0.00682)	(0.00978)	(0.00945)	(0.0113)
Bed count = L,	0.00355	0.00433	0.000413	-0.00695	-0.00617	-0.0112
	(0.00516)	(0.00538)	(0.00638)	(0.00851)	(0.00920)	(0.0105)
Legal services offered		-0.0807			-0.262*	
		(0.0647)			(0.134)	
Legal services * bed count	-0.00404	-0.000892	-0.00439	0.00423	0.00780	0.00365
	(0.00765)	(0.00321)	(0.00786)	(0.00770)	(0.00864)	(0.00890)
Homicide (not IPV) = L,	0.0862***	0.0624***	0.0701***			
	(0.0247)	(0.0202)	(0.0174)			
Homicide (not DV) = L,				0.181***	0.122***	0.145***
				(0.0677)	(0.0465)	(0.0437)
Observations	4,986	4,844	4,834	4,986	4,844	4,834
R-squared	0.296	0.341	0.440	0.377	0.376	0.515
County FE	X	-	X	X	-	X
Year FE	X	-	-	X	-	-
State x Year FE	-	X	X	-	X	X
Controls	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County
Outcome mean	0.794	0.799	0.798	1.315	1.328	1.324

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) IPV homicide	(4) DV homicide	(5) DV homicide	(6) DV homicide
Bed count	-0.0125	-0.0101*	-0.00790	0.00371	0.000863	0.0149
	(0.0107)	(0.00565)	(0.00974)	(0.0117)	(0.00907)	(0.0118)
Bed count = L,	0.00379	0.00429	0.000523	-0.00703	-0.00708	-0.0113
	(0.00507)	(0.00533)	(0.00632)	(0.00856)	(0.00896)	(0.0105)
Counseling offered		-0.230*			-0.511***	
		(0.118)			(0.195)	
Counseling * bed count	0.00497	0.00409	0.00345	0.00832	0.0113**	0.000477
	(0.0110)	(0.00314)	(0.00894)	(0.00918)	(0.00550)	(0.00810)
Homicide (not IPV) = L,	0.0862***	0.0626***	0.0700***			
	(0.0247)	(0.0204)	(0.0175)			
Homicide (not DV) = L,				0.182***	0.122***	0.145***
				(0.0676)	(0.0463)	(0.0435)
Observations	4,986	4,844	4,834	4,986	4,844	4,834
R-squared	0.296	0.341	0.439	0.377	0.376	0.515
County FE	X	-	X	X	-	X
Year FE	X	-	-	X	-	-
State x Year FE	-	X	X	-	X	X
Controls	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County
Outcome mean	0.794	0.799	0.798	1.315	1.328	1.324

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts are not per-capita. Lagged homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table 2.9: OLS regression interacting bed count with state-level DV arrest/gun laws

VARIABLES	(1) IPV homicide	(2) DV homicide	(3) IPV homicide	(4) DV homicide	(5) IPV homicide	(6) DV homicide	(7) IPV homicide	(8) DV homicide
Bed count	-0.0132**	-0.00437	-0.00462	0.0113	-0.00458	0.00461	-0.00985	0.0115
	(0.00554)	(0.00852)	(0.00494)	(0.00926)	(0.00681)	(0.0109)	(0.00753)	(0.0135)
Bed count = L,	0.00325	-0.00465	0.00415	-0.00710	0.00120	-0.00728	0.00120	-0.00770
	(0.00433)	(0.00734)	(0.00498)	(0.00852)	(0.00637)	(0.0107)	(0.00629)	(0.0106)
Primary aggressor arrest * bed count			-0.0106	-0.000866				
			(0.00904)	(0.00818)				
Homicide (not IPV) = L,			0.0860***		0.0334**		0.0339**	
			(0.0249)		(0.0149)		(0.0148)	
Mandatory arrest * bed count	0.00513	0.0132						
	(0.00712)	(0.00846)						
Homicide (not DV) = L,				0.182***		0.0532**		0.0533**
				(0.0677)		(0.0213)		(0.0214)
TPO ban * bed count					-0.00185	0.0113		
					(0.00796)	(0.00791)		
TPO relinquishment * bed count							0.00635	0.000156
							(0.00802)	(0.0106)
Observations	5,957	5,957	4,986	4,986	4,091	4,091	4,091	4,091
R-squared	0.215	0.229	0.297	0.377	0.224	0.281	0.224	0.280
County FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
State x Year FE	-	-	-	-	-	-	-	-
Controls	X	X	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County	County	County
Outcome mean	0.817	1.369	0.794	1.315	0.762	1.265	0.762	1.265

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts are not per-capita. Lagged homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

2.5.5 Non-lethal violence

The prior sections have focused on lethal forms of violence because of the reduced measurement error issues. However, homicide is an extremely rare event in the scope of all violence. A reader may suggest that, instead of lethal violence, this work should assess how shelter affects the much more frequent forms of DV and IPV. Here, I present results using two-way fixed effects and discontinuity design models and discuss the challenges with non-lethal violence measurement that have led me to focus on lethal violence.

Table 2.10 uses the estimation strategy described in equation 2.1 to look at the effect of shelter capacity on four types of non-lethal IPV and DV assault (all assault, aggravated assault, simple assault, and intimidation) at the county level. Results are statistically insignificant, but are generally negative and small relative to the mean. At the county level, there is no pattern to the relationship between bed counts and assault rates (again normalized per 100,000 population).

Figure 2.11 presents a discontinuity over time design on DV and IPV assault at the county and agency

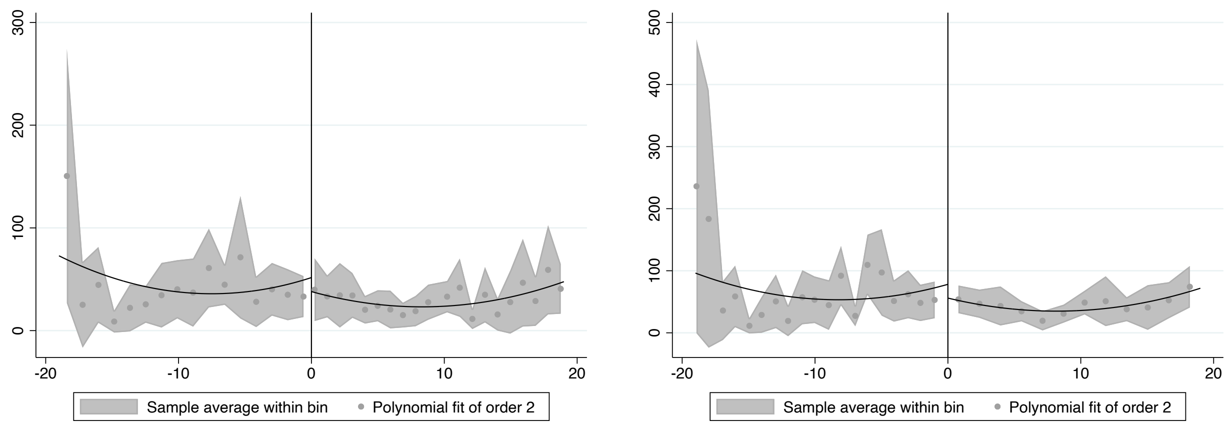
Table 2.10: OLS regression: per-capita bed counts on non-lethal IPV (county-level)

VARIABLES	(1) IPV assault	(2) IPV agg. assault	(3) IPV simple assault	(4) IPV intimidation	(5) DV assault	(6) DV agg. assault	(7) DV simple assault	(8) DV intimidation
Bed count	-0.0434	-0.00813	0.0280	-0.00193	-0.372	-0.0118	-0.149	-0.0232
	(0.306)	(0.0277)	(0.231)	(0.0163)	(0.444)	(0.0329)	(0.305)	(0.0232)
IPV homicide = L,	-0.0544	0.121	0.0767	0.0837				
	(3.188)	(0.147)	(2.739)	(0.181)				
IPV assault = L,	-0.0270							
	(0.0348)							
IPV agg. assault = L,		-0.0788***						
		(0.0181)						
IPV simple assault = L,			-0.0261					
			(0.0378)					
IPV intimidation = L,				-0.0851***				
				(0.0275)				
DV homicide = L,					2.777	0.173	1.887	0.312
					(2.631)	(0.202)	(2.143)	(0.307)
DV assault = L,					-0.0519			
					(0.0354)			
DV agg. assault = L,						-0.0911***		
						(0.0168)		
DV simple assault = L,							-0.0274	
							(0.0366)	
DV intimidation = L,								-0.125***
								(0.0359)
Observations	1,337	1,337	1,337	1,337	1,337	1,337	1,337	1,337
R-squared	0.164	0.111	0.164	0.120	0.165	0.107	0.158	0.170
County FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County	County	County
Outcome mean	40.91	1.169	22.39	1.103	56.80	1.766	30.15	1.759

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of incidents of IPV assault, sexual assault, or intimidation per 100,000 population. Bed counts, lagged bed counts, lagged dependent variables, and lagged homicide, are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

levels. The break designs are noisy and do not demonstrate a conclusive relationship between capacity and assault. However, further work should look into the effects of shelter on non-lethal violence, perhaps by using reports of violence in response to a survey.

Figure 2.11: Discontinuity over time specification: IPV and DV assault



(a) All IPV assault

(b) All DV assault

2.5.6 Robustness of the null result

Robustness tests are provided in appendix B.3. I show that the null results presented in the main text of the paper persist across a number of specifications, including aggregating to the census-based statistical areas (CBSA) or policing agency to address concerns about shelters' effective service areas, lagging the dependent variable, using an Arellano-Bond estimator to account for potential serial auto-correlation, using different measures of bed counts, and varying window width for the net-change specification.

I test for robustness for the RD specification using a running outcome variable, defined as the three-year average of the DV or IPV homicide rate (appendix figure B6); including counties that had no change observed in the data, which were assigned a false treatment date outside the scope of the data (chosen as five years after the panel ended, 2023) (appendix figure B7); and instrumenting for bed counts with indicator variable equal to one if the shelter experienced an increase in the observed year (appendix figure B8). All robustness tests show a small and negative effect on IPV after the discontinuity with the exception of inclusion of counties that did not experience changes. The effect on DV is inconsistently positive and not statistically significant.

2.5.7 Threats to identification

If shelter placement is endogenous to the crime rate, then this may result in reverse causality, whereby changes in the dependent variable of interest drive changes in the independent variable of interest. Specifically, if DV/IPV homicides are subject to contagion effects, where one homicide event precipitates others, then events prior to shelter construction may be serially correlated with events following shelter capacity changes. Because DV organizations take several years to fundraise and develop new shelters, this contagion effect of already rare events would have to span multiple years. For this reason, I am not concerned that endogenous shelter placement is driving my results after controlling for lagged crime rates.

Additionally, a reader may be concerned that other forms of abuse are being conflated with DV/IPV in such a way as to bias the results. Specifically, human trafficking is a form of abuse wherein victims are forced or coerced into performing sexual acts or other labor (National Human Trafficking Hotline, 2021). Human trafficking, while an extremely serious issue, involves fewer victims per year than DV/IPV. As an example, the National Human Trafficking Hotline reported 11,500 cases of human trafficking in 2019, while the National Network to End Domestic Violence reported roughly 20,000 calls for service in a single day in 2019 (National Network to End Domestic Violence, 2017, 2019). Further, the only homicides included as being DV/IPV in this study are those where the offender was identified as being a current/former intimate partner or close family member. As a result, lethal human trafficking where the trafficker is not a family member or intimate partner would not be included in the IPV/DV homicide count. However, if an individual is being trafficked by a family member or intimate partner and is killed by someone other than their abuser (e.g., by a john who is soliciting sexual services), then this homicide would not be identified in the UCR-SHR data as being DV/IPV. Additionally, it is quite possible that victims of trafficking are taking shelter bed spaces. Future work should look into how the different populations taking advantage of shelter space affect crime incidence outside of shelter.

2.6 Discussion

2.6.1 Contextualizing the effect

Ultimately, any reduction in homicide is a public and private good. Comparing negative coefficient magnitudes to determine what is an "economically meaningful" effect should be handled with care. Here, I contextualize the effect I find in four ways to assess the cost-effectiveness of increasing locally available bed capacity, in contrast to other policies and programs, as a means of reducing IPV homicide.

First, I compare the identified effect size to the mean dependent variable and mean shelter capacity change. In my sample, the average capacity change (conditional on the change being greater than zero) was 4.45 beds per 100,000 population. If the true effect size is $\beta \geq -0.008$, then a 4.5-bed increase in shelter capacity would correspond to 0.034 fewer homicides in that county in that year. The average homicide rate per 100,000 population in county-years with nonzero changes to bed counts was 0.888. Therefore, these effects correspond to 3.8% fewer IPV homicides. A change of 4.5 beds per 100,000 population may be a relatively affordable policy option if there is already a large shelter in the county or the county is not very populous. However, if this change requires building a new shelter, then this change will be much more expensive.

Second, I compare the effects I find to existing work that identifies an economically significant effect on homicide. Koppa (2018) examines the effect of lethality assessment programs (LAPs) in Maryland on female homicides perpetrated by men. LAPs work to nudge women who are at risk of homicide to seek services and create safety plans. Koppa finds that LAPs served to reduce the female homicide rate by 0.25-0.3 homicides per 100,000 population, representing a 37% to 44% reduction in the female homicide rate. This effect is one to two orders of magnitude larger than the effect I find of shelter capacity changes. As a second example, Cunningham et al. (2019) use the UCR-SHR to identify the effect of roll-out of the Craigslist Erotic Services Section (ERS) on female homicide. They find that ERS reduced the city-month female homicide rate per 100,000 by 0.013-0.019 homicides (a 10-15% reduction), corresponding to 0.156-0.228 fewer homicides in that city in one year. These effects are larger than those I estimate in a given county-year, conditional on population. Both of these papers indicate that programs that increase the *information* available to those at risk of homicide can yield much larger reductions in homicide than increasing shelter bed capacity, and the results in Koppa (2018) suggest that LAPs specifically may be a lower-cost way of effecting these changes than large changes to shelter capacity.[18]

A different approach is to compare an identified "economically meaningful effect" in the literature to the effect size proposed in this paper outside of the 90% confidence interval. If policymakers seek to reduce the absolute number of homicides by one and the average county in my sample has a population of 316,007, then this is equal to roughly 0.3 fewer homicides per 100,000 population. How does this compare to the 90% confidence interval bounds? For $\beta = -.00753$ and $\hat{\sigma} = .00593$, the 90% confidence interval is $[-.017, .0022]$.

[18] Other papers that identify the effects of policies such as alcohol tax rates and firearms restriction laws on homicide and IPV homicide include Durrance et al. (2011), Sabbath et al. (2020) and Zeoli and Webster (2010). I do not directly compare my results to the results in those papers because they use different measures of homicide risk.

A 4.5 bed change in capacity would therefore reduce the local homicide rate by 0.077 homicides per year, a much smaller effect that would equal one fewer homicide in the average city. This is also much smaller than the effect found in Cunningham et al. (2019) (0.156-0.228).

Finally, I use the inverse power calculation proposed by Andrews (1989). This method provides a way of answering the question,"For a given standard error size, what maximum coefficient value is not type II error at a 95% significance level?" If $\hat{\beta} = -0.00753$ and $\hat{\sigma} = .00593$ (the null result from column 6 of table 2.5), I can reject the possibility of a type II error (incorrectly failing to reject a null hypothesis) for a true effect size on IPV homicide of $\beta \geq -0.0214$ at the 95% significance level. Conversely, $\hat{\beta} = -0.0116\%$ falls within the area of high rate (50%) of type II error.

When taken together, the four strategies described above are evidence that the identified negative effect of increased bed counts on IPV homicide is 1) a small change relative to the average homicide rate, 2) a small change compared to other effects found in the literature, and 3) likely not a result of type II error.

2.6.2 Limitations

As described in section 2.4.1, there is cause for concern about selection in both the shelter sample identified and in the outcome variable of interest. While homicide itself is minimally subject to reporting bias relative to other forms of interpersonal violence (Daly and Wilson, 1988), there are still significant concerns for reporting bias and measurement error. There are likely cases of DV and IPV that are not caught using this strategy. One particular concern would be IPV homicides of gender and sexual minorities. There is historical variance in social acceptance and recognition of non-cisheteronormative relationships, including same-gender relationships and relationships involving one or more transgender partners. For this reason, one would be justifiably concerned that, intentional or otherwise, violence in these couples may not have been classified as relationship violence. Recall error from shelters is particularly concerning regarding small, marginal changes. Researchers have found that there is the least recall error when events are highly salient and more recent (Beckett et al., 2001). In this context, this would mean that small changes of a few beds and changes farther in the past are less likely to be reported than, for example, a change where a shelter moved to a larger site.

Second, individuals who seek shelter do not represent the totality of victims of DV and IPV. In fact, clients in shelter are likely not a representative sample of victims, as these individuals tend to be especially resource constrained, have limited or no outside options, and are often emotionally taxed by their time in shelter (as opposed to staying with a family member) (Hamby and Gray-Little, 2007). One might worry that increases in DV shelter capacity lead to increased shelter utilization by non-victims of violence, particularly those who are not at risk of DV homicide. However, if people who are not at risk of DV/IPV are receiving services, this would bias my results downward. Future work should be done to assess how social support services can best access and aid individuals who are not likely to seek out shelter.

Finally, the sample of shelters identified is not representative of shelters in all US counties. Though the counties represented in my sample are statistically significantly different from non-respondent counties and those that do not have shelters (threatening generalizability), my sample represents a total population of 128,298,872 (as of 2018). For this reason, the sample identified in this paper is important to consider in and of itself. Future work should seek to understand how the addition of a DV shelter to counties that have never had such a shelter may impact the incidence of violence.

2.7 Conclusion

I find no consistent evidence of a relationship between shelter capacity changes and DV and IPV homicide. Despite some evidence that DV and IPV homicide rates decrease after changes to local capacity, the small magnitudes of these changes suggest that such changes are not significantly shifting patterns of lethal family violence. When compared to effect sizes found elsewhere in the literature and the substantial cost of making large changes to local shelter capacity, changes to shelter capacity in isolation do not seem to be a cost-effective policy mechanism to reduce local violence. However, sample selection limits generalizability to counties outside the sample included in this paper. The question remains open as to whether or not these changes significantly affect non-lethal forms of violence.

2.7.1 Policy implications

While this work offers direct policy implications, it is important to underscore what this work does *not* imply for policy supporting survivors of DV/IPV. Emergency DV shelters across the US house thousands of victims every day, and they offer broader services like counseling, legal assistance, forensic nurse examination accompaniment, childcare, transportation, and more to even greater numbers. The work done by these organizations genuinely changes lives and offers safety to people in some of the most vulnerable and dangerous moments of their lives. This paper should not be taken to imply that these organizations are not vital components of the broader network of social support services.

Rather, this work finds that, in isolation, marginal changes to DV emergency shelter capacity may not be an efficient policy tool for reducing one type of violence – DV/IPV homicide. The effects of small changes to capacity are close to zero and statistically insignificant, and even large changes requiring construction of new shelters are likely to be an infeasible policy tool. Opening new domestic violence shelters is a very costly process that often requires fundraising millions of dollars over multiple years, on top of annual operating costs of hundreds of thousands of dollars (Erickson, 2014; Morgan & Morgan Business Trial Group, 2016; The Family Center, 2021). Therefore, policymakers concerned with reducing the incidence of these homicides should look into whether other mechanisms and programs to reduce the homicide rate can do so more cost-effectively. Such mechanisms may include legal protections like temporary protective orders, changes to firearms laws, or increased education for communities with high rates of violence.

Additionally, it is possible that the individuals most likely to be killed by a partner or family member are not those who are accessing shelter. Policymakers, practitioners, and researchers should work collaboratively to broadly identify what characteristics of relationships and individuals increase the rates of homicide risk (e.g., access to firearms or romantic relationship separation) and whether these characteristics map onto the likelihood of seeking shelter. If there is a disconnect there, policymakers and practitioners should consider whether shelter is the best resource for victims in these relationships versus more efficient policy mechanisms to protect individuals at risk of lethal IPV/DV.

There is a particular concern with the high likelihood that an individual who enters a shelter will return to the abuser. If a shelter stay does not prevent a person from returning, it is possible that the insignificant relationship between shelter capacity and homicide reduction is a result of the limitations of the support that emergency shelters can provide. Because victims are only able to stay in shelter for a short time, shelters are limited in their ability to foster the necessary long-term support to prevent victims from returning. If this is the case, then shelters need resources to offer longer stays, partnerships with transitional housing, and increased services to break the cycle of violence.

2.7.2 Future research

Future research using these data should assess the heterogeneous effects of shelter beds across different policy contexts. Specifically, do increases in the number of shelter beds have different effects on violence against women in states with community property laws or in states with more or less restrictive gun control? Shelter capacity may affect violence differently in states with community property laws if such laws increase the bargaining power of the abused person in a marriage. How does this interact with gun restriction laws, as most murder-suicides involve firearms? Further, do policy changes requiring that shelters receiving federal funding be open to clients of all genders affect the rate of violent crime committed against men and gender non-binary individuals?

Similarly, social support services may affect the effectiveness of shelter programs. Diversion programs allow perpetrators of certain offenses to avoid jail time (often with charges dropped entirely) if they complete some sort of intervention program. In the case of DV diversion programs, these interventions can include counseling or batterers' intervention programs. Do states with violence diversion programs have fewer instances of retaliatory homicide? Do differences in who is incarcerated versus who is offered a diversion program (such as differences resulting from disparate sentencing for people of color) play a role?

It is likely that there is a difference between large and small changes in capacity, with the possibility that large changes correspond to significant differences in complementary services at the shelter, including access to legal services or counseling. Researchers should look into how changes in capacity correspond to changes in non-residential shelter services and the effect of these services on DV/IPV incidence. Qualitative research among a group of shelters may offer insight into how these organizations make decisions about expansion and services and the difficult choice sets they face.

Chapter 3

Pesticides increase pediatric cancer deaths: Evidence from Brazilian soy production

with Marin Elisabeth Skidmore and Holly K. Gibbs

Abstract

Brazil simutaneously became the world's leading soy producer and pesticide consumer. Despite the identified link between pesticide exposure and carcinogenesis, there has been little causal research on the effects of intensification and use of agrotoxins on human health in Brazil. We estimate the causal effect of expanded soy production – and related community exposure to pesticides – on childhood cancer incidence using 15 years of data on disease mortality. We find a statistically significant and positive effect of soy production on pediatric leukemia. We show evidence that agrotoxin exposure occurs via penetration into the water supply. This work underscores the need for stronger regulation of agrotoxins and increased public health attention to exposure in the broader community.

3.1 Introduction

Medical studies and correlative research have documented adverse health effects for those working directly with pesticides as well as those living in soy-producing regions (Recena et al., 2006; Soares and Porto, 2009).

Considering only the effects of direct exposure to pesticides (e.g., pesticide poisoning) underestimates the full cost for local health, as indirect, chronic, low-level exposure to some pesticides has long-term negative health outcomes (Lai, 2017). Despite the known exposure of the broader community to pesticides, there is a lack of causal empirical evidence on how chronic, low-level exposure from agricultural pesticides affects public health broadly.

Brazil is the world's leading soy producer, producing 124,845 million tons of soy in 2019 after surpassing one million tons in only 1969 (EMBRAPA Soja, 2020). This dramatic increase was accomplished by a combination of expansion of cropped area (from 10.5 million hectares in 2000 to 36.3 million hectares in 2019) (Rausch et al., 2019; MapBiomas, 2020), development of new varieties suitable for tropical soils (Assunção and Bragança, 2015; Bragança, 2018; Rada, 2013), technology adoption led by a federal electrification campaign (Assunção and Bragança, 2015; Assunção et al., 2017), and increased use of pesticides (a term we use here to refer broadly to the class of chemical inputs that control pests in agriculture, such as fungicides, herbicides, and insecticides (US EPA, 2021b)). Pesticide use increased five-fold in twenty years, from 113,000 tons in 1997 to 540,000 tons in 2017 (Dasgupta et al., 2001; Gonzales, 2020; Melo, 2019). Brazil is now the world's leading pesticide consumer, with half of these chemicals rated as Highly Hazardous (Gonzales, 2020). In one study, researchers found that the majority of non-agricultural workers living in a surrounding community of a leading soy-producing region of Mato Grosso state had elevated levels of pesticide compounds in their blood and urine samples (Belo et al., 2012). Pesticide residue has also been detected in surface water sources in Brazil (Belo et al., 2012; Dores et al., 2008). The direct and indirect effects of extensification of soy cultivation have been widely studied (Gibbs et al., 2015; Rausch et al., 2019), but less is known about the effects of intensification, specifically the increase in use of pesticides.

We provide the first causal analysis of indirect exposure to agricultural pesticides and cancer, and the first such study at scale. We use 15 years of nation-wide panel data to study childhood cancer mortality through the expansion of soy and pesticides through Brazil's Cerrado and Amazon biomes. This panel data consists of municipal-level counts of cancer deaths by age and diagnostic code from 2004-2019. We pair this with municipal-level data on soy cultivation and a series of state- and municipal-level controls. We then use a difference-in-differences estimation strategy assuming orthogonality of soy expansion and cancer diagnosis after controlling for economic and demographic trends.

We focus on childhood blood-borne cancers – specifically, acute lymphoblastic leukemia (ALL) – the most common childhood cancer. In cases of ALL, genetic mutations cause bone marrow to over-produce immature cells that develop into leukemic lymphoblasts, crowding out healthy white blood cells. ALL is diagnosed most commonly in children under age 10 and can generally be accomplished using a blood test or bone marrow biopsy. Treatment usually consists of chemotherapy, radiation, and bone marrow transplant, requiring regular visits and care for up to several years after diagnosis. Important for our causal estimation, ALL is quickly fatal without treatment, reducing endogeneity of survival bias beyond age 10 (Mayo Clinic, 2020).

Limiting analysis to ALL both accounts for the confounding effects of age and the inherently multifactorial risk for solid tumor development. Carcinogenesis is a complex process, and the necessary genetic mutations can come from many sources. Cancer development risk increases with age, exposure to radiation, diet, alcohol use, and tobacco use (Cancer Treatment Centers of America, 2018; Jolly and Van Loo, 2018; Lecca et al., 2015). Detection is key to effective and timely treatment, but methods of detection vary by cancer type. Unlikely solid tumors, many “liquid” tumors, including ALL, can be diagnosed using blood tests (Mayo Clinic, 2018, 2020). In contexts where access to medical care is limited (such as in the Brazilian Amazon and Cerrado biomes) detection of “liquid” tumors remains feasible.

We find a positive and significant effect of soy production on pediatric ALL. We find no evidence that less input intensive row crops increase deaths from ALL. This rules out economic or lifestyle changes associated with crop agriculture as the mechanism through which soy expansion leads to cancer. The effect of soy expansion is stronger when we include all soy production in the watershed. Our results support our hypothesis that pesticide exposure is the cause of the deaths and that a primary source of exposure is through contaminated water supply.

3.2 Background

3.2.1 Soy expansion in the Amazon and Cerrado biomes

Under Brazil’s military dictatorship (1964-1985), expansion of the agricultural frontier into the Amazon and Cerrado became a major national goal for reasons of national security and to reduce pressure on land use in other regions (Assunção and Bragança, 2015; Smith, 1981). Following early expansion of roads, the cropped area in the Cerrado rose, but production remained low, as the seed varieties used in the South were unsuited for the soils and climate of the northern biomes (Assunção and Bragança, 2015; Pfaff et al., 2007). Starting in the 1970s, researchers at the Brazilian Agricultural Research Agency (Embrapa) and Brazilian universities developed soy varieties that could produce in the acidic soils and frequent droughts of the Cerrado and the Amazon (Rada, 2013).

This technological innovation was followed by a rapid expansion of soy production in the Cerrado and Amazon, with a sharp increase in the last two decades (Bragança, 2018; Assunção and Bragança, 2015; Rausch et al., 2019). Area in soy in the Cerrado tripled from 5 million hectares in 2000 to 15 million hectares in 2019. In the Amazon, the increase was twenty-fold, as soy increased from 0.25 million to 5 million hectares (MapBiomas, 2020). Ability to deliver products to markets in a cost-effective manner continues to be a key factor in expansion (Rada, 2013); today, most land that has been cleared for soy is within 10 kilometers of a federal or state highway and within 100 kilometers of a silo or crushing facility owned by a major soy trader (Rausch et al., 2019). Because soy cultivation in this region requires substantial investments in fertilizers and equipment, the expansion of crop cultivation occurs in large-scale and mechanized farms (Bragança, 2018).

Figure 3.1 charts the increase in soy production and productive area across the Amazon and Cerrado biomes from 2004 to 2019. In total, the area in soy in the sample doubled from 8 million to 16 million hectares, and production increased from 17 million to 41 million tons. Increased productivity is consistent with adoption of intensification methods, such as pesticide application or GM seeds.

Soy expansion typically occurs through conversion of pasture to soy rather than clearing of native forest (Rausch et al., 2019; Gibbs et al., 2015; Garrett and Rausch, 2016). Importantly, cattle production uses relatively few inputs, particularly in terms of pesticides, while input use for soy production is high (de Moraes, 2019; Garrett and Rausch, 2016). Input use for soy has increased further since 2004, when genetically-engineered pesticide-resistant soybean varieties were approved in Brazil (Dias et al., 2020). Farmers also apply more inputs per hectare of soy than other temporary crops, including corn, rice, beans, and sugarcane (Pignati et al., 2017; Dias et al., 2020; de Moraes, 2019).

3.2.2 Pesticide use and control

Pesticides are a textbook example of a good with a negative externality (Soares and Porto, 2009). Farmers and pesticide producers earn private benefits in the form of profits from continued use and production of pesticides, and in many cases farmers receive training and extension from their pesticide dealers. However, the plentiful health and environmental concerns lead to a high social cost. There is strong evidence that individuals who work directly with pesticides (e.g., farmers and applicators) are at risk for acute pesticide poisoning (Kouser and Qaim, 2011; Soares and Porto, 2009) and long-term pesticide-related health concerns (Pingali et al., 1994). There is growing evidence that pesticides pose a health risk to vulnerable populations in the broader community, including fetuses and infants (Dias et al., 2020; Taylor, 2021; Brainerd and Menon, 2014) and the elderly (Lai, 2017), particularly through water contamination. Water is a primary form of

Figure 3.1: Percent of municipal area planted in soy in 2004 and 2019 across the Amazon and Cerrado

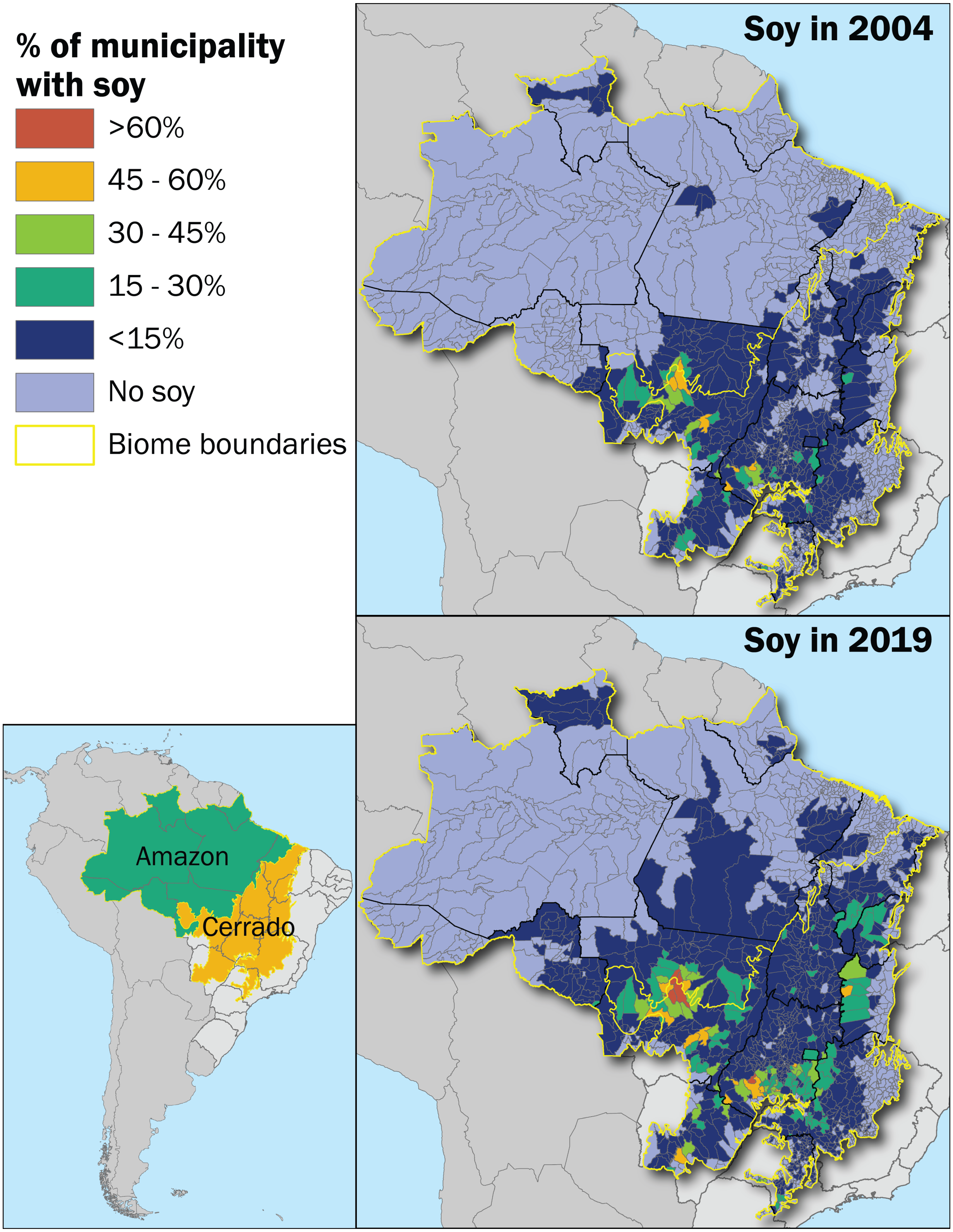
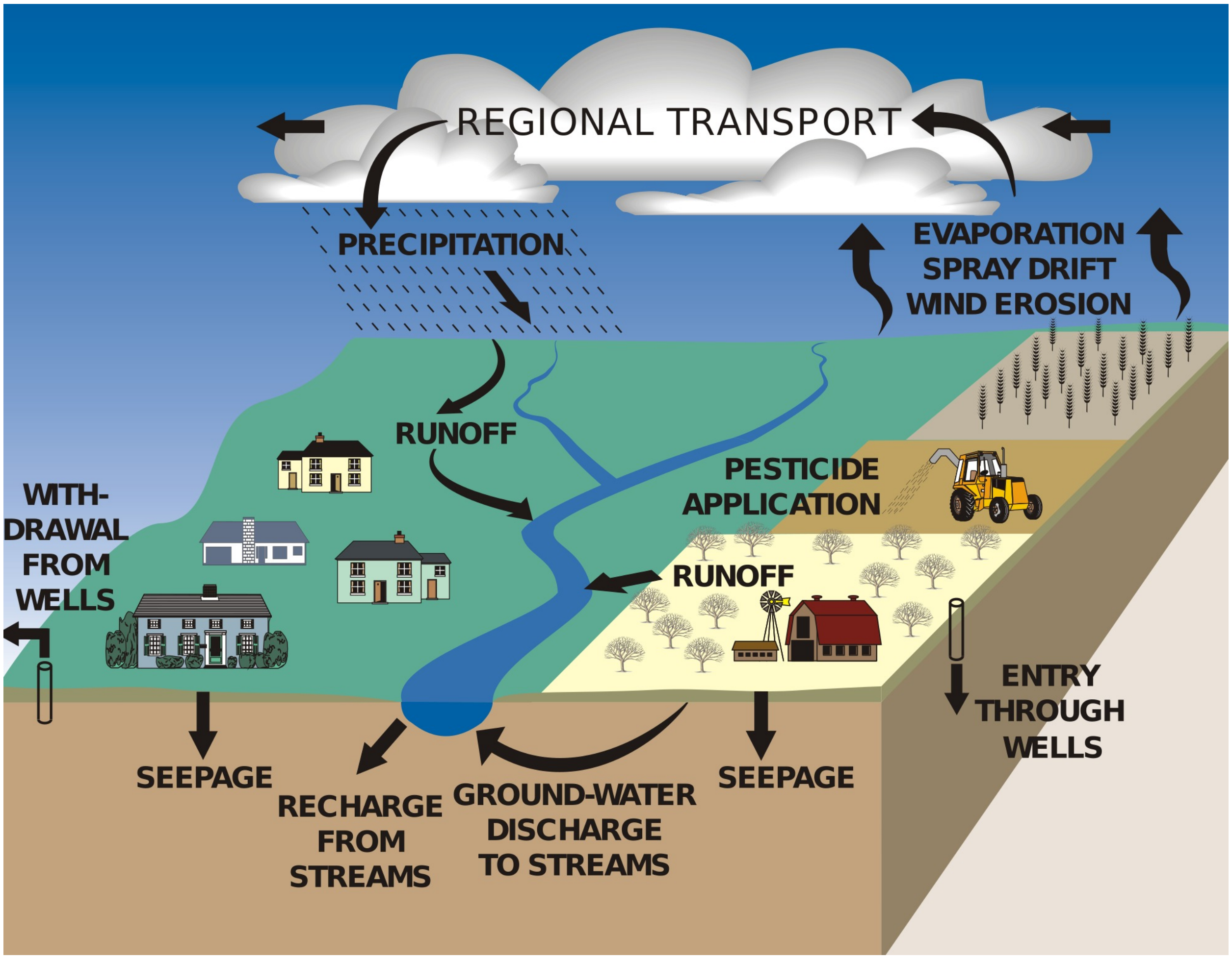


Figure 3.2: Source: US Geological Survey Fact Sheet 2004-3093 (Eddy-Miller and Remley, 2004)



exposure to pesticides, as runoff from agricultural land carries chemical compounds to water sources used by households (see figure 3.2) (Dias et al., 2020; Lai, 2017).

These externalities have merited strict governmental control of pesticides around the globe (US EPA, 2021a). Brazil's landmark pesticide control law, passed in 1989, was one of the strongest globally (Gonzales, 2018, 2020). However, agricultural trade liberalization in the 1990s brought with it a dramatic increase in pesticide use in Brazil (Dasgupta et al., 2001). Limited funding for staffing and enforcement, along with increased pressure from lobbying groups, have eroded the power of the law over time (Gonzales, 2020).

3.2.3 Agrotoxins and cancer risk

In Brazil, several studies have found correlations between pesticide exposure and cancer, either by using data on lagged pesticide sales and cancer mortality (Chrisman et al., 2009) or by looking at specific types of tumors like colon (Uyemura et al., 2017) or prostate cancer (da Silva et al., 2015). Pesticides are a known risk factor for childhood cancer in particular (Belson et al., 2007; Omidakhsh et al., 2017; Soldin et al., 2009; Vinson et al., 2011), though the empirical evidence tends to draw on case control studies and retrospective data on pesticide exposure. Some studies have found a significant effect of exposure to pesticides and ALL (Cárceles-Álvarez et al., 2017; Gunier et al., 2017; Hernández and Menéndez, 2016; Wang et al., 2019), while other studies have found a negligible effect of paternal exposure on ALL specifically (Glass et al., 2012; Patel et al., 2020; Vinson et al., 2011).

Risk factors for ALL beyond pesticide exposure include radiation exposure, prior exposure to chemotherapy, and certain rare genetic conditions (American Cancer Society, 2019). ALL is a highly treatable cancer conditional on timely and high-quality care, but is fatal without it (St. Jude Children's Research Hospital, 2018).

3.2.4 Cancer treatment in Brazil

Health care is provided in Brazil by public health insurance (the Sistema Único de Saúde, or SUS), though 25% of the population has supplementary private health insurance (Da Silva et al., 2019). Public hospitals are free but often have long wait lists for appointments, making receiving timely care difficult. These disparities are more egregious in rural areas and among poor populations (De Souza et al., 2016).

The majority of cancer treatment is done by high-complexity oncology centers (Portuguese acronym CACON) or high-complexity oncology units (Portuguese acronym UNACON), with some complementary treatment done at general hospitals (da Silva et al., 2015). Across the entirety of Brazil, there are 299 accredited oncology programs, though almost half of those are in the Southeast (Espírito Santo, Minas Gerais, Rio de Janeiro, and São Paulo). Pediatric oncology centers are even fewer and farther between, with 72 in the entire country as of 2017. However, there are only two pediatric oncology centers in the entirety of the Amazon. In the Cerrado there are 35 pediatric oncology centers, but 31 of these are within the relatively urban states of Bahia, Minas Gerais, and São Paulo, and 26 are in São Paulo alone.

Table C1 describes the number of hospitals that treated pediatric oncology patients from 2005 - 2009, 2010 - 2014, and 2015 - 2019 by state. Consistent with (Da Silva et al., 2019), the South and Southeast lead the nation in treatment availability, while availability is low in the North. Most states saw only modest increases or decreases in treatment availability over the period. Maranhão saw the largest percent increase (2 hospitals, 200%), while Minas Gerais and the Federal District saw the largest absolute increase (5 hospitals, 19% and 167%, respectively). Mato Grosso saw the largest percent decrease (1 hospital, -25%), while São

Paulo saw the largest absolute decrease (7 hospitals, -15%).

Concentration of UNACONs and CACONs in the wealthier and more populous states of Brazil further exacerbates health disparities seen in the Amazon and the frontier. Socioeconomic status (SES) is inversely related to overall cancer risk (Dean et al., 2018; Doubeni et al., 2012; Singh and Jemal, 2017; Underhill et al., 2017). Notably, the literature is divided on the relationship between SES and ALL, with some researchers finding higher ALL incidence in higher SES households and others finding the opposite relationship (Marquant et al., 2016; Poole et al., 2006). In the context of Brazil, geographic isolation from pediatric CACONs and UNACONs will likely limit the kinds of treatments locally available and/or require patients and their families to travel hundreds or thousands of kilometers to seek treatment. Additionally, distance limits the kinds of treatments that are available daily. For example, if there is no hospital that is accredited to administer chemotherapy within driving distance, patients may not be able to receive treatment at optimal time intervals.

3.3 Materials and methods

3.3.1 Data

Our data consists of a a 15-year, municipal-level panel on health outcomes, land use, surface water, and demographics. Mortality data is publicly available from DataSUS. These encounters are defined by ICD-10 (International Classification of Disease) diagnosis category codes and stratified by age bins, allowing us to identify fatal cases of lymphoid leukemia (ICD-10 code C91) in the population under ages 5 and 10 at the municipal-year level. Among children (under age 5 and under age 10), these cases should overwhelmingly consist of deaths from ALL (St. Jude Children's Research Hospital, 2018).

We compile data on soy, sugarcane, all other temporary crops, pasture, mining, and natural vegetation using land cover maps from Mapbiomas version 5. Using this data, we can calculate the total number of hectares in the municipality that are planted in soy as well as control for sugarcane, remaining forest, natural vegetation, and area in pasture. Sugar cane is a major temporary crop grown is Brazil that is not intercropped with soy, unlike corn. Population data are available from the Brazilian Institute of Geography and Statistics (IBGE), although they are not available annually stratified by age group. Watersheds (geographic areas over which rainfall is channeled via rivers and creeks to eventual outflow points) are measured as Ottobasins in Brazil and are available from the National Water Assocation (ANAS). Following (Dias et al., 2020), we focus our analysis on Level 3 Ottobasins, which includes catchment areas that overlap multiple municipalities (SI figure C1).

3.3.2 Sample

Our main sample includes municipalities in the Amazon and Cerrado that are classified as "rural" per IBGE categories and have at least 25% of land cover in agriculture. This excludes municipalities that are either urban or highly forested, both of which are likely to have different patterns of cancer rates than our sample of interest.

We exclude municipalities outside the Amazon and Cerrado biomes, which correspond to the Atlantic Forest, Caatinga, Pampa, and Pantanal biomes. These biomes either had established soy production at the beginning of our study period (i.e., the Pampa and Atlantic Forest regions of Paraná, Santa Catarina, and Rio Grande do Sul) or had relatively little soy production throughout the period and significant urbanization and industry that presents a challenge to identification (i.e., the Atlantic Forest and Cattinga portions of Rio de Janeiro, Espírito Santo, Rio Grande do Norte, Paraíba, Pernambuco, and Sergipe).

To avoid contamination of our treatment, we consider soy production beginning in 2004, the year that genetically modified (GM) soy was approved for use in Brazil. This GM soy is herbicide-resistant, allowing farmers to use chemical inputs to control weeds without damaging the growing soy plants. GM soy has been shown to have changed the use of pesticide in Brazil, especially the highly hazardous chemical glyphosate (the active agent in Round-Up), and is linked to increased adverse health outcomes (Dias et al., 2020).

3.3.3 Identification strategy

We estimate the effect of soy production on pediatric deaths from ALL using an OLS model with fixed effects:

$$C_{mt} = \alpha S_{m,\overline{t-5}} + \beta X_{mt} + \delta_m + \gamma_{rt} + \epsilon_{mt}. \tag{3.1}$$

Outcomes C_{mt} are pediatric (under age 5 and under age 10) deaths from ALL in municipality m in year t. We measure deaths per 10,000 total population and as a binary indicator.

Our independent variable of interest, $S_{m,\overline{t-5}}$, is the average soy production in municipality m in years $t-1$ to $t-5$. To account for this lag, we include observations of health outcomes beginning in 2009. We measure production as the percent of area in soy production and the tons of soy produced per municipal hectare. Controls X_{mt} include the percent of municipal area in natural vegetation, mining, and sugarcane and municipal population. We include municipal fixed effects and meso-region-year fixed effects. Mesoregions are unit of analysis that were designated by IBGE. They are smaller than a state, and are meant to represent an "individualized area [...] with its own regional identity" (IBGE, 2018). Standard errors are clustered at the Ottobasin (level 3). When municipalities are located in multiple, we choose the majority Ottobasin.

Our identification strategy rests on the assumption that soy expansion patterns are exogenously related to pediatric ALL after controlling for fixed effects. Municipal fixed effects account for unvarying characteristics that might influence cancer outcomes, such as sun exposure. Meso-region-year fixed effects account for different trends in development across regions, which relates to socioeconomic status and treatment access. As discussed, our focus on childhood cases reduces the risk of behavioral factors that might increase risk (e.g., exposure to radiation in the workplace), and our focus on a cancer that can be diagnosed using a relatively straightforward blood test reduces disparities in likelihood of diagnosis.

Next, we consider the role of soy production occurring outside the municipality but within the Ottobasin. Water is a primary form of exposure to pesticides, as runoff from agricultural land carries chemical compounds to water sources used by households (see figure 3.2) (Dias et al., 2020; Lai, 2017). We estimate Ottobasin-level soy productive area by overlaying MapBiomas land use maps with Ottobasin boundaries. We impute Ottobasin-level soy production by assuming that production occurs uniformly across the municipal area and estimating production based on the municipal area in the Ottobasin. For municipalities that fall within multiple Ottobasins, we again assume that production is uniform across space and estimate the Ottobasin exposure based on the proportion of the municipality that falls within each Ottobasin. Figure C1 maps the Ottobasin and municipal boundaries in the Cerrado and Amazon. To estimate pediatric ALL deaths due to soy production in the Ottobasin, we modify equation 3.1 so that the treatment, $S_{h,\overline{t-5}}$, and land use controls, X_{ht}, are estimated at the level of Ottobasin h.

3.4 Results

Our results show that pediatric deaths from ALL increased with soy expansion in Brazil's Amazon and Cerrado. We present the coefficient of interest from all model specifications in figure 3.3 with symbols denoting the specification (i.e., outcome variable, observation level, and sample). We report include full regression tables of these models in the supplement.

We find that a 10 percentage point increase in municipal area in soy led to an additional 0.019 deaths under 5 per 10,000 population and an additional 0.025 deaths under 10 per 10,000 population; the latter is our main specification (figure 3.3). During this period, the mean level of soy coverage in the sample was 3% with a standard deviation of 8%, and the mean deaths per 10,000 population for children under 5 and 10 were 0.007 and 0.016, respectively. The binary model shows that a 10 percentage point increase in municipal area in soy increases the likelihood that a single child in that municipality and year under 5 would die from ALL by 1.6% and a child under 10 by 2.4% (figure 3.3).

We also find a positive relationship between soy production and pediatric deaths from ALL. An increase of 0.10 tons of soy per municipal hectare increased deaths under 5 by 0.003 per 10,000 population and deaths under 10 by 0.005 per 10,000 population. The mean value of production in the period was 0.11 tons per hectare with a standard deviation of 0.25.

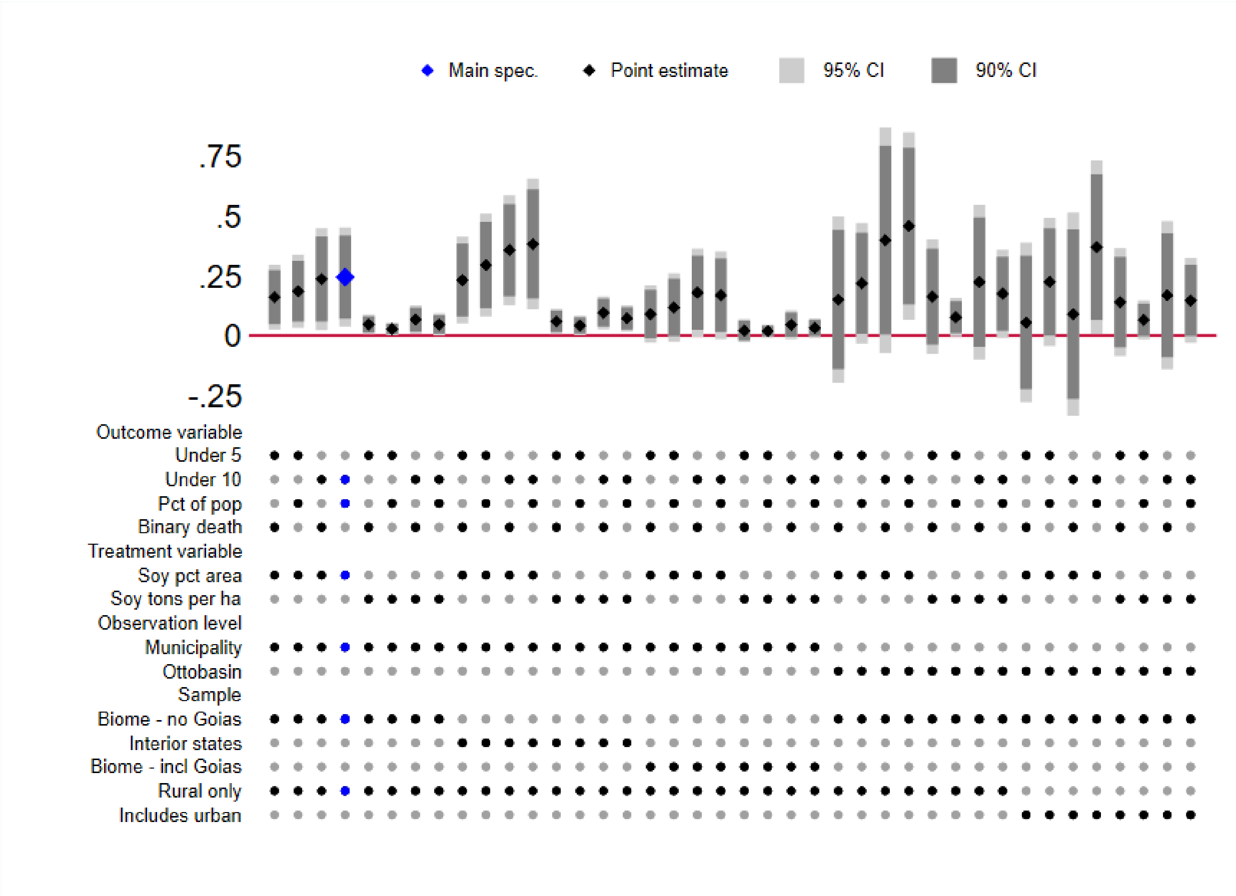
Our primary sample includes all municipalities that were at least 90% contained in the Amazon or Cerrado, excluding those in the state of Goiás. Goiás includes the Federal District and differs from the remainder of the sample economically and in terms of availability of cancer treatment. We discuss this choice further in the supplement. When we do include Goiás in the sample, our results decrease in magnitude. The coefficients increase in magnitude when we further limit analysis to states in the "interior" of Brazil, excluding municipalities in the states of Minas Gerais, São Paulo, and Goiás. These states have larger urban populations and more access to oncology hospitals than the rest of our sample (table C1. This suggests that the relationship between pesticide exposure and fatal cases of ALL weakens as cancer treatment is more available. This follows the highly treatable nature of ALL. In interior states with few hospitals treating pediatric oncology, we find that a 10 percentage point increase in municipal area in soy led to an additional 0.030 deaths under 5 per 10,00 population and an additional 0.38 deaths under 10 per 10,000 population in the interior states. In contrast, in Goiás alone, which had 11 hospitals (including those in DF) treating pediatric oncology cases from 2015 - 2019, we estimate a precise null effect.

The results of our main specification are equivalent to an additional 124 deaths of children under 10 from 2008 to 2020 across the sample. This compares to 213 total deaths from ALL for children under 10 in our sample during the period.

3.4.1 Investigating mechanisms

Next, we investigate if water sources were a primary method of exposure. Moreover, we implement a series of tests to confirm that our results were driven by our proposed mechanism of pesticide exposure rather than through endogenous changes that occur in a community when crop agriculture replaces cattle production. We present the results of these falsification tests (i.e., those where we would not expect a significant relationship)

Figure 3.3: Coefficients of previous five year production of soy on pediatric deaths from ALL under various specifications



in figure 3.4.

First, we use watershed-level soy production to test whether water was a source of pesticide exposure. We find that a 10 percentage point increase in Ottobasin area in soy led to an additional 0.022 deaths under age 5 per 10,000 population and an additional 0.046 deaths under age 10 per 10,000 population (figure 3.3). These effects are larger than the effects of municipal-level production, and suggest that production anywhere in the Ottobasin may reach a child via waterways and water sources. The relationship between soy production in tons is positive, although insignificant in some specifications, as we cannot directly measure Ottobasin-level soy production in tons.

To further confirm our results are due to indirect exposure, we include a set of municipalities that are classified as urban but are located in Ottobasins with high agricultural production (i.e., at least 25% of area in 2019 was in agriculture). Children in urban municipalities are unlikely to have frequent direct contact with pesticide or soy production but may be exposed to pesticides indirectly through water sources. We find a positive and significant effect of Ottobasin-level soy area on deaths per 10,000 population after including these municipalities (figure 3.3 and appendix table C5). The results of the binary model are insignificant, likely due to the large differences in population between urban and rural municipalities. Our results suggest that residents of urban areas may be at risk of pesticide exposure if production is high in the watershed.

Next, we test the effect of non-soy annual crops (excluding sugarcane) on pediatric deaths from ALL. Soy is the most heavily-treated crop (Dias et al., 2020; de Moraes, 2019), so we would not anticipate a relationship between non-soy annual crops and deaths from ALL. Indeed, we find little evidence of a relationship between these crops and deaths from ALL (figure 3.4).

Further, we test whether a one-year lag of soy production is related to pediatric deaths from ALL. The timing from pesticide exposure to deaths from ALL is likely longer than a year, even for young children. Thus, a single-year lag captures the current levels of development in a municipality rather than the child's lifetime exposure to pesticide. Again, we find no statistically significant relationship between the one-year lagged soy production variables and pediatric deaths from ALL (figure 3.4).

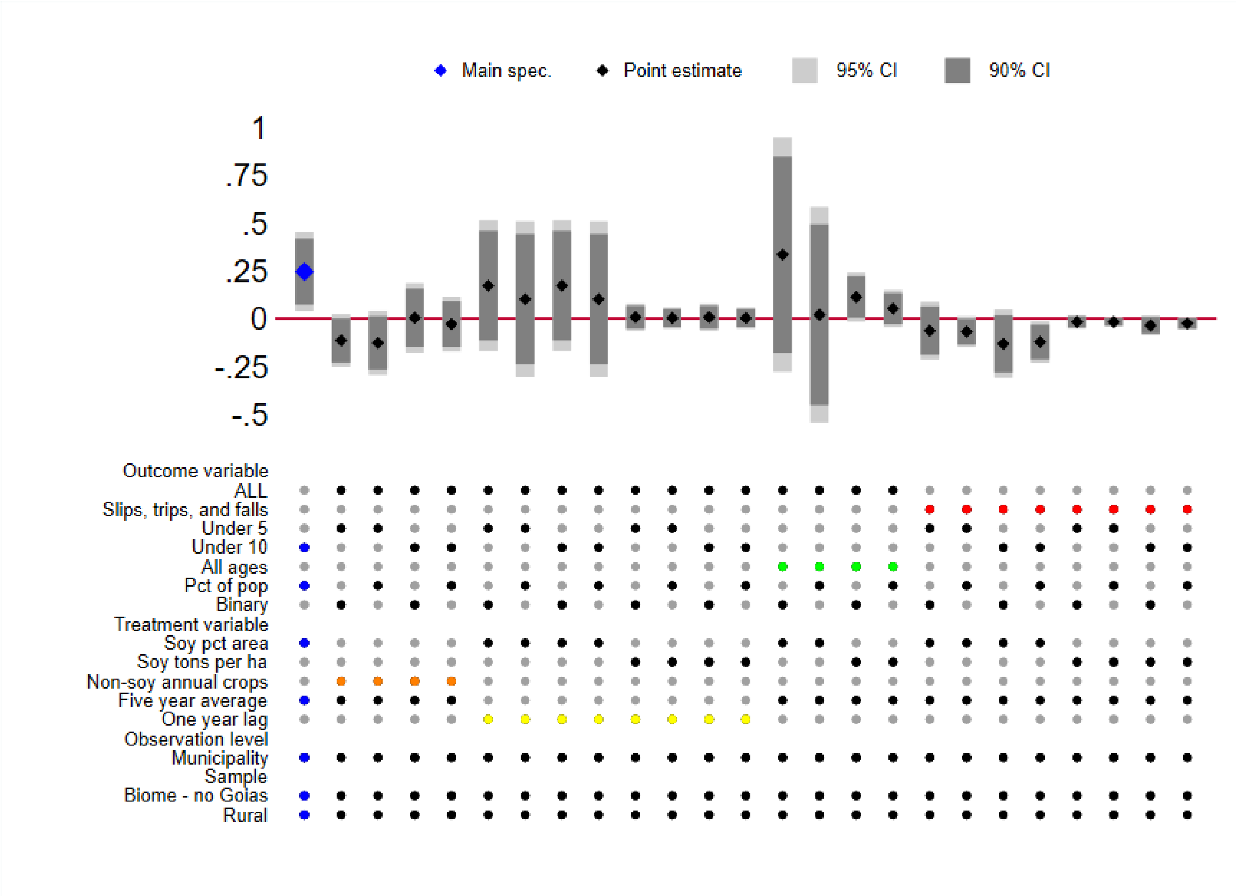
We also conduct a placebo test to measure whether soy expansion affected pediatric deaths from slips, trips, and falls (ICD10 code W00 - W19) (figure 3.4). The chemicals in pesticides are not a risk factor for this condition, and the only path for soy expansion to influence deaths from this accident is indirectly through socioeconomic conditions and healthcare availability. We find weak evidence that deaths from slips, trips, and falls decreased as area in soy expanded. This result would bias our results downward, as it suggests that healthcare availability and quality increased as soy expanded. Importantly, however, the majority of care that children receive for ALL takes place outside of the municipality at specialized cancer clinics.

3.5 Conclusion

We identify a causal relationship between soy expansion in the Brazilian Amazon and Cerrado and childhood deaths from ALL. The relationship is present for both municipal-level and Ottobasin-level soy area and municipal-level soy production. In total, we estimate that 124 deaths of children under 10 from ALL were due to soy exposure between 2008 and 2012. This is roughly half of the deaths from ALL in the sample period. Through a series of tests, we argue that this effect was due to indirect exposure to pesticides through water sources.

Our results are a conservative estimate of the adverse health effects due to soy expansion. It is likely

Figure 3.4: Coefficients of previous agricultural production on deaths in a series of falsification tests. Colors highlight the defining characteristic of the model.



that pesticide exposure caused many other forms of illness that we don't study here, including other forms of cancer, neurological illnesses, and acute pesticide poisoning. Additionally, we do not quantify the human or financial costs of non-fatal cases of ALL, which are substantial (Iyer et al., 2015; Kanellopoulos et al., 2016). Pediatric oncology and hematology are able to use aggressive forms of radiation and chemotherapy to treat cancer in children given the resilience of younger bone marrow. However, exposure to radiation and chemotherapy have lifelong impacts on physical and cognitive development, especially for very young children (under age 3) (Iyer et al., 2015; Kanellopoulos et al., 2016). These effects put even further strain on already-taxed public health systems, especially in poor, rural areas of Brazil. Further, we do not estimate effects in the historic soy-producing regions of Brazil or in non-rural municipalities. Our estimate is therefore a lower bound of the total cost of pesticide exposure to Brazilian public health.

This work has substantial implications for both health and agricultural policy at the local and national levels. Rural populations require access to timely cancer treatment; increasing access may reduce the number of fatal pediatric ALL cases. Programs to develop registries of all identified diagnoses of cancer, as have been developed in some countries, (National Institute for Cancer Epidemiology and Registration, 2021). These registries can aid researchers and policymakers in identifying potential cancer clusters, improving the speed and efficiency of public health response. While such programs are costly, they are much more feasible in the context of Brazil's public health system. Medical doctors may consider adopting a standard screening procedure for children in communities with increasing or high soy production. Such a procedure may include annual blood tests to assess immune system health and detect early signs of liquid tumors and parental education on water source safety. Regulation of highly hazardous chemicals may reduce the effects we study here. Additionally, advising in proper levels of pesticide use by individuals that don't privately benefit from pesticide use may reduce the magnitude of the negative externality.

We present this narrowly-defined study in order to provide the first causal evidence on the effect of pesticide exposure on cancer deaths. This work underscores the importance of further studies on the health impact of pesticide exposure.

Appendix A

Turning a house into a home: Delayed property rights and education investment decisions in urban Chile

A.1 Sample sizes

In the CASEN questionnaire, individuals are defined by their relationship to the household head. As such, it is important to be careful in defining inclusion into the sample in a way that ensures treatment assignment is accurate to that person's lived experience. Individuals can join a household in three primary ways. Direct members are born into the household or start the household (and become household head themselves). This includes siblings, parents, and children. Members-by-marriage join when they (or one of their direct relatives) marry a direct member of the household. Finally, individuals can join the household through some other means such as moving in with (more distant) relatives or employment as a domestic worker.

Table A1 reports the sample size of each of these groups (broken down by specific relationship to household head) and the necessary assumptions to include members of each of these groups. Household members are defined as "treated" or "control" based on their age at the time housing was received. As described in the main text, all households in the sample receive the housing subsidy, but this distinction by age allows different individuals in the same household to be assigned to different experimental treatment/control groups. When assigning treatment to members of a household, I am making three assumptions. The credibility of these assumptions differs across relationship to household head, and so I conduct robustness checks throughout Appendix A.2 that address these concerns.

First, I assume that the individual was a member of the household at the time of treatment (or born into the household after treatment). This is likely to not hold for individuals who joined the household by marriage or some other way. Second, the member was not previously treated in a household they belonged to before the current household. This is possibly violated for individuals who joined indirectly or by marriage. Finally, the individual was a continuous member of the household since treatment. For this reason, I check for robustness by limiting the sample to younger individuals who are likely to not have moved households (table A3), children of the household head (table A4), and specifically young children of the household head (table A5).

A limitation to this method is the inability to distinguish between children born into the household and children who entered the household through their parent's marriage (step-children). A recent OECD shows that Chile has the lowest divorce rate of all OECD countries at roughly 0.1 divorces per 1,000 people. As a result, there should be relatively few step-children counted in the "children" category. Further, the age at first marriage is over 30 for both men and women in Chile, thus there should be (and is) a large sample of individuals who are old enough to have finished schooling (age 18-30), and still residing at home and therefore still defined as children of the head (OECD, 2019). Additionally, individuals who are counted as being members of the household at the time of treatment but who did not reside there should bias the results down, as these individuals did not receive the treatment.

Table A1: Sample size by relationship to household head, age

	Treated	Control	Treated	Control	Treated	Control	Treated	Control
Relationship	**All Ages**	**All Ages**	**0-24**	**0-24**	**25-50**	**25-50**	**51+**	**51+**
Household Head	403	112,658	141	770	262	63,580	0	48,308
Child	158,127	37,870	150,019	14,850	8,108	21,985	0	1,035
Parent	0	2,860	0	0	0	121	0	2,739
Sibling	383	2,963	299	221	84	1,484	0	1,258
Grandchild	28,658	1,352	28,375	869	283	478	0	5
Partner	838	87,761	415	2,628	423	60,490	0	25,397
Parent-In-Law	0	1,915	0	0	0	88	0	1,827
Child-In-Law	2,468	3,652	1,780	800	867	2,741	0	111
Sibling-In-Law	202	1,158	1,360	106	32	709	0	343
Total	191,079	252,189	181,020	19,490	10,059	151,676	0	81,023

Note: Treatment and control groups are defined by individual's age at the time that the household received housing.

A.2 Robustness tests: pooled cross-section

In this section I present a series of robustness checks referred to throughout section 1.6.1 of the paper.

A.2.1 Restriction: age

In the main text of the paper, I restrict the sample to be individuals over age 25 to avoid counting individuals who may have yet to complete secondary school or university but will in the future. As a robustness test, I lower the age cap for inclusion into the sample to 18 for secondary school and 22 for university (table A2). The results remain statistically significant and positive.

Table A2: Pooled cross-section: lower age caps

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling
Use Rights	0.0614***	0.0303***		
	(0.00479)	(0.00616)		
Use + Transfer Rights	0.0379***	0.00186		
	(0.00487)	(0.00642)		
Use Rights			0.0140***	0.0144***
			(0.00287)	(0.00398)
Use + Transfer Rights			0.0353***	0.0192***
			(0.00394)	(0.00498)
Constant	0.0638***	0.477***	0.00140	0.0948***
	(0.00805)	(0.00940)	(0.00384)	(0.00533)
Observations	295,304	295,304	261,576	261,576
R-squared	0.169	0.671	0.024	0.632
Household FE	No	Yes	No	Yes
Clustering	Household	Household	Household	Household
Outcome mean	0.376	0.376	0.0608	0.0608

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample is limited to individuals at least age 18 at the time of the survey. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

I also restrict the sample to only include those aged 25 to 50 at the time of the survey to confirm that the results are not driven by the oldest individuals in the sample. Results are reported in table A3. Again, the results remain positive and statistically significant.

Table A3: Pooled cross-section: sample aged 25-50

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling
Use Rights	0.0695***	0.0678***		
	(0.00790)	(0.0102)		
Use + Transfer Rights	0.0523***	0.0194		
	(0.00987)	(0.0129)		
Use Rights			0.0136***	0.0261***
			(0.00345)	(0.00424)
Use + Transfer Rights			0.0435***	0.0251***
			(0.00546)	(0.00618)
Constant	0.171***	0.134***	0.0371***	0.0437**
	(0.0257)	(0.0310)	(0.0117)	(0.0176)
Observations	161,071	128,165	161,071	128,165
R-squared	0.056	0.716	0.019	0.680
Household FE	No	Yes	No	Yes
Clustering	Household	Household	Household	Household
Outcome mean	0.398	0.400	0.0661	0.0641

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample is limited to individuals age 25-50 at the time of the survey. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

A.2.2 Restriction: children of household heads

It is reasonable to be concerned about using household fixed effects when considering adults age 25 and above in this dataset. Household fixed effects are defined by the individual's household *at the time of observation.* As a result, they may be living in a household which received title before that individual joined the household (e.g. if they married into the household). If an individual marries and moves households, then the age at housing in their current household would likely be uncorrelated with their age at housing at the time they were in school.

To mitigate this, I have done the following two robustness checks. First, I restrict the sample to only include individuals who are the children of the household head. This limits the sample to individuals who still live with their parents, and therefore are in the same household they were in at the time they received housing. The results of this restriction are reported in table A4. For comparability with other results, this sample is limited to only include individuals age 25-50 at the time of the survey. Without household fixed effects (columns 1 and 3), the results are robust. However, with these fixed effects, the results are indistinguishable from zero.

However, it is important to note that children age 25-50 who still live with their parents are a very specific sub-population. Further, when using household fixed effects, they are only being compared to their adult

Table A4: Pooled cross-section: children of household head

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling
Use Rights	0.0350***	-0.00689		
	(0.00923)	(0.0177)		
Use + Transfer Rights	0.0294***	0.00194		
	(0.0108)	(0.0211)		
Use Rights			0.0136**	0.0121
			(0.00534)	(0.00913)
Use + Transfer Rights			0.0199***	-0.00480
			(0.00693)	(0.0112)
Constant	0.152***	0.368***	0.0461***	0.0951***
	(0.0248)	(0.0571)	(0.00677)	(0.0250)
Observations	29,993	13,126	29,993	13,126
R-squared	0.131	0.746	0.052	0.738
Household FE	No	Yes	No	Yes
Clustering	Household	Household	Household	Household
Outcome mean	0.515	0.467	0.110	0.0914

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample is limited to individuals age 25-50 at time of survey who are children of the household head. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

siblings who also live at home. To address this, I restrict the sample instead to individuals age 18-25 at the time of the survey. (To address concerns that individuals age 18-22 would potentially still be in university and therefore not able to have achieved 16 years of schooling, I also present the sample restricted to ages 22-27 in columns 5 and 6.) The results are reported in table A5. Again, when not using household fixed effects, the results remain positive and statistically significant for all specifications.

I also run the same regression on age-specific cohorts. In table A6, I report the results of the same regressions on samples of 18, 22, and 25 year old respondents. This allows me to look at the impact of housing on an 18 year old compared to another 18 year old who did not receive housing. Note that this specification cannot include household fixed effects as such a specification would only be identified off of twins (two individuals age 18 who are children of the household head in the year of observation), and there would be no variation in age of housing between twins.

Column 1 shows that there is a positive effect of housing on finishing high school for 18 year-olds, with a greater and significant effect for use and transfer rights. Column 2 shows the same specification on 22 year-olds, where instead having use rights is positive and significant. This suggests that there is important variation in completion rates after 18 not being considered by only looking at individuals who are 18. Future work could dig into whether this is reflective of housing inducing students to re-enter schooling, therefore completing school after age 18 (but completing it more so than peers who did not receive housing).

Table A5: Pooled cross-section: young children of household head

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling	(5) 16+ Years of Schooling
Use Rights	0.0540***	0.00351			
	(0.00622)	(0.0131)			
Use + Transfer Rights	0.0275***	-0.0184			
	(0.00558)	(0.0131)			
Use Rights			0.0187***	0.0343***	0.0207***
			(0.00461)	(0.00867)	(0.00598)
Use + Transfer Rights			0.0103***	0.0140*	0.0189***
			(0.00251)	(0.00804)	(0.00563)
Constant	0.619***	0.920***	0.0378***	0.289***	0.0628***
	(0.0433)	(0.0593)	(0.0109)	(0.0249)	(0.00765)
Observations	48,823	23,519	48,823	23,519	24,886
R-squared	0.083	0.698	0.090	0.598	0.038
Household FE	No	Yes	No	Yes	No
Clustering	Household	Household	Household	Household	Household
Outcome mean	0.680	0.660	0.0601	0.0656	0.141

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample is limited to children of the household head. Columns 1-4 are age 18-25, columns 5 and 6 are age 22-27. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

Column 3 shows the effect of housing on finishing university for 22 year-olds, and column 4 the same for 25 year-olds. The impact of housing is not statistically different from 0 for 22 year-olds, suggesting that they have yet to complete university by this age. Instead, the 25 year old cohort sees a positive effect of housing, with a statistically significant effect of transfer rights on completion. 25 year-olds who had received transfer rights by age 18 are roughly 5 percentage points more likely to have completed university than their peers who had not received housing.

Table A6: Pooled cross-section: children of household head (specific age cohorts)

VARIABLES	(1) 12+ Years of Schooling	(2) 12+ Years of Schooling	(3) 16+ Years of Schooling	(4) 16+ Years of Schooling
Use Rights	0.0317*	0.0573***		
	(0.0163)	(0.0161)		
Use + Transfer Rights	0.0550***	0.0272*		
	(0.0126)	(0.0151)		
Use Rights			0.0104	0.00632
			(0.0118)	(0.0151)
Use + Transfer Rights			0.00331	0.0464***
			(0.00932)	(0.0155)
Constant	0.285***	0.593***	0.0532***	0.0929***
	(0.0292)	(0.0398)	(0.0182)	(0.0214)
Observations	9,129	5,974	5,974	3,748
R-squared	0.073	0.078	0.018	0.038
Household FE	No	No	No	No
Clustering	Household	Household	Household	Household
Outcome mean	0.574	0.706	0.103	0.169

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level. Sample limited to children of the household head. Columns 1 is limited to individuals age 18, columns 2 and 3 are age 22, and column 4 is age 25. Note that the two sets of coefficients ("Use Rights Only" and "Use + Transfer Rights") are presented separately because they are defined differently for university degrees versus secondary school degrees.

A.2.3 Flexible specification

Finally, I present a flexible specification of the regressions in section 1.6.1, according to the following regression equation and reported in table A7:

$$\text{Education Level}_{i,h} = \alpha + \sum_{1}^{J} \beta_j \mathbb{1}\{\text{YearsofTreatment}_{i,h} = j\} + \delta X_i + \lambda_t + \epsilon_{i,h}. \tag{A.1}$$

The fully flexible specification suggests that individuals who received housing graduated from both college and university more than students who did not yet have housing. However, when household fixed effects are added, the story is not as clear. This is likely for reasons discussed in the preceding subsections. I report the flexible specification on the sample ages 25-50 in columns 3 and 4.

Table A7: Years of treatment on educational attainment (pooled cross-section)

VARIABLES	(1) Years of Schooling	(2) Years of Schooling	(3) 12+ Years of Schooling	(4) 12+ Years of Schooling
treatmentyears = 1	0.129*	0.0172	0.0245	0.0404
	(0.0683)	(0.0858)	(0.0152)	(0.0272)
treatmentyears = 2	0.278***	0.0782	0.0548***	0.0466*
	(0.0646)	(0.0841)	(0.0159)	(0.0258)
treatmentyears = 3	0.278***	-0.0125	0.0583***	0.0369
	(0.0639)	(0.0937)	(0.0165)	(0.0287)
treatmentyears = 4	0.264***	-0.219**	0.0531***	0.0501
	(0.0632)	(0.105)	(0.0173)	(0.0305)
treatmentyears = 5	0.350***	-0.237**	0.0596***	0.0839***
	(0.0634)	(0.118)	(0.0175)	(0.0317)
treatmentyears = 6	0.345***	-0.413***	0.0716***	0.0705*
	(0.0634)	(0.133)	(0.0183)	(0.0361)
treatmentyears = 7	0.393***	-0.427***	0.0569***	0.0819**
	(0.0637)	(0.148)	(0.0187)	(0.0391)
treatmentyears = 8	0.458***	-0.559***	0.0957***	0.0999**
	(0.0635)	(0.163)	(0.0195)	(0.0408)
treatmentyears = 9	0.470***	-0.648***	0.0640***	0.126***
	(0.0642)	(0.180)	(0.0207)	(0.0448)
treatmentyears = 10	0.492***	-0.610***	0.0932***	0.180***
	(0.0645)	(0.195)	(0.0207)	(0.0476)
treatmentyears = 11	0.490***	-0.829***	0.0978***	0.205***
	(0.0649)	(0.213)	(0.0218)	(0.0516)
treatmentyears = 12	0.613***	-0.737***	0.129***	0.189***
	(0.0648)	(0.229)	(0.0212)	(0.0552)
treatmentyears = 13	0.587***	-0.800***	0.105***	0.212***
	(0.0643)	(0.247)	(0.0240)	(0.0599)
treatmentyears = 14	0.717***	-0.714***	0.150***	0.269***
	(0.0667)	(0.265)	(0.0262)	(0.0696)
treatmentyears = 15	0.656***	-0.788***	0.126***	0.236***
	(0.0683)	(0.281)	(0.0297)	(0.0746)
treatmentyears = 16	0.625***	-0.829***	0.109***	0.221**
	(0.0698)	(0.300)	(0.0358)	(0.0881)
treatmentyears = 17	0.610***	-0.897***	0.0787*	0.250**
	(0.0724)	(0.314)	(0.0433)	(0.106)
treatmentyears = 18	0.604***	-0.898**	0.0764	0.280***
	(0.0594)	(0.350)	(0.0602)	(0.103)
Constant	7.932***	8.715***	0.375***	0.548***
	(0.296)	(0.371)	(0.0532)	(0.0508)
Observations	93,760	61,866	17,628	7,296
R-squared	0.101	0.680	0.075	0.715
Household FE	NO	YES	No	Yes
Clustering	Household	Household	Household	Household
Sample	Ages $\geq$ 25	Ages $\geq$ 25	Age 25-50	Age 25-50
Outcome mean	10.71	10.76	0.595	0.588

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level.

A.2.4 Alternative identification strategy

Next, I present an alternative identification strategy. Here, I limit the sample to individuals age 18-30 and look at the impact of housing on the amount of schooling they completed by age 18. To do this, I assume that students attended school starting in age 5 and attended continuously. (These are the same assumptions as in section (1.6.2)). I estimate the following:

$$\text{Education by Age } 18_{i,h} = \alpha + \beta_1 \mathbb{1}\{\text{YearsofTreatment}_{i,h} > 0\} + \beta_2 \mathbb{1}\{\text{YearsofTreatment}_{i,h} \geq 5\} + \delta X_i + \lambda_t + \epsilon_{i,h} \tag{A.2}$$

and report the results in table A8. Column 1 is restricted to only children of the household head, column 2 to individuals age 18-30, and column 3 to 18-24 year old children of the household head. Taking this most restrictive sample, we see that having use rights leads to an additional one-quarter of a year of schooling completed, while having use *and* transfer rights leads to an additional 0.4 years of schooling by age 18.

Table A8: Housing on education completed by age 18

VARIABLES	(1) Education by Age 18	(2) Education by Age 18	(3) Education by Age 18
Use Rights	0.263***	0.392***	0.284***
	(0.0293)	(0.0308)	(0.0329)
Use + Transfer Rights	0.0948***	0.101***	0.0821***
	(0.0256)	(0.0266)	(0.0274)
Constant	0.0584	9.472***	9.607***
	(0.102)	(0.186)	(0.224)
Observations	76,324	53,635	45,196
R-squared	0.158	0.056	0.055
Household FE	No	No	No
Clustering	Household	Household	Household
Children	Yes	No	Yes
18-24	No	Yes	Yes
Outcome mean	10.24	10.71	10.84

Household-clustered standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represents significance at the 5% level; and three asterisks (***) represents significance at the 1% level.

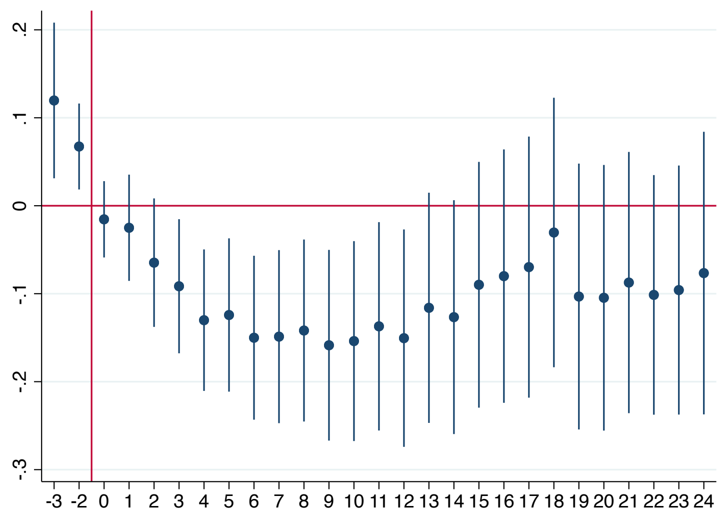
A.3 Robustness test: panel

Here I do an event study following:

$$NotAttend_{i,t} = \alpha + \sum_{j=-3}^{8} \beta_j \mathbb{1}\{YearsinHome_{i,t} = j\} + \eta age_{i,t} + \gamma_i + \lambda_t + \epsilon_{i,t}.. \tag{C1}$$

The results of the event study are presented in figure C1.

Figure C1: Years of housing on non-attendance



The figure suggests that either lagged security effects from use rights or an expectation of financial effects from transfer rights before year five lead to increased education investment. These results become statistically significant beginning in year 3. I argue that lagged housing security effects are less plausible than anticipation of financial effects (or cheating). If the lag on housing security was the dominating effect, this would mean families wait four years before making changes to investment in education. This would likely be too long of a wait for students to catch up to their peers. Instead, it is likely that this is an anticipation of a wealth effect or cheating the renting and selling prohibition. Because the survey data does not report when households were able to sell or rent their homes, it is possible that this effect beginning in year four is due to measurement imprecision. It is also plausible that this effect is biased due to the types of programs included in this analysis. Future work will divide the sample by program type to account for possible cheating in certain programs.

Appendix B

Seeking safe harbors: Emergency domestic violence shelters and family violence

Here, I describe the series of summary tables and robustness tests of the results presented in the main text of chapter 2.

Figure B1 describes variation in the independent variable of interest over time, first as binary shelter openings and second as indicators for any change to capacity. Note that there is bunching at 1984 as all shelters that opened before 1984 are lumped together in the initial data. Variation in any capacity change is distributed across all observed years. Figure B2 shows the homicide rate at the agency, county, and CBSA level for both IPV and DV. As the level of aggregation increases, variation in the dependent variable increases, though it is still left-skewed in all three levels of aggregation. Figure B3 shows variation in the dependent variable over time to address concerns about temporal variation in crime patterns. Sub-figure B3a shows that reported DV and IPV homicides are relatively uniform across the months of the year. Sub-figure B3b shows that the number of DV and IPV homicides per month have remained relatively constant, though the total homicide count has decreased slightly. Next, I provide correlation scatter plots of the different outcome variables of interest. Figure B4 presents correlations of all homicide, IPV/DV homicide, and IPV/DV assault at the county or agency level. While the strongest correlations are within type (IPV to DV homicide and IPV to DV assault), there are still positive correlations between IPV and DV homicide and assault.

Table B1 further describes sample selection by conducting balance tests for respondent counties versus all US counties and for counties identified as having a shelter versus all US counties. Respondent counties have less IPV and DV homicide, have a lower county poverty and unemployment rate, and have higher average annual pay and SNAP issuance compared to all other US counties. Counties identified as probably having shelter (regardless of response status) are similarly less poor, have higher incomes, and have lower crime rates than US counties that were not identified as probably having shelter.

The results presented in the main text use the county as the level of analysis. Here, I present results aggregated up to the Census-based statistical area (CBSA) and down to the level of the individual reporting agency. Table B2 estimates the primary specifications aggregated to the Census-based statistical area (with and without rural counties). Results on IPV homicide are robust to this aggregation without rural counties, but sensitive to inclusion of rural counties when equally weighted against CBSAs. Agency-level baseline estimations are presented in table B3, with the structural break design in figure B5. I also estimate the effect of shelter capacity on non-lethal violence at the agency level (table B4). Again, I find no substantial differences between the agency-level and county-level estimation strategies.

I then show a series of robustness tests for the structural break design. The negative effect of the change on IPV homicide holds using the 3-year average homicide rate as the outcome (figure B6), including counties with no changes to capacity has having (placebo) changes in the year 2023 (figure B7), and instrumenting for bed counts with an indicator for whether the shelter experienced a positive change (figure B8). The negative result for DV is not robust to a running homicide rate or inclusion of counties with no changes to capacity.

Next, I test for heterogeneity across different characteristics of shelters and counties. When interacting with a dummy for county population over 100,000 or 200,000 (table B6), the direct effect for counties over these thresholds is large and negative for IPV and positive but statistically indistinguishable from zero for DV. The interaction term for populations over 100,000 or 200,000 are negative but statistically insignificant, suggesting that the negative relationship between bed counts and homicide is greater in more populous areas. In other words, large changes to bed counts relative to current bed capacity have the strongest effects on reducing homicide in larger cities. I similarly test for heterogeneity with indicators for whether the observed change was a shelter opening (table B7) and find no significant results. It is possible that these effects are different in areas with increased unemployment, a major risk factor for DV/IPV. Table B8 interacts bed counts with the county-level unemployment rate and finds no statistically significant relationship. A higher unemployment rate is positively associated with IPV homicide and negatively with DV homicide, and the interaction term is small and sensitive to fixed effects.

I next show a series of results with modified versions of the outcome variable. Table B9 uses a three-year or five-year running average homicide rate as the outcome of interest. The result on beds per capita does not meaningfully change. I present a specification where the outcome variable is simplified to an indicator equal to 1 if the relevant homicide rate increased, -1 if it decreased, and 0 if it did not change (table B10). Results remain statistically insignificant. Finally, I do placebo tests where the outcome variable of interest is any homicide, homicide excluding IPV, and homicide excluding DV (table B11). I find a positive relationship between bed counts and these homicide rates that is small and insignificant.

I use the control variables to predict treatment and outcome (table B12). The coefficients on the control variables remain relatively stable and have strong predictive power for both the bed count variable and IPV/DV homicide. I then use the UCR-SHR homicide data to predict non-lethal IPV and DV assault and aggravated assault (table B13). I find much less predictive power in this regression and the sample size drops significantly.

I next do a series of robustness tests to different functional forms of the independent variable. First, I use a modified difference-in-differences design to more flexibly identify the effects of bed count changes. In this framework, I include separate bin dummies for changes of magnitude less than -5, greater than 5, and falling in between -5 and 5 (excluding 0). In this specification, the omitted category is changes of 0 (no change). I then interact these dummies with the contemporaneous bed count (per-capita). The full specification is as follows:

$$\begin{aligned}
\mathrm{Y}_{c,t} =& \alpha + \beta_1 \mathrm{Beds}_{c,t} + \mu_1 \mathbb{1}\{\mathrm{Change}_{c,t} < -5\} + \mu_2 \mathbb{1}\{\mathrm{Change}_{c,t} \in [-5,5]\} + \mu_3 \mathbb{1}\{\mathrm{Change}_{c,t} > 5\} \\
&+ \nu_1 \mathbb{1}\{\mathrm{Change}_{c,t} < -5\} * \mathrm{Beds}_{c,t} + \nu_2 \mathbb{1}\{\mathrm{Change}_{c,t} \in [-5,5]\} * \mathrm{Beds}_{c,t} \\
&+ \nu_3 \mathbb{1}\{\mathrm{Change}_{c,t} > -5\} * \mathrm{Beds}_{c,t} + X_c \delta + \gamma_c + \lambda_t + \epsilon_{c,t}
\end{aligned} \tag{1}$$

Results are reported in table B14. Again, I find no statistically significant or consistent relationship between changes in bed counts and either IPV and DV homicide rates. Here, the standard errors are much larger (by a factor of 10-100) than in the baseline OLS or net change specifications, reflecting insufficient power to precisely identify effects in three different directions.

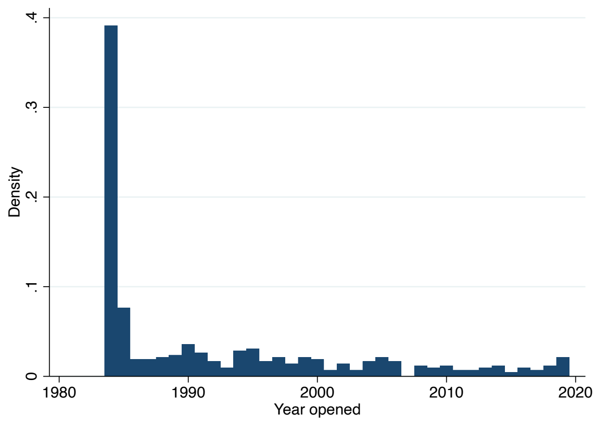
Table B15 presents the main specifications from the text including the lagged dependent variable. To address concerns about serial auto-correlation when using a dynamic panel (controlling for the lagged dependent variable) and fixed effects, I do the same analysis using the Arellano-Bond estimator (table B16). I also present results where the outcome variable of interest is a 3- or 5-year running average DV or

IPV homicide rate. Results do not change significantly.

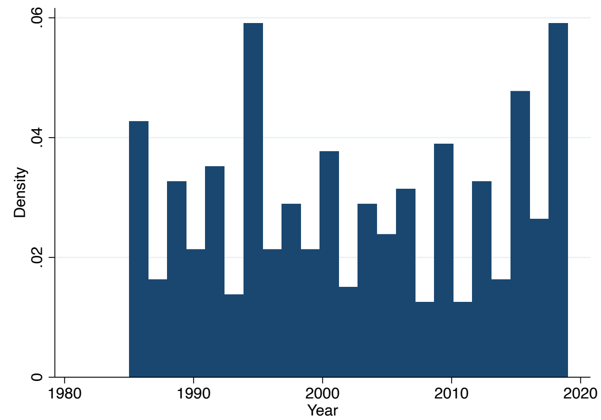
Finally, I provide supplemental information on the policies and laws included in the heterogeneity analyses (tables B17, B18, and B19).

B.1 Summary statistics

Figure B1: Histograms of shelter openings and capacity changes



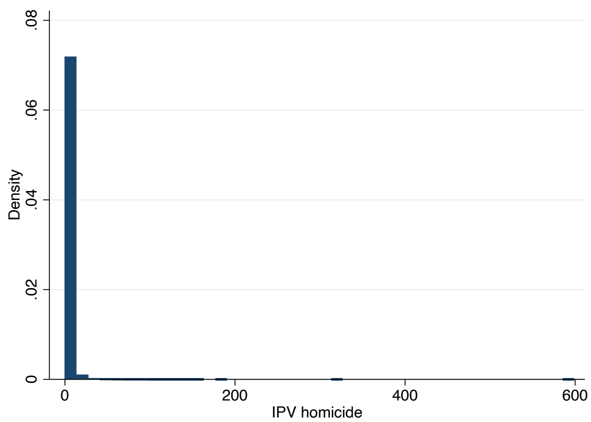
(a) Year Opened



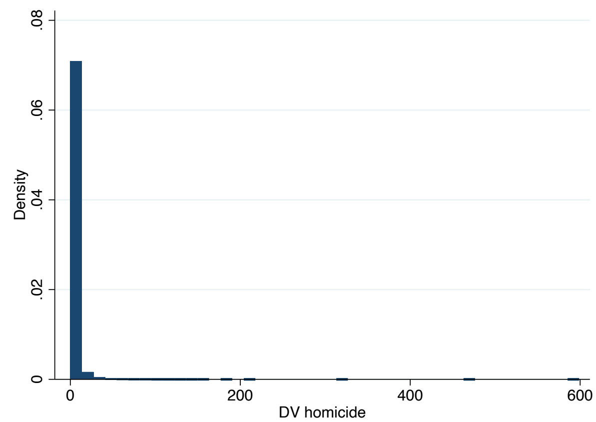
(b) Capacity changes per year

Note that all shelters that reported beginning operations before 1984 were binned together.

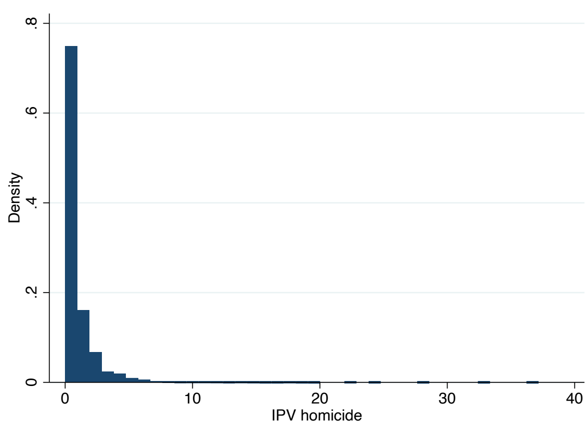
Figure B2: Histograms of homicide (by agency, county, or CBSA)



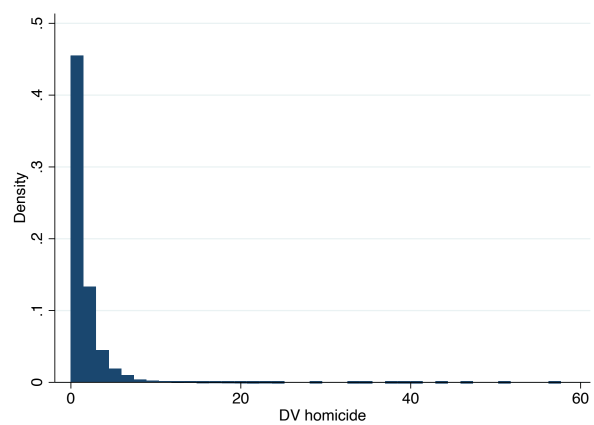
(a) IPV (agency level)



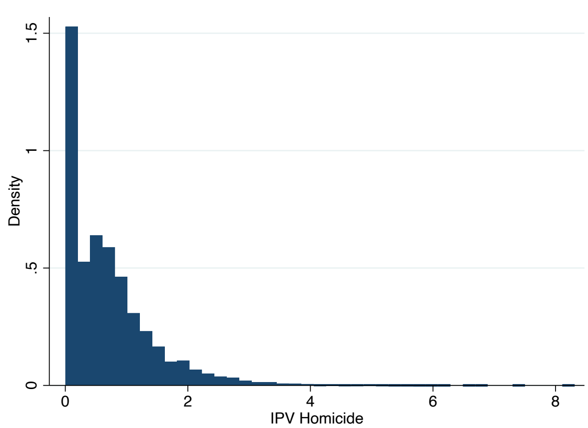
(b) DV (agency level)



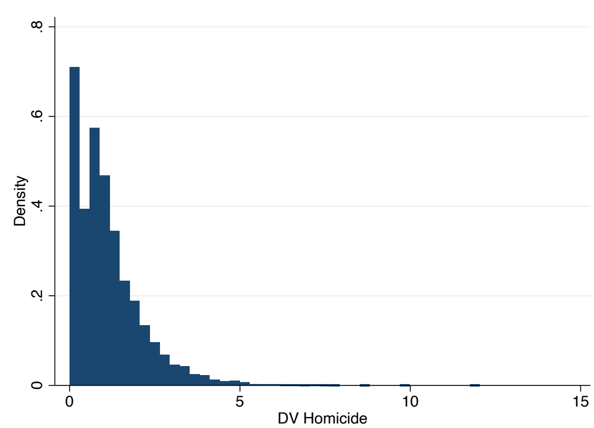
(c) IPV(county level)



(d) DV(county level)

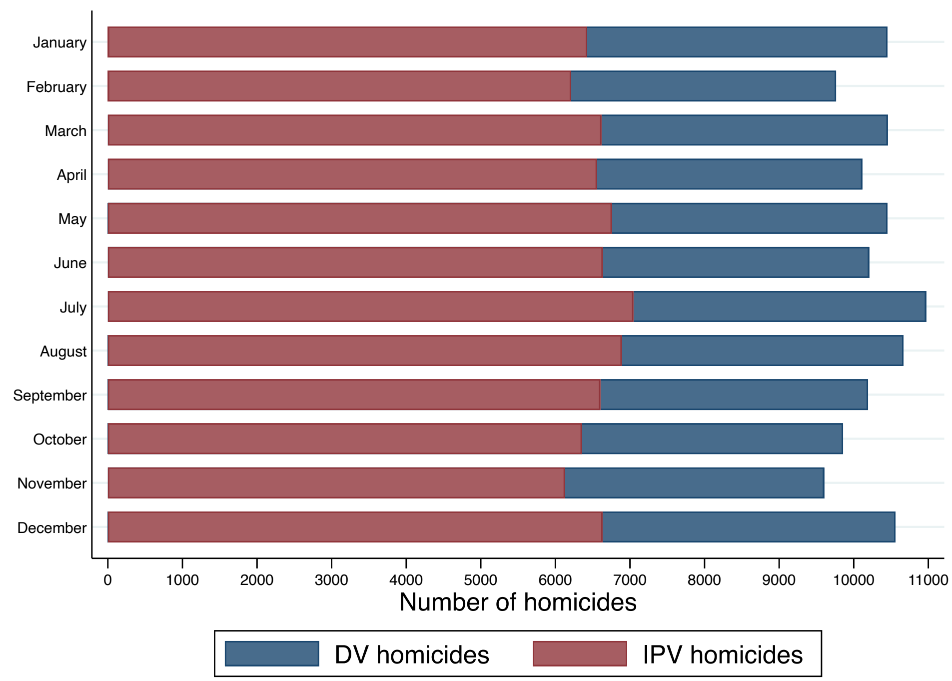


(e) IPV (CBSA level, excluding rural areas)

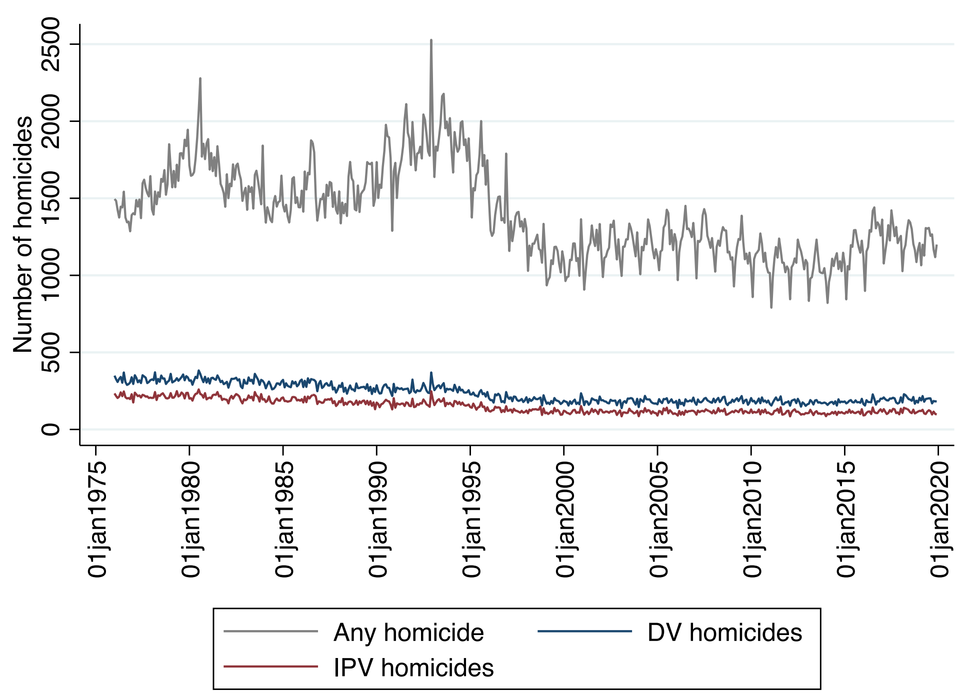


(f) DV (CBSA level, excluding rural areas)

Figure B3: Temporal variation in homicide counts

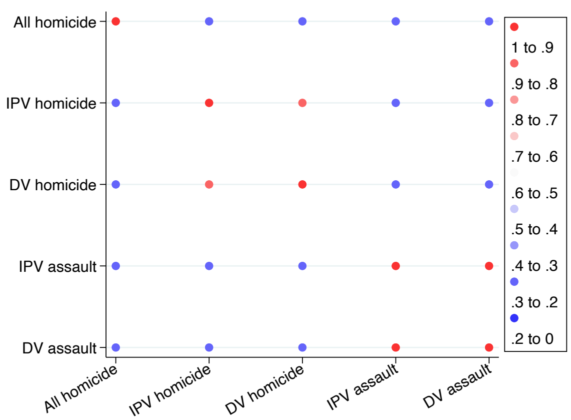


(a) By month

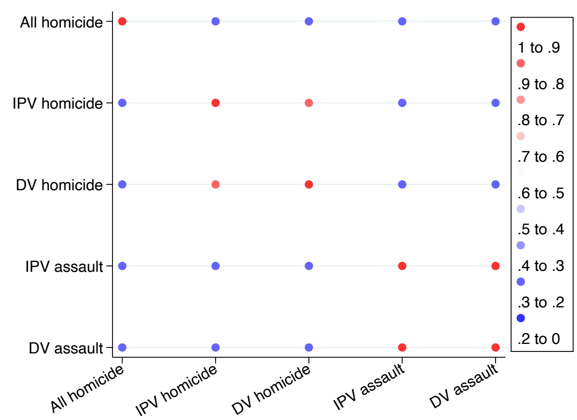


(b) By month and year

Figure B4: Correlation between lethal and non-lethal outcome variables



(a) County level (all counties with crime data)



(b) County level (counties with shelter bed data)

Table B1: Balance tests

Variable	(1) All Other Counties N	(1) All Other Counties Mean/SE	(2) Respondent N	(2) Respondent Mean/SE	T-test Difference (1)-(2)
IPV Homicide	28079	1.336 (0.023)	5994	0.817 (0.021)	0.519***
DV Homicide	28079	2.145 (0.028)	5994	1.368 (0.030)	0.777***
IPV assault	6509	42.834 (1.618)	1460	39.719 (3.023)	3.114
DV assault	6509	60.801 (2.079)	1460	54.847 (3.796)	5.954
Population	28079	1.11e+05 (1153.663)	5994	3.84e+05 (11810.881)	-2.73e+05***
Poverty Rate	28079	14.317 (0.021)	5994	13.468 (0.043)	0.849***
County unemployment rate	28079	6.571 (0.018)	5994	5.987 (0.034)	0.584***
State unemployment rate	28079	5.775 (0.010)	5994	5.739 (0.023)	0.037
Annual Average Pay	28079	29451.541 (59.707)	5994	33050.581 (141.526)	-3599.040***
Total SNAP issuance	28079	1.16e+06 (16359.458)	5994	4.07e+06 (1.57e+05)	-2.91e+06***
Governor is Democrat (1=Yes)	28079	0.432 (0.003)	5994	0.434 (0.006)	-0.002

Variable	(1) Counties without shelter N	(1) Counties without shelter Mean/SE	(2) Counties with shelter N	(2) Counties with shelter Mean/SE	T-test Difference (1)-(2)
IPV Homicide	14752	1.729 (0.040)	19333	0.876 (0.013)	0.853***
DV Homicide	14752	2.766 (0.050)	19333	1.431 (0.017)	1.335***
IPV assault	3532	42.017 (2.219)	4439	42.440 (1.871)	-0.422
DV assault	3532	59.534 (2.899)	4439	59.823 (2.352)	-0.289
Population	14752	43137.952 (578.709)	19333	2.47e+05 (4009.772)	-2.04e+05***
Poverty Rate	14752	14.657 (0.030)	19333	13.796 (0.024)	0.860***
County unemployment rate	14748	6.802 (0.025)	19325	6.213 (0.020)	0.589***
State unemployment rate	14752	5.722 (0.014)	19333	5.805 (0.013)	-0.083***
Annual Average Pay	14752	27404.981 (72.907)	19333	32136.345 (77.625)	-4731.364***
Governor is Democrat (1=Yes)	14752	0.420 (0.004)	19333	0.442 (0.004)	-0.022***
Total SNAP issuance	14752	4.74e+05 (6314.422)	19333	2.59e+06 (54013.303)	-2.12e+06***

The values displayed for t-tests are the difference in means across groups. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level.

B.2 Alternative units of analysis

Table B2: OLS regression: per-capita bed counts on homicide (aggregated to the CBSA)

VARIABLES	(1) IPV Homicide	(2) IPV Homicide	(3) DV Homicide	(4) DV Homicide	(5) IPV Homicide	(6) IPV Homicide	(7) DV Homicide	(8) DV Homicide
Bed count	-0.00112	-0.0131	0.00208	-0.00552	0.00208	-0.000597	0.0206*	0.0185
	(0.00576)	(0.00809)	(0.00599)	(0.0103)	(0.0109)	(0.0123)	(0.0115)	(0.0136)
Bed count = L,	0.00551	0.0132	0.00363	0.00347	-0.00567	-0.00562	-0.00566	-0.0109
	(0.00607)	(0.00932)	(0.00695)	(0.0106)	(0.0109)	(0.0124)	(0.0115)	(0.0141)
Homicide (not IPV) = L,	0.0204***	0.0185**			0.0850***	0.0861***		
	(0.00604)	(0.00808)			(0.0268)	(0.0261)		
Homicide (not DV) = L,			0.0342***	0.0273***			0.175**	0.188***
			(0.00768)	(0.0100)			(0.0704)	(0.0648)
Observations	4,835	3,478	4,835	3,478	5,487	5,152	6,621	5,325
R-squared	0.335	0.538	0.381	0.578	0.267	0.454	0.347	0.530
Observation	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA
CBSA FE	X	X	X	X	X	X	X	X
Year FE	X	-	X	-	X	-	X	-
State x Year FE	-	X	-	X	-	X	-	X
Controls	X	X	X	X	X	X	X	X
Rural Counties	-	-	-	-	X	X	X	X
Standard Errors	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA	CBSA
Outcome mean	0.664	0.694	1.078	1.118	0.853	0.872	1.330	1.417

Observations are at the CBSA level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts, lagged bed counts, lagged dependent variables, and lagged homicide, are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B3: OLS regression: per-capita bed counts on homicide (agency level)

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) DV homicide	(4) DV homicide
Bed count	-0.00713	-0.00868	-0.00132	0.0125
	(0.0102)	(0.0138)	(0.0127)	(0.0196)
Bed count = L,	0.00930	0.00586	0.00864	0.0165
	(0.0108)	(0.0156)	(0.0133)	(0.0206)
Homicide (not IPV) = L,	-0.00180	-0.0105**		
	(0.00152)	(0.00432)		
Homicide (not DV) = L,			0.000677	-0.00460
			(0.00364)	(0.00609)
Observations	11,848	11,947	11,848	11,947
R-squared	0.250	0.159	0.277	0.199
Observation	Agency	Agency	Agency	Agency
Agency FE	X	-	X	-
County FE	-	X	-	X
Year FE	X	-	X	-
State x Year FE	-	X	-	X
Controls	X	X	X	X
Standard Errors	County	County	County	County
Outcome mean	0.967	1.006	1.586	1.639

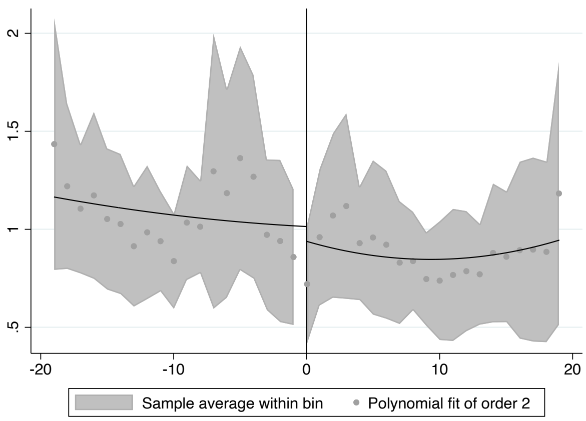
Observations are at the county or agency level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts, lagged bed counts, and lagged homicide, are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than \$10,000; households with income \$10,000 to \$19,999; county-level unemployment; households with income \$20,000 to \$29,999; and households with income \$30,000 to \$44,999; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B4: OLS regression: per-capita bed counts on non-lethal IPV (agency-level)

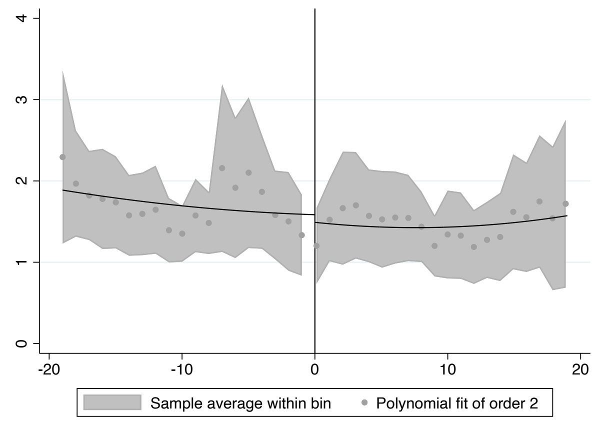
VARIABLES	(1) IPV assault	(2) IPV agg. assault	(3) IPV simple assault	(4) IPV intimidation	(5) DV assault	(6) DV agg. assault	(7) DV simple assault	(8) DV intimidation
Bed count	-3.350	-0.278	-1.147	-0.00403	-4.458	-0.271	-1.842	-0.00360
	(2.508)	(0.283)	(1.178)	(0.0678)	(3.503)	(0.340)	(1.644)	(0.0790)
IPV homicide = L,	-6.210	-0.317	-2.204	-0.208				
	(4.559)	(0.296)	(3.523)	(0.208)				
IPV assault = L,	-0.0845*							
	(0.0453)							
IPV agg. assault = L,		-0.112***						
		(0.0165)						
IPV simple assault = L,			-0.0654					
			(0.0432)					
IPV intimidation = L,				-0.128***				
				(0.0308)				
DV homicide = L,					-4.971	-0.181	-2.125	-0.131
					(4.245)	(0.319)	(3.028)	(0.229)
DV assault = L,					-0.0795**			
					(0.0393)			
DV agg. assault = L,						-0.111***		
						(0.0158)		
DV simple assault = L,							-0.0669*	
							(0.0396)	
DV intimidation = L,								-0.123***
								(0.0252)
Observations	1,324	1,324	1,324	1,324	1,324	1,324	1,324	1,324
R-squared	0.171	0.148	0.156	0.171	0.161	0.143	0.155	0.192
Observation	Agency	Agency	Agency	Agency	Agency	Agency	Agency	Agency
Agency FE	X	X	X	X	X	X	X	X
County FE	-	-	-	-	-	-	-	-
Year FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County	County	County
Outcome mean	63.75	2.200	33.69	1.125	81.30	3.026	42.16	1.543

Observations are at the agency level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of incidents of IPV assault, sexual assault, or intimidation per 100,000 population. Bed counts, lagged bed counts, lagged dependent variables, and lagged homicide, are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than \$10,000; households with income \$10,000 to \$19,999; households with income \$20,000 to \$29,999; and households with income \$30,000 to \$44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

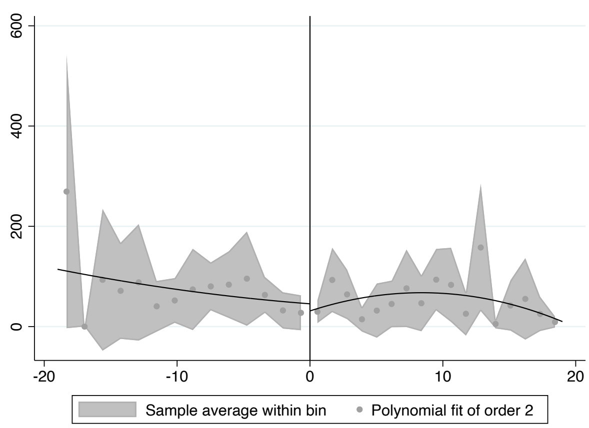
Figure B5: Structural break specification (baseline): agency-level



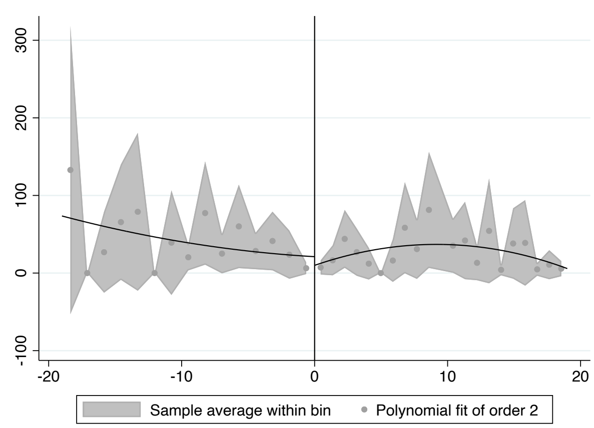
(a) IPV homicide (agency-level)



(b) DV homicide (agency-level)



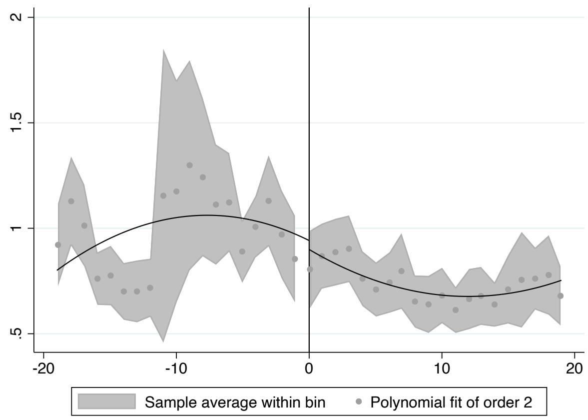
(c) All IPV assault (agency-level)



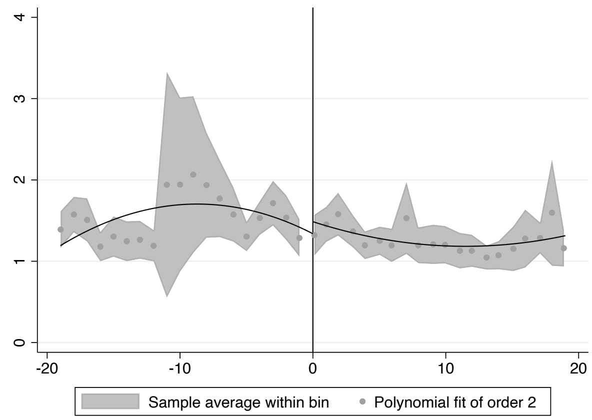
(d) IPV simple assault (agency-level)

B.3 Robustness tests

Figure B6: Structural break: baseline specification on the 3-year average homicide rate

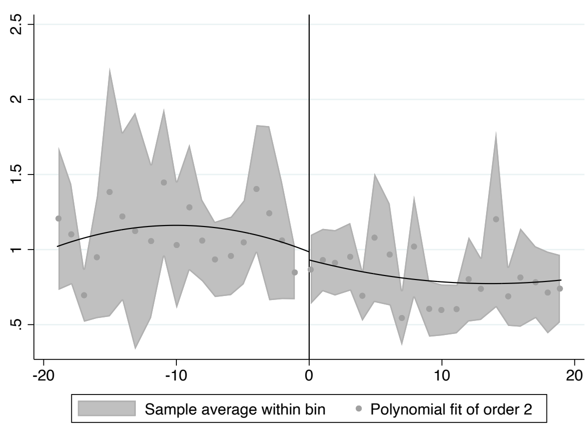


(a) IPV Homicide

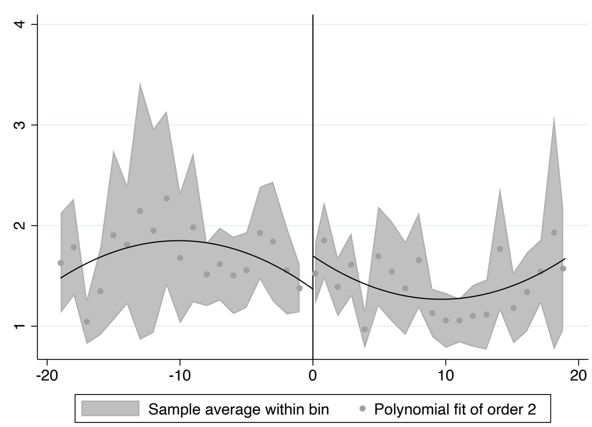


(b) DV homicide

Figure B7: Structural break: specification including counties with no changes to capacity



(a) IPV Homicide



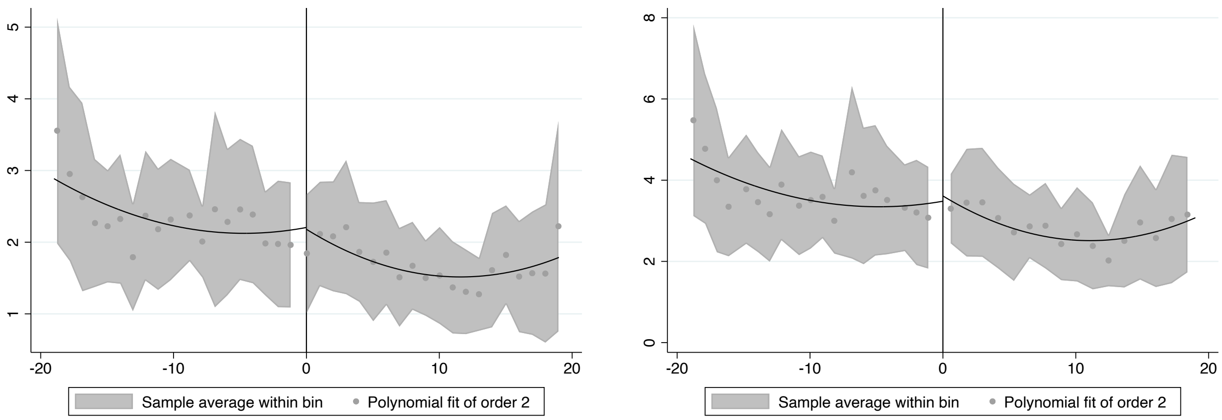
(b) DV homicide

Table B5: OLS regression of shelter presence on IPV or DV homicide rate, including counties identified as having no probable shelter

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) DV homicide	(4) DV homicide
Shelter in county	-0.271***	-0.215***	-0.429***	-0.357***
	(0.0465)	(0.0482)	(0.0659)	(0.0694)
Homicide (not IPV) = L,	0.0642***	0.0592***		
	(0.0116)	(0.0115)		
Homicide (not DV) = L,			0.102***	0.0960***
			(0.0240)	(0.0219)
Observations	13,636	13,544	13,636	13,544
R-squared	0.063	0.118	0.081	0.143
County FE	-	-	-	-
Year FE	X	-	X	-
State x Year FE	-	X	-	X
Controls	X	X	X	X
Standard Errors	County	County	County	County
Outcome mean	1.188	1.193	1.906	1.914

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts are not per-capita. Lagged homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than \$10,000; households with income \$10,000 to \$19,999; households with income \$20,000 to \$29,999; and households with income \$30,000 to \$44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Figure B8: Structural break: IV specification



(a) IPV Homicide

(b) DV homicide

Table B6: OLS regression interacting with bins for population

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) IPV homicide	(4) IPV homicide	(5) DV homicide	(6) DV homicide	(7) DV homicide	(8) DV homicide
Bed count	-0.00809	-0.00459	-0.00862*	-0.00513	0.0123	0.0165	0.0106	0.0151
	(0.00527)	(0.00639)	(0.00506)	(0.00619)	(0.00897)	(0.0103)	(0.00871)	(0.0101)
Bed count = L,	0.00376	0.000425	0.00386	0.000598	-0.00753	-0.0117	-0.00698	-0.0112
	(0.00501)	(0.00629)	(0.00505)	(0.00631)	(0.00858)	(0.0104)	(0.00855)	(0.0105)
Population $\geq$ 100,000	0.0704	0.0637			0.187	0.185		
	(0.182)	(0.208)			(0.235)	(0.247)		
Population $\geq$ 100,000 * bed count	-0.000409	-0.00176			-0.00880	-0.00968		
	(0.00543)	(0.00567)			(0.00679)	(0.00767)		
Homicide (not IPV) = L,	0.0862***	0.0700***	0.0866***	0.0702***				
	(0.0247)	(0.0175)	(0.0244)	(0.0173)				
Population $\geq$ 200,000			-0.136	-0.0947			-0.0742	-0.0708
			(0.130)	(0.136)			(0.209)	(0.231)
Population $\geq$ 200,000 * bed count			0.0175*	0.0113			0.0160	0.00719
			(0.0100)	(0.00882)			(0.0189)	(0.0147)
Homicide (not DV) = L,					0.181***	0.145***	0.182***	0.145***
					(0.0678)	(0.0438)	(0.0674)	(0.0435)
Observations	4,986	4,834	4,986	4,834	4,986	4,834	4,986	4,834
R-squared	0.296	0.439	0.297	0.440	0.377	0.515	0.377	0.515
County FE	X	X	X	X	X	X	X	X
Year FE	X	-	X	-	X	-	X	-
State x Year FE	-	X	-	X	-	X	-	X
Controls	X	X	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County	County	County
Outcome mean	0.794	0.798	0.794	0.798	1.315	1.324	1.315	1.324

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts are not per-capita. Lagged homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than \$10,000; households with income \$10,000 to \$19,999; households with income \$20,000 to \$29,999; and households with income \$30,000 to \$44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B7: OLS regression interacting with dummy for shelter opening

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) IPV homicide	(4) IPV homicide	(5) DV homicide	(6) DV homicide	(7) DV homicide	(8) DV homicide
Bed count	-0.00832*	-0.0130***	-0.00451	-0.00431	-0.00444	-0.00258	0.00385	0.00427
	(0.00438)	(0.00422)	(0.00410)	(0.00441)	(0.00508)	(0.00632)	(0.00420)	(0.00494)
Bed count = L,		0.00473				-0.00205		
		(0.00461)				(0.00607)		
Opening year	0.0348	0.0426	0.0472	-0.358	-0.351	-0.342	-0.300	-0.514
	(0.174)	(0.178)	(0.166)	(0.302)	(0.329)	(0.335)	(0.278)	(0.380)
Opening year * bed count	-0.00364	0.000877	-0.00479	0.00716	0.0126	0.0105	0.0171	0.0233
	(0.00662)	(0.00781)	(0.00691)	(0.0102)	(0.0156)	(0.0167)	(0.0164)	(0.0194)
Homicide (not IPV) = L,			0.0862***	0.0697***				
			(0.0247)	(0.0174)				
Homicide (not DV) = L,							0.181***	0.145***
							(0.0677)	(0.0437)
Observations	5,984	5,957	4,986	4,834	5,984	5,957	4,986	4,834
R-squared	0.220	0.215	0.296	0.440	0.231	0.228	0.377	0.515
County FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	-	X	X	X	-
State x Year FE	-	-	-	X	-	-	-	X
Controls	X	X	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County	County	County
Outcome mean	0.818	0.817	0.794	0.798	1.369	1.369	1.315	1.324

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts are not per-capita. Lagged homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than \$10,000; households with income \$10,000 to \$19,999; households with income \$20,000 to \$29,999; and households with income \$30,000 to \$44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B8: OLS regression interacting bed count with county-level unemployment rate

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) DV homicide	(4) DV homicide
Bed count	-0.000412	-0.000678	0.0181	0.0176
	(0.00580)	(0.00628)	(0.0127)	(0.0148)
Bed count = L,	0.00359	0.000351	-0.00726	-0.0114
	(0.00507)	(0.00628)	(0.00852)	(0.0104)
County unemplopyment * bed count	-0.00120	-0.000616	-0.00110	-0.000350
	(0.000823)	(0.000858)	(0.00141)	(0.00171)
Homicide (not IPV) = L,	0.0861***	0.0699***		
	(0.0248)	(0.0175)		
County unemployment rate	0.0212	-0.00662	-0.0435	-0.0801*
	(0.0312)	(0.0329)	(0.0375)	(0.0448)
Homicide (not DV) = L,			0.181***	0.145***
			(0.0677)	(0.0435)
Observations	4,986	4,834	4,986	4,834
R-squared	0.297	0.440	0.377	0.515
County FE	X	X	X	X
Year FE	X	-	X	-
State x Year FE	-	X	-	X
Controls	X	X	X	X
Standard Errors	County	County	County	County
Outcome mean	0.794	0.798	1.315	1.324

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts are not per-capita. Lagged homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B9: OLS regression: per-capita bed counts on running IPV or DV homicide rate

	(1)	(2)	(3)	(4)
VARIABLES	Running IPV homicide rate (3yr)	Running IPV homicide rate (5yr)	Running DV homicide rate (3yr)	Running DV homicide rate (5yr)
Bed count	-0.00590	-0.00501	-0.00201	-0.00374
	(0.00469)	(0.00360)	(0.00697)	(0.00476)
Bed count = L,	0.00114	0.00130	0.00351	0.00138
	(0.00500)	(0.00328)	(0.00655)	(0.00436)
Homicide (not IPV) = L,	0.0821**	0.0289***		
	(0.0351)	(0.00920)		
Homicide (not DV) = L,			0.165*	0.0521**
			(0.0861)	(0.0209)
Observations	4,401	3,539	4,401	3,539
R-squared	0.563	0.838	0.590	0.880
County FE	X	X	X	X
Year FE	X	-	X	-
State x Year FE	-	X	-	X
Controls	X	X	X	X
Standard Errors	County	County	County	County
Outcome mean	0.811	0.826	1.321	1.329

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is the 3- or 5- year running average of either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts, lagged bed counts, lagged dependent variables, and lagged homicide, are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B10: Binned outcome variable regression (OLS)

VARIABLES	(1) Δ IPV rate	(2) Δ IPV rate	(3) Δ IPV rate	(4) Δ IPV rate	(5) Δ DV rate	(6) Δ DV rate	(7) Δ DV rate	(8) Δ DV rate
Bed count	-0.00114	-0.00261	-0.00115	-0.000387	-0.000918	-0.00521	-0.000950	-0.000145
	(0.00105)	(0.00279)	(0.00105)	(0.00124)	(0.00131)	(0.00340)	(0.00131)	(0.00149)
Bed count = L,		0.00167				0.00485		
		(0.00271)				(0.00322)		
Homicide (not IPV) = L,			0.00256	0.00393				
			(0.00353)	(0.00324)				
Homicide (not DV) = L,							0.00373	0.00368
							(0.00641)	(0.00398)
Observations	4,986	4,986	4,986	4,834	4,986	4,986	4,986	4,834
R-squared	0.039	0.039	0.039	0.201	0.042	0.043	0.043	0.209
County FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	-	X	X	X	-
State x Year FE	-	-	-	X	-	-	-	X
Controls	X	X	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County	County	County
Outcome mean	-0.0786	-0.0786	-0.0786	-0.0784	-0.0959	-0.0959	-0.0959	-0.0960

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. The outcome variable is an indicator equal to 1 if the relevant homicide rate increased, -1 if it decreased, and 0 if it did not change. Bed counts are not per-capita. Lagged homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999 ;county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B11: Main specifications, using all homicide as dependent variable

VARIABLES	(1) Homicide (all)	(2) Homicide (all)	(3) Homicide (all)	(4) Homicide (not IPV)	(5) Homicide (not IPV)	(6) Homicide (not IPV)	(7) Homicide (not DV)	(8) Homicide (not DV)	(9) Homicide (all)
Bed count	0.0215	0.0203	0.00521	0.0282*	0.0293**	0.00735	0.00995	0.00464	0.00219
	(0.0170)	(0.0139)	(0.0161)	(0.0161)	(0.0130)	(0.0149)	(0.0123)	(0.0121)	(0.0166)
Bed count = L,	-0.00205	0.000757	0.0167	-0.00551	-0.00491	0.0108	0.00201	0.00718	0.0152
	(0.0171)	(0.0132)	(0.0148)	(0.0160)	(0.0124)	(0.0138)	(0.0121)	(0.0116)	(0.0153)
Homicide (all) = L,	0.560***	0.560***	0.565***						
	(0.107)	(0.0107)	(0.0117)						
Net change in beds (5yr)		-0.00419			-0.00523			0.00134	
		(0.00670)			(0.00630)			(0.00587)	
Net change in beds (10yr)			-0.0108*			-0.00947			-0.00934
			(0.00631)			(0.00588)			(0.00652)
Homicide (not IPV) = L,					0.532***	0.543***			
					(0.0114)	(0.0123)			
Homicide (not DV) = L,							0.493***	0.495***	0.683***
							(0.119)	(0.0119)	(0.0154)
Observations	4,986	4,914	4,082	4,986	4,914	4,082	4,986	4,914	4,082
R-squared	0.780	0.780	0.786	0.778	0.778	0.786	0.784	0.784	0.772
County FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County	County	County	County
Outcome mean	6.622	6.636	6.364	5.828	5.841	5.628	5.307	5.316	6.364

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of homicide per 100,000 population. Lagged homicide is per-capita. Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B12: OLS regression: predicting treatment and outcomes using controls

VARIABLES	(1) Bed count	(2) IPV homicide	(3) DV homicide
Population	-4.62e-07	-1.78e-06	-3.98e-06
	(2.13e-05)	(1.63e-06)	(2.66e-06)
Poverty Rate	-0.212	-0.00181	0.0120
	(0.213)	(0.0142)	(0.0184)
State unemployment rate	0.648	-0.0374	0.0128
	(0.717)	(0.0579)	(0.0698)
Annual Average Pay	-0.000846**	-3.82e-06	1.35e-05
	(0.000334)	(1.30e-05)	(2.25e-05)
Governor is Democrat (1=Yes)	-0.556	-0.0823	-0.1000
	(0.970)	(0.0753)	(0.137)
Fraction of State House that is Democrat	17.86**	0.763	0.995
	(9.053)	(0.595)	(0.911)
Fraction of State Senate that is Democrat	-16.51*	-0.0683	-0.171
	(8.519)	(0.553)	(0.841)
SNAP Total PA/Non-PA Issuance	-1.14e-07*	4.11e-09	6.03e-09
	(6.06e-08)	(3.36e-09)	(5.76e-09)
White non-Hispanic population	1.06e-05	0.00188	0.00411
	(0.0225)	(0.00194)	(0.00326)
Black non-Hispanic population	0.0148	0.00205	0.00360
	(0.0310)	(0.00166)	(0.00249)
American Indian/Alaska Native population	-0.415	0.000562	0.000681
	(0.332)	(0.0200)	(0.0263)
Asian American/Pacific Islander population	0.0177	0.00191	0.00329
	(0.0308)	(0.00145)	(0.00206)
Hispanic/Latinx population	0.0331	0.00106	0.00283
	(0.0276)	(0.00192)	(0.00306)
Federal military employment	0.226	0.0140*	0.0201
	(0.208)	(0.00845)	(0.0129)
State & local government employment	-0.296**	0.0106*	0.0181**
	(0.140)	(0.00559)	(0.00892)
Federal military earnings	-0.00383*	-0.000177*	-0.000298*
	(0.00229)	(0.000101)	(0.000178)
State & local government earnings	-0.000335	-8.88e-06	-3.13e-05
	(0.00112)	(5.26e-05)	(9.25e-05)
Woods & Poole Economics Wealth Index	0.487***	-0.00118	0.000217
	(0.141)	(0.00403)	(0.00602)
Persons per household	-36.91***	-0.671	-1.202*
	(11.96)	(0.435)	(0.705)
No. households w/income less than 10k	0.279	0.00970	0.00963
	(0.173)	(0.00725)	(0.0110)
No. households w/income 10k-20k	-0.277	0.00605	0.00552
	(0.203)	(0.00967)	(0.0134)
No. households w/income 20k-30k	0.279	-0.0180	-0.00672
	(0.247)	(0.0134)	(0.0217)
No. households w/income 30k-45k	-0.173	-0.00111	-0.00602
	(0.215)	(0.0137)	(0.0210)
County unemployment rate	-0.516	0.0320	-0.0171
	(0.707)	(0.0369)	(0.0535)
Observations	8,627	5,984	5,984
R-squared	0.772	0.216	0.230
County FE	X	X	X
Year FE	X	X	X
State x Year FE	-	-	-
Standard Errors	County	County	County

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level.

Table B13: Using lethal IPV or DV to predict non-lethal DV and IPV

VARIABLES	(1) IPV assault	(2) IPV agg. assault	(3) DV assault	(4) DV agg. assault
IPV homicide	0.665	-0.0808		
	(4.937)	(0.300)		
Homicide (not IPV) = L,	-1.004	-0.0517		
	(0.922)	(0.0752)		
DV homicide			-0.922	-0.260
			(4.556)	(0.285)
Homicide (not DV) = L,			-1.552	-0.0659
			(1.298)	(0.108)
Observations	1,181	1,086	1,181	1,086
R-squared	0.159	0.200	0.164	0.250
County FE	X	X	X	X
Year FE	X	-	X	-
State x Year FE	-	X	-	X
Controls	X	X	X	X
Standard Errors	County	County	County	County
Outcome mean	42.66	1.484	58.71	2.210

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV or DV assault or aggravated assault per 100,000 population. Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B14: Binned difference-in-differences regression

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) DV homicide	(4) DV homicide
Bed count	-0.0185	-0.0272	-0.0115	-0.0147
	(0.0141)	(0.0194)	(0.0177)	(0.0224)
Bed count = L,	0.0139	0.0223	0.0149	0.0179
	(0.0136)	(0.0190)	(0.0173)	(0.0223)
Change $\leq$ -5	-0.0734	-0.509	0.0916	-0.214
	(0.229)	(0.321)	(0.379)	(0.433)
Change $\in$ (-5,5)	-0.130	-0.0538	-0.877**	-0.848*
	(0.301)	(0.385)	(0.411)	(0.462)
Change $\geq$ 5	0.0350	-0.210	-0.199*	-0.354**
	(0.0816)	(0.149)	(0.106)	(0.176)
Bed count * Change $\leq$ -5	-0.0131	0.00681	-0.0242	-0.00899
	(0.0181)	(0.0206)	(0.0324)	(0.0330)
Bed count * Change $\in$ (-5,5)	-0.0130	-0.00827	0.0585***	0.0664***
	(0.0122)	(0.0139)	(0.0160)	(0.0169)
Bed count * Change $\geq$ 5	0.00796	0.0248	0.0231**	0.0344**
	(0.0104)	(0.0163)	(0.00937)	(0.0157)
Homicide (not IPV) = L,	0.0862***	0.0697***		
	(0.0248)	(0.0174)		
Homicide (not DV) = L,			0.182***	0.145***
			(0.0675)	(0.0433)
Observations	4,986	4,834	4,986	4,834
R-squared	0.297	0.440	0.378	0.516
County FE	X	X	X	X
Year FE	X	-	X	-
State x Year FE	-	X	-	X
Controls	X	X	X	X
Standard Errors	County	County	County	County
Outcome mean	0.794	0.798	1.315	1.324

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Bed counts, lagged bed counts, and lagged homicide are all per-capita. Lagged homicide excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B15: Main specifications, controlling for the lagged dependent variable

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) IPV homicide	(4) DV homicide	(5) DV homicide	(6) DV homicide
Bed count	-0.00626	-0.0104**	-0.00439	0.0135	0.0138**	0.00411
	(0.00519)	(0.00434)	(0.00487)	(0.00961)	(0.00625)	(0.00721)
Bed count = L,	0.00246	0.00582	0.00554	-0.00693	-0.00786	-0.000701
	(0.00528)	(0.00414)	(0.00450)	(0.00960)	(0.00597)	(0.00666)
Homicide (not IPV) = L,		0.0837***	0.0776***			
		(0.00400)	(0.00429)			
Net change in beds (5yr)		0.00108			-0.00426	
		(0.00210)			(0.00303)	
IPV homicide = L,	0.123	0.0294**	0.0563***			
	(0.133)	(0.0139)	(0.0155)			
Net change in beds (10yr)			-0.000725			-0.00211
			(0.00192)			(0.00284)
DV homicide = L,				0.268	0.136***	0.151***
				(0.191)	(0.0141)	(0.0161)
Homicide (not DV) = L,					0.156***	0.156***
					(0.00670)	(0.00750)
Observations	4,986	4,914	4,082	4,986	4,914	4,082
R-squared	0.230	0.297	0.316	0.317	0.390	0.427
County FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Standard Errors	County	County	County	County	County	County
Outcome mean	0.794	0.795	0.736	1.315	1.320	1.242

Observations are at the county level. Cluster-robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

Table B16: Main specifications, using the Arellano-Bond estimator

VARIABLES	(1) IPV homicide	(2) IPV homicide	(3) IPV homicide	(4) DV homicide	(5) DV homicide	(6) DV homicide
Bed count	-0.00814	0.000643	0.00375	0.00435	0.00447	0.00344
	(0.0146)	(0.00527)	(0.00588)	(37,850)	(0.0182)	(0.0207)
Net change in beds (5yr)		-0.00160			-0.00273	
		(0.00228)			(0.00646)	
Homicide (not IPV) = L,	0.0697***	0.0706***	0.0705***			
	(0.0221)	(0.0225)	(0.0229)			
IPV homicide = L,	-0.0155	-0.0127	0.00457			
	(0.0607)	(0.0620)	(0.0496)			
Net change in beds (10yr)			-0.00305			-0.00374
			(0.00196)			(0.00618)
Homicide (not DV) = L,				0.112	0.113***	0.126***
				(88,099)	(0.0342)	(0.0393)
DV homicide = L,				0.216	0.203**	0.191*
				(154,159)	(0.0974)	(0.101)
Observations	4,997	4,925	4,101	4,997	4,925	4,101
Number of county_fips	320	316	303	320	316	303
Model	AB	AB	AB	AB	AB	AB
State FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust
Outcome mean	0.795	0.798	0.734	1.318	1.324	1.243

Observations are at the county level. Robust standard errors are in parentheses. Single asterisk (*) represents significance at the 10% level; two asterisks (**) represent significance at the 5% level; and three asterisks (***) represent significance at the 1% level. Outcome variable is either the number of victims of IPV homicide or DV homicide per 100,000 population. Homicide is per-capita and excludes homicide classified as being of the dependent variable type (e.g. IPV or DV). Suppressed control variables: county-level population; white population; Black population; American Indian/Alaska Native population; Asian American/Pacific Islander population; Hispanic/Latinx population; county-level average annual pay; federal military employment; state/local government employment; federal military earnings; state/local government earnings; Woods & Poole Economic Wealth Index; persons per household; households with income less than $10,000; households with income $10,000 to $19,999; households with income $20,000 to $29,999; and households with income $30,000 to $44,999; county-level unemployment; county-level total SNAP issuance; state-level unemployment, an indicator for a Democratic state governor, the fraction of the state house that is Democratic, and the fraction of the state senate that is Democratic.

B.4 Supplemental materials

Table B17: Level of arrest discretion for suspected domestic violence offenders by state

Arrest law type	States
Discretionary	Alabama; Delaware; Florida; Georgia; Hawaii; Idaho; Illinois; Indiana; Kentucky; Maryland; Michigan; Minnesota; Missouri; Nebraska; North Carolina; Oklahoma; Pennsylvania; Texas; Vermont; West Virginia; Wyoming
Preferred	Arkansas; Montana; North Dakota; Tennessee
Mandatory	Alaska (1996); Arizona* (1991); California** (2012); Colorado (1994); Connecticut (1986); Iowa*,*** (1986); Kansas (1991); Louisiana (1985); Maine (1979); Massachusetts** (1990); Mississippi (1995); Nevada (1985); New Hampshire** (2000); New Jersey (1991); New York** (1994); Ohio (1994); Oregon (1977); Rhode Island (1988); South Carolina* (1995); South Dakota (1989); Utah (1995); Virginia (1996); Washington, D.C. (1991); Washington (1984); Wisconsin (1987)

Notes: South Carolina repealed mandatory arrest in 2015. For a list of relevant penal code sections, please contact the author directly.
* indicates mandatory if a deadly weapon is involved; arrest is otherwise discretionary.
** indicates mandatory specifically for violation of protection orders.
*** indicates mandatory if a physical injury has occurred.

Table B18: Laws directing law enforcement to identify a "primary aggressor" by state

Arrest law type	States
Primary aggressor	Alabama; Alaska; California; Colorado; Florida; Georgia; Iowa; Maryland; Missouri; Montana; Nevada; New Hampshire; New Jersey; New York; Ohio; Oregon; Rhode Island; South Carolina; South Dakota; Tennessee; Utah; Virginia; Washington; Wisconsin
Mutual aggressor	Arizona; Arkansas; Connecticut; Delaware; Hawaii; Idaho; Illinois; Indiana; Kansas; Kentucky; Louisiana; Maine; Massachusetts; Michigan; Minnesota; Mississippi; Nebraska; New Mexico; North Carolina; North Dakota; Oklahoma; Pennsylvania; Texas; Vermont; Washington, D.C.; West Virginia; Wyoming

Notes: Primary aggressor laws require responding officers to identify the primary aggressor of a DV/IPV incident when more than one individual involved alleges violence. The primary aggressor is the individual arrested (if any) under these laws. Mutual aggressor laws allow the arrest of any aggressor. For a list of relevant penal code sections, please contact the author directly. Source: (Hirschel et al., 2007).

Table B19: Gun ownership laws for domestic violence offenders by state

Firearm law type	States
Misdemeanor ban (DV)	All states, effective 1996 (18 U.S.C. §922(g)(8), (9))
Misdemeanor seizure	California; Colorado; Connecticut; Hawaii; Illinois; Iowa; Louisiana; Maryland; Massachusetts; Minnesota; Nevada; New Jersey; New York; Oregon; Pennsylvania; Rhode Island; Tennessee
TPO ban	Arizona^A; California; Colorado; Hawaii; Illinois; Maine^A; Massachusetts; Michigan^A; Montana^A; Nebraska^A; New York; North Carolina; North Dakota^A; Pennsylvania^A; South Dakota^A; Texas; Washington DC; Washington; Wisconsin
TPO seizure	Alaska^A; Arizona^A; California; Colorado; Connecticut; Delaware^A; Hawaii; Illinois; Iowa^A; Louisiana; Maryland; Massachusetts; Minnesota; Nevada^A; New Hampshire; New Jersey; New Mexico; New York; North Carolina; North Dakota^A; Oregon; Pennsylvania; Rhode Island^A; South Dakota^A; Tennessee; Vermont^A; Virginia; Washington; Wisconsin
Misdemeanors reported to NICS	Illinois; Massachusetts; Minnesota; New York
Waiting period for purchase	California* (10 days); Florida* (3 days); Hawaii* (14 days); Illinois* (3 days); Iowa*** (3 days); Maryland*** (7 days); Minnesota** (7 days); New Jersey*** (7 days); Rhode Island* (7 days); Washington DC* (10 days); Washington** (10 days)

Notes: For a list of relevant penal code sections, please contact the author directly. Misdemeanor bans on gun ownership/purchase by individuals convicted of misdemeanor DV charges. were codified in 1996. Misdemeanor seizure requires misdemeanants to surrender all firearms after conviction. TPO bans prevent gun ownership by anyone who is listed on a temporary protection order (*ex parte* protection orders). TPO seizure allows/requires law enforcement to take the firearms of anyone subject to a TPO. NICS is the National Instant Criminal Background Check System, maintained by the FBI. Waiting period for purchase prevents same-day sale and delivery of firearms.

^{A} indicates the ban or seizure is authorized but not required.

* indicates applies to all firearms.

** indicates applies to certain classes of firearms.

*** indicates applies only to handguns.

Appendix C

Pesticides increase pediatric cancer deaths: Evidence from Brazilian soy production

C.1 Summary statistics

Table C1: Number of hospitals treating pediatric oncology cases per state over the study period

	2005 - 2009	2010 - 2014	2015 - 2019	Pct increase
AC	1	1	1	0
AL	4	5	5	25
AM	2	2	4	100
AP	0	1	0	
BA	7	10	11	57
CE	8	10	10	25
DF	3	5	8	167
ES	4	6	7	75
GO	0	4	3	
MA	1	3	3	200
MG	26	30	31	19
MS	3	4	4	33
MT	4	5	3	-25
PA	2	3	4	100
PB	4	4	4	0
PE	8	9	7	-13
PI	1	1	1	0
PR	15	22	18	20
RJ	8	14	15	88
RN	5	6	6	20
RO	0	1	3	
RR	0	0	1	
RS	19	21	19	0
SC	15	16	15	0
SE	1	3	3	200
SP	47	51	40	-15
TO	2	2	2	0

Note: We construct data using the National Cancer Institute's Register of Cancer Hospitals (Portuguese acronym RHC) and identifying hospitals that treated at least five patients under 20 during the five year period.

Table C2 reports mean values and standard errors of our outcome, treatment, and control variables from 2005 to 2019. We separate municipalities into three groups: those in our sample (i.e., rural municipalities with at least 25% of area in agriculture in the Amazon and Cerrado), those in the Amazon and Cerrado that are either urban or do not have at least 25% of area in agriculture, and those outside the Amazon and Cerrado.

Municipalities in our sample produced an average of 41,526 tons of soy on 13,071 hectares. This corresponds to 0.11 tons per municipal hectare and 3.1% of the municipal area being in soy production. They had an average of 0.01 deaths from ALL per 10,000 (total) population for children under 5 or under 10. In total, 1% of observations (municipal-year) had any children under 5 die from ALL and 2% had any children under 10 die from ALL. Municipalities were 228,558 hectares with 41% of area still in natural vegetation and 5% of area in sugarcane cultivation. Their mean population was 17,765, and they had a GDP per capita of R$13,230.

In contrast, municipalities in the Amazon that are outside our sample were larger (871,052 hectares) with higher remaining forest (78%), larger populations (62,004 people), and lower GDP per capita (R$9,388). They produced less soy in total and dedicated less area to it. These differences exemplify that the municipalities outside of our sample were either very remote and forested or urban.

Outside of the Amazon, municipalities were far smaller (65,007 hectares) with higher population (36,664 people) and similar GDP per capita (R$13,025). While they produce less soy in total, they produce a higher amount of soy per municipal hectare (0.17 tons per hectare) and dedicate a larger portion of the municipality to soy cultivation (5%).

Rates of ALL (per 10,000 total population) are similar across the three groups, both for children under 5 and under 10. We are unable to normalize by population under 5 or 10 due to data limitations.

C.2 Tables of main results

C.3 Robustness tests

First, we test the effect of municipal-level soy production on pediatric cancer outcomes using a sample of municipalities in the Amazon and Cerrado that excludes those municipalities in three states that fall partially in the Cerrado biome: Bahia, Minas Gerais, and São Paulo. These states have far larger economies than the rest of our sample. The smallest of the three, Bahia, had a GDP of R$287 million in 2018; Mato Grosso

Figure C1: Boundaries of Level 3 Ottobasins and municipalities in the Brazilian Amazon and Cerrado

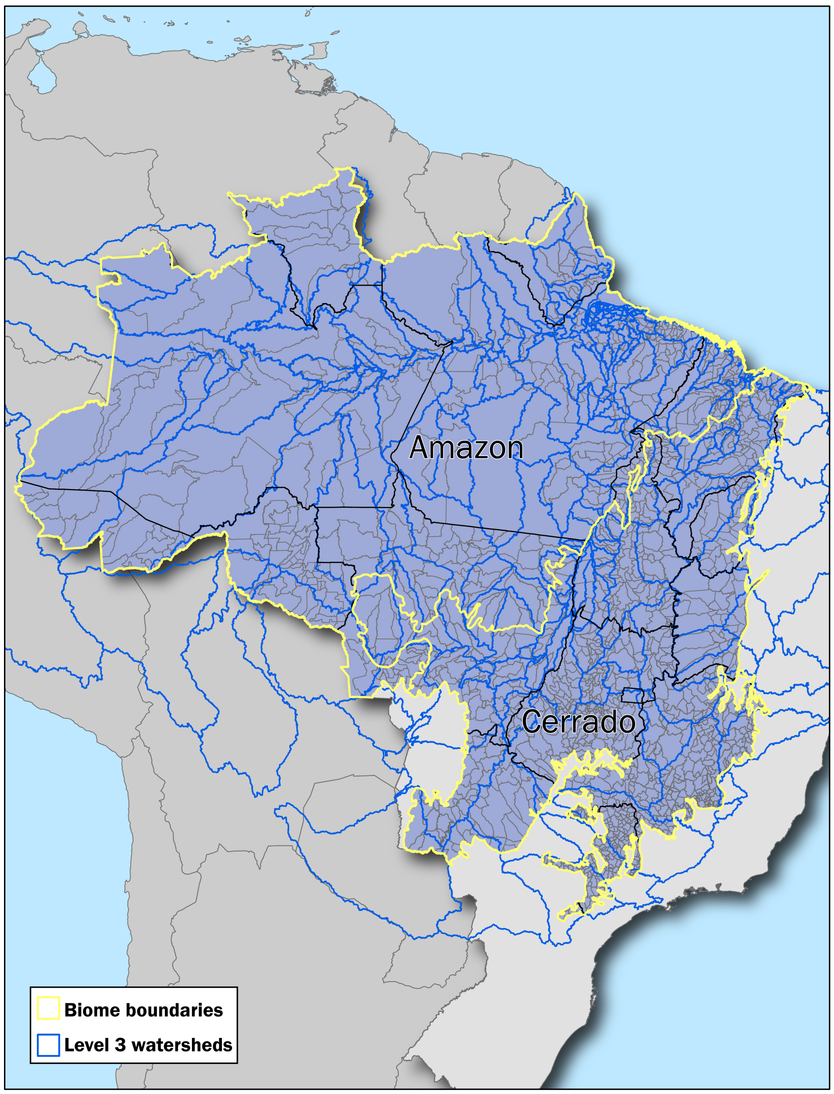


Table C2: Summary statistics of our sample and two excluded groups from 2004 - 2019

	Amazon & Cerrado		Other biomes
	In sample	Not in sample	Sample-like*
Treatments			
Tons of soy	41,526.27	13,758.53	9,614.22
	(152,563.49)	(54,881.50)	(38,090.47)
Tons soy per ha	0.11	0.03	0.17
	(0.25)	(0.10)	(0.44)
Soy (ha)	13,071.99	4,150.10	3,219.91
	(48,228.85)	(17,968.85)	(13,229.21)
Pct area in soy	3.10	0.78	5.39
	(7.83)	(2.84)	(14.04)
Outcomes			
ALL deaths under 4 per 10,000	0.007	0.007	0.005
	(0.115)	(0.065)	(0.088)
ALL deaths under 4 (0/1)	0.012	0.039	0.014
	(0.110)	(0.193)	(0.119)
ALL deaths under 9 per 10,000	0.014	0.015	0.011
	(0.142)	(0.096)	(0.124)
ALL deaths under 9 (0/1)	0.025	0.073	0.029
	(0.155)	(0.260)	(0.168)
Controls			
Pct area in natural vegetation	41.38	77.79	37.64
	(20.19)	(23.55)	(25.56)
Pct area in sugarcane	5.00	2.49	3.25
	(15.44)	(10.92)	(10.49)
Population	17,765.09	62,004.42	36,663.91
	(20,207.23)	(214,853.88)	(230,727.50)
GDP per capita	13,230.29	9,388.70	13,025.90
	(12,277.46)	(11,908.05)	(14,854.47)
Total area	228,558.05	871,052.19	65,007.05
	(302,508.23)	(1,707,202.65)	(146,888.98)
Observations	11,505	6,765	70,874

Note. We do not include GDP per capita in the model but list it here for descriptive purposes. *We limit these counties to those with comparable levels of rurality and agricultural production as our sample, although they are not included in our sample due to their location outside of the Amazon and Cerrado.

Table C3: Pediatric deaths from ALL relative to previous five year soy production

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
Pct area in soy	0.185**	0.160**	0.245**	0.236**
	(0.077)	(0.068)	(0.104)	(0.107)
Pct area in natural vegetation	-0.038	-0.035	-0.112*	-0.135
	(0.038)	(0.036)	(0.059)	(0.082)
Pct area in sugarcane	0.047	0.059	0.124***	-0.061
	(0.029)	(0.036)	(0.047)	(0.083)
Pct area in mining	14.411	37.242	9.720	19.187
	(9.626)	(22.762)	(8.198)	(11.629)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Soy production				
Tons soy per ha	0.027**	0.047**	0.047**	0.067**
	(0.013)	(0.021)	(0.023)	(0.030)
Pct area in natural vegetation	-0.037	-0.031	-0.109*	-0.129
	(0.039)	(0.036)	(0.058)	(0.082)
Pct area in sugarcane	0.034	0.055	0.111**	-0.066
	(0.028)	(0.036)	(0.044)	(0.081)
Pct area in mining	14.455	37.480	9.869	19.521*
	(9.619)	(22.685)	(8.182)	(11.556)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	8433	8433	8433	8433
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. We only show the effect on deaths per 10,000 as the linear probability model performs poorly given the wide variation in population after including urban municipalities. Results of the LPM are insignificant and are available from the authors upon request. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * $p< 0.10$, ** $p<0.05$, *** $p<0.01$.

Table C4: Pediatric deaths from ALL relative to previous five year production of all annual crops in the municipality

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Combined				
Pct area in soy + crops	0.053	0.044	0.140*	0.150*
	(0.052)	(0.048)	(0.070)	(0.076)
Pct area in natural vegetation	-0.037	-0.034	-0.107*	-0.130
	(0.039)	(0.037)	(0.058)	(0.082)
Pct area in sugarcane	0.037	0.049	0.127***	-0.054
	(0.028)	(0.039)	(0.042)	(0.091)
Pct area in mining	14.155	37.023	9.296	18.760
	(9.763)	(22.868)	(8.254)	(11.753)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Separated				
Pct area in soy	0.175**	0.151**	0.245**	0.239**
	(0.075)	(0.067)	(0.103)	(0.108)
Pct area in crops	-0.107	-0.097	0.001	0.033
	(0.082)	(0.067)	(0.071)	(0.092)
Pct area in natural vegetation	-0.042	-0.038	-0.112*	-0.133
	(0.040)	(0.037)	(0.059)	(0.082)
Pct area in sugarcane	0.034	0.047	0.124***	-0.056
	(0.026)	(0.035)	(0.047)	(0.089)
Pct area in mining	14.641	37.450	9.717	19.116
	(9.487)	(22.584)	(8.205)	(11.656)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	8433	8433	8433	8433
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * p< 0.10, ** p<0.05, *** p<0.01.

Table C5: Pediatric deaths from ALL relative to previous five year soy production in the Ottobasin

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
Pct area in soy	0.218*	0.150	0.457**	0.398*
	(0.127)	(0.175)	(0.197)	(0.237)
Pct area in natural vegetation	-0.279	-0.252	-0.239	-0.355
	(0.199)	(0.262)	(0.210)	(0.275)
Pct area in sugarcane	0.448	0.301	-0.157	-0.592
	(0.387)	(0.375)	(0.446)	(0.478)
Pct area in mining	8.354	36.896	13.617	51.706
	(9.306)	(24.037)	(12.961)	(38.036)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Soy production				
Tons soy per ha	0.076*	0.162	0.175*	0.223
	(0.040)	(0.121)	(0.093)	(0.163)
Pct area in natural vegetation	-0.287	-0.258	-0.256	-0.370
	(0.202)	(0.260)	(0.216)	(0.275)
Pct area in sugarcane	0.589	0.485*	0.151	-0.268
	(0.387)	(0.287)	(0.481)	(0.480)
Pct area in mining	9.215	38.101	15.506	53.745
	(9.558)	(24.374)	(13.330)	(38.710)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	8433	8433	8433	8433
Urban-included sample				
Soy area				
Pct area in soy	0.224*	0.054	0.369**	0.090
	(0.135)	(0.167)	(0.182)	(0.213)
Pct area in natural vegetation	-0.426**	-0.589**	-0.330	-0.446
	(0.205)	(0.268)	(0.209)	(0.271)
Pct area in sugarcane	0.232	-0.358	-0.077	-0.248
	(0.319)	(0.575)	(0.344)	(0.582)
Pct area in mining	3.709	1.431	9.432	44.408
	(8.957)	(13.628)	(10.736)	(31.031)
Population	-0.000	-0.000***	-0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	7751	7751	7751	7751
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Municipalities are included in the urban-included sample if more than 25% of area in the Ottobasin is used for agricultural production. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * p< 0.10, ** p<0.05, *** p<0.01.

had the next largest economy in the sample, with a value of R$137 million Instituto Brasileiro de Geografia e Estatística (2020). Moreover, these states rely far more on manufacturing and service sectors; agriculture counted for less 10% or less of their GDP, while states in the rest of the sample rely on agriculture for a third or more of their GDP TEMA Governo do Estado de Rondônia (2021); Ministério da Agricultural Pecuária e Abastecimento (2021).

The results of the modeling excluding these states are larger and have lower p-values than those in our main results (table C7). We find that a 10 percentage point increase in municipal area in soy led to an additional 0.030 deaths under 5 per 10,000 population and an additional 0.038 deaths under 10 per 10,000 population. Additionally, an increase of 0.10 tons per municipal hectare led to an increase of 0.023 deaths under 5 per 10,000 population and an additional 0.036 deaths under 10 per 10,000 population. These larger results demonstrate the significance of soy expansion for rural communities in Brazil's inland states. Agriculture plays a much larger role in local economies, and our results suggest that it is a relatively more important source of risk for developing ALL. Further, the relative lack of pediatric cancer treatment centers in the central states underscore the difficulties people in these communities face to accessing timely treatment.

Our results are largely robust to the inclusion of state-year fixed effects rather than meso-region-year fixed effects (table C8). The coefficients of interest for children under five are more precisely estimated, while the coefficients for children under ten are less precise. We conclude that our overall conclusions hold with the inclusion of more fine-grained fixed effects.

Finally, we include all deaths from ALL, including those of adults, in our outcome variable. Here, we find no evidence that area in soy is relatedto municipal-level ALL deats and only weak evidence that the previous five years of soy production increased municipal-level ALL deaths. This is likely due to behavioral risk factors that adults undertake (e.g., smoking or occupations with exposure to other carcinogens) and migration that adults undertake throughout their lifetimes.

C.4 The role of Goiás in soy expansion

In our primary results, we exclude Goiás from the sample. This is primarily due to the presence of the Federal District (Portuguese acronym DF) in the state, which significantly alters the economic and health care landscape. The rural agricultural municipalities in Goiás had an average of 6% of land area in soy (compared to 3% in our sample) and produced 0.19 tons per municipal hectare (compared to 0.11 tons per hectare in our sample). While pesticide expenditure per cropped hectare was comparable to neighboring

Table C6: Pediatric deaths from ALL relative to previous year soy production in the municipality

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
L.Pct area in soy	0.047	-0.028	0.048	-0.041
	(0.073)	(0.078)	(0.122)	(0.091)
Soy production				
L.Tons soy per ha	-0.008	-0.011	-0.010	-0.019
	(0.012)	(0.020)	(0.015)	(0.021)
Observations	11497	11497	11497	11497
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * p< 0.10, ** p<0.05, *** p<0.01.

states and higher than much of our sample, Goiás had a higher value of agricultural production relative to pesticide use compared to our sample, suggesting the state has relatively "efficient" use of pesticides de Moraes (2019). Goiás also has the lowest percent of municipalities (11%) in which a water sample tested above the maximum allowable value for pesticide chemicals This suggests that more of the chemicals are taken up by the crop rather than being lost to the air and water supply, which may weaken the relationship between soy production and cancer rates.

Importantly for our study, there are currently five public cancer treatment centers (UNACON) in Goiás and nine in the Federal District; thus, access to healthcare is high compared to other inland states.[1] Additionally, Goiás and the Federal District together saw the largest relative (267%) and absolute (8) increase in hospitals treating pediatric cancer patients during the period.[2] Given that ALL is a largely treatable illness St. Jude Children's Research Hospital (2018), this stark increase in treatment may have weakened the relationship between pesticide exposure and death from ALL to the extent that increases in soy production did not culminate in increased deaths from ALL in Goiás in the later period of the sample.

There may be a difference in state-level institutions that weakened the relationship between pesticides and ALL in Goiás. As discussed, the region has heavy influence from the Federal District, which is the base

[1]Mato Grosso do Sul has the highest number of UNACON of inland states in our sample with eight centers, though this is still lower than a combined 14 within GOiás.

[2]In contrast, neighboring states saw relatively modest changes in treatment availability. Mato Grosso do Sul gained 1 hospital or 33%. Minas Gerais gained 5 hospitals or 19%. Tocantins saw no change in hospitals. Mato Grosso lost 1 hospital or 25%. São Paulo lost 7 hospitals or 15%.

Table C7: Pediatric deaths from ALL relative to annual crop production of previous five year soy production excluding coastal states

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
Pct area in soy	0.295***	0.232**	0.382***	0.357***
	(0.108)	(0.091)	(0.137)	(0.115)
Pct area in natural vegetation	-0.043	-0.012	-0.099	-0.082
	(0.046)	(0.040)	(0.071)	(0.099)
Pct area in sugarcane	0.066	0.053	0.041	0.008
	(0.054)	(0.059)	(0.077)	(0.079)
Pct area in mining	13.346*	50.389	2.269	23.263
	(7.769)	(30.404)	(4.043)	(17.452)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Soy production				
Tons soy per ha	0.042**	0.059**	0.072**	0.096***
	(0.021)	(0.027)	(0.028)	(0.035)
Pct area in natural vegetation	-0.043	-0.011	-0.098	-0.081
	(0.048)	(0.040)	(0.069)	(0.097)
Pct area in sugarcane	0.024	0.028	-0.008	-0.030
	(0.041)	(0.069)	(0.048)	(0.068)
Pct area in mining	13.182*	50.450	2.180	23.388
	(7.807)	(30.458)	(4.037)	(17.518)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	5232	5232	5232	5232
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * p< 0.10, ** p<0.05, *** p<0.01.

Table C8: Pediatric deaths from ALL relative to previous five year soy production in the municipality using state x year fixed effects

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
Pct area in soy	0.173**	0.153**	0.189	0.177*
	(0.079)	(0.076)	(0.116)	(0.096)
Pct area in natural vegetation	-0.073*	-0.018	-0.136**	-0.094
	(0.041)	(0.039)	(0.066)	(0.089)
Pct area in sugarcane	0.011	0.039	0.144***	0.007
	(0.020)	(0.034)	(0.034)	(0.060)
Pct area in mining	13.763	36.772	8.885	22.566*
	(9.782)	(22.706)	(8.594)	(13.464)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Soy production				
Tons soy per ha	0.026**	0.049**	0.037	0.060**
	(0.011)	(0.021)	(0.025)	(0.030)
Pct area in natural vegetation	-0.073*	-0.014	-0.135**	-0.089
	(0.042)	(0.039)	(0.066)	(0.089)
Pct area in sugarcane	0.003	0.039	0.138***	0.008
	(0.018)	(0.033)	(0.036)	(0.060)
Pct area in mining	13.983	37.066	9.158	22.920*
	(9.702)	(22.618)	(8.592)	(13.412)
Population	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	8411	8411	8411	8411
Municipal FE	X	X	X	X
State-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * $p < 0.10$, ** $p<0.05$, *** $p<0.01$.

Table C9: ALL deaths of all ages relative to previous five year soy production in the municipality and the Ottobasin

	Municipality		Ottobasin	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
Pct area in soy	0.019	0.334	0.636	0.348
	(0.285)	(0.309)	(0.530)	(0.504)
Soy production				
Tons soy per ha	0.051	0.112*	0.420	0.395**
	(0.049)	(0.065)	(0.299)	(0.196)
Observations	8433	8433	8433	8433
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * p< 0.10, ** p<0.05, *** p<0.01.

Table C10: Pediatric deaths from slips, trips, and falls relative to previous five year production of soy in the municipality

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
Pct area in soy	-0.070*	-0.064	-0.124**	-0.133
	(0.041)	(0.076)	(0.055)	(0.090)
Soy production				
Tons soy per ha	-0.018	-0.018	-0.026	-0.037
	(0.012)	(0.018)	(0.017)	(0.026)
Observations	8433	8433	8433	8433
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * p< 0.10, ** p<0.05, *** p<0.01.

of the country's federal institutions. Goiás is one of eight states that have implemented requirements for buffer zones for mechanized ground spraying (State Law 19,423 of 2016) [3] While this law was only passed in 2016, and was therefore unlikely to have a meaningful effect during our period, it may signal an overall higher involvement of local and state institutions in mitigating harm from pesticides. describes the steps taken by each state in combating acute pesticide poisoning.[4] Goiás was unique in our sample in focusing their action on and through regional health centers, including trainings for healthcare workers on the health needs of populations exposed to pesticides. This focus may have built capacity in ways that created higher spillover to other aspects of health that are affected by pesticides. The Federal District communicated directly with farmers and laborers through presentations and materials and through agricultural and health students, the benefits of which may have spilled over into the rural areas of Goiás.

Table C11 reports the results of our main specifications including Goiás. We find a weak relationship between soy area and deaths from ALL of children under 10 when we include Goiás, although all coefficients remain positive. Notably, Goiás has 210 rural agricultural municipality, which is nearly one third the size of the remainder of our sample of 767 municipalities.

Table C12 reports the results of our main specification exclusively in Goiás. We generally find a precisely estimated null effect of soy production on pediatric deaths from ALL; coefficients are on average at least an order of magnitude smaller than those in the sample excluding Goiás. There is one exception; we find a statistically significant negative coefficient in the LPM of soy production in tons on deaths under 5. However, we do not interpret this coefficient overmuch, as some variation is expected in the case of multiple models.

[3] Two other states in our sample also had such a law: Mato Grosso ((State Decree 1,651 of 2013) and Tocantins (State Law 224 of 1991).

[4] While this is a distinct issue from the chronic illness we study, it may be that the actions taken to mitigate acute harm from pesticides have positive externalities in terms of mitigating longer-term harm as well.

Table C11: Pediatric deaths from ALL relative to previous five year production of annual crops in the municipality, including Goiás

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
Pct area in soy	0.117	0.090	0.169*	0.179*
	(0.072)	(0.061)	(0.092)	(0.093)
Pct area in natural vegetation	-0.018	-0.027	-0.114	-0.120
	(0.041)	(0.040)	(0.072)	(0.084)
Pct area in sugarcane	0.019	0.035	0.106***	-0.030
	(0.024)	(0.027)	(0.040)	(0.070)
Pct area in mining	12.153	33.123*	8.281	16.514*
	(7.945)	(18.587)	(6.841)	(9.376)
Population	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Soy production				
Tons soy per ha	0.019	0.021	0.032	0.045
	(0.013)	(0.025)	(0.021)	(0.030)
Pct area in natural vegetation	-0.016	-0.025	-0.111	-0.116
	(0.042)	(0.040)	(0.071)	(0.084)
Pct area in sugarcane	0.012	0.032	0.097***	-0.034
	(0.023)	(0.026)	(0.036)	(0.068)
Pct area in mining	12.129	33.242*	8.345	16.820*
	(7.998)	(18.717)	(6.875)	(9.474)
Population	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	10556	10556	10556	10556
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * p< 0.10, ** p<0.05, *** p<0.01.

Table C12: Pediatric deaths from ALL relative to previous five year production of annual crops in the municipality, sample limited to Goiás

	Under 5		Under 10	
	Per 10,000	Binary	Per 10,000	Binary
Soy area				
Pct area in soy	-0.098	-0.011	-0.099	0.043
	(0.088)	(0.066)	(0.098)	(0.088)
Pct area in natural vegetation	0.598	0.220	0.817**	0.503
	(0.363)	(0.258)	(0.289)	(0.339)
Pct area in sugarcane	-0.082	-0.012	0.031	0.035
	(0.074)	(0.065)	(0.074)	(0.055)
Pct area in mining	-4.184	3.216	-1.307	3.890
	(8.344)	(4.837)	(9.263)	(4.055)
Population	-0.000	-0.000	0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Soy production				
Tons soy per ha	-0.009	-0.025***	-0.013	0.004
	(0.014)	(0.007)	(0.018)	(0.020)
Pct area in natural vegetation	0.585	0.228	0.805**	0.509
	(0.357)	(0.249)	(0.282)	(0.332)
Pct area in sugarcane	-0.070	-0.028	0.041	0.029
	(0.062)	(0.050)	(0.063)	(0.046)
Pct area in mining	-4.011	1.729	-1.363	3.807
	(8.300)	(4.401)	(8.815)	(4.526)
Population	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2123	2123	2123	2123
Municipal FE	X	X	X	X
Meso-region-year FE	X	X	X	X
Controls	X	X	X	X

Note. Unit of observation is the municipality. Robust standard errors are in parentheses and are clustered at the Ottobasin level. Municipalities falling in multiple Ottobasins are considered in their primary Ottobasin. * p< 0.10, ** p<0.05, *** p<0.01.

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