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COLLABORATIVE COMPETITION IN HOMELESS SERVICES: THREE ESSAYS ON FEDERAL-LOCAL PARTNERSHIPS

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COLLABORATIVE COMPETITION IN HOMELESS SERVICES: THREE ESSAYS ON FEDERAL-LOCAL PARTNERSHIPS

DISSERTATION

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Graduate School at the University of Kentucky

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ABSTRACT OF DISSERTATION

COLLABORATIVE COMPETITION IN HOMELESS SERVICES: THREE ESSAYS ON FEDERAL-LOCAL PARTNERSHIPS

The United States' federal government funds homeless services provided at the local level through the McKinney-Vento Act, encouraging collaboration among providers. This dissertation studies three aspects of homelessness: merging of local planning bodies, identification of homeless students, and the relationship between experiencing homelessness in high school and long-term educational outcomes.

The first chapter studies the effect of merging Continuums of Care (CoCs), local planning bodies for homeless services. While merging brings organizations into the same network and could make use of economies of scale, it brings service provision to a less-local level, taking away responsiveness to the community and inter-jurisdictional competition. I find merging actually reduces service provision and increases homelessness, using a difference-in-differences design in an event study context.

The second chapter explores the effect of intergovernmental grants on the identification of homeless students. I estimate for each state and year the percentile (threshold) where there is the greatest discontinuity in a district's likelihood of receiving a homeless assistance grant. I find grants do not explain the increase in student homelessness, using the thresholds in a fuzzy regression discontinuity design. The findings show that worsening economic conditions likely explain the increase and policy should address this increase in housing insecurity. I also find the grants do not increase the share of homeless students scoring proficient on state tests.

The third chapter estimates how experiencing homelessness in high school relates to rates of high school graduation and college-going. I find that students have lower graduation rates even after adjusting for observable characteristics. However, the magnitude differs depending on how one considers past experiences of homelessness.

KEYWORDS: Homelessness; Collaborative Governance; Urban Policy; Fiscal Federalism; Difference-in-Differences; Regression Discontinuity

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04/07/2021

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COLLABORATIVE COMPETITION IN HOMELESS SERVICES: THREE ESSAYS ON FEDERAL-LOCAL PARTNERSHIPS

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DEDICATION

To my family for their endless support.

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1. *INTRODUCTION*

The United States' federal government has taken a decentralized approach in many of its programs addressing homelessness, the state of lacking fixed, stable, and adequate nighttime shelter. Doing so conceivably provides funding and resources to communities in need while giving local communities control over how to provide services. A decentralized approach also encourages collaboration among providers by forcing providers to coordinate to maximize grants to the community. Collaboration could improve community-level outcomes by sharing information and eliminating some negative externalities. However, incentives to gain resources incentivize competition. While service providers have a collective goal, each still has an individual goal and aims to secure resources to achieve that goal. Organizations, public administrators, and jurisdictions are thus faced with conflicting incentives to both collaborate and compete with each other, creating a governance structure of collaborative competition. In this dissertation, I study three aspects of homelessness: merging of local planning bodies, identification of homeless students, and the relationship between experiencing homelessness in high school and high school graduation and college enrollment.

The first chapter exemplifies collaborative competition by studying the effect of merging Continuums of Care (CoCs), local planning bodies for homeless services. HUD has encouraged CoCs to merge since 2009 to increase coordination and decrease homelessness, despite lacking evidence that merging leads to this effect. While merging brings organizations into the same network and could make use of economies of scale, it brings service provision to a less-local level, taking away responsiveness to the community and inter-jurisdictional competition. It could also increase inter-organizational competition by suddenly having more members in the network competing for resources. I find merging reduces service provision and increases homelessness, using a difference-in-differences design in an event study context. As persons experiencing homelessness are highly mobile, I see how merging affects nearby CoCs and rule out migration as the explanation, leaving the drop in service provision as the likely mechanism.

The second chapter explores the effect of intergovernmental grants on the identification of students experiencing homelessness. The U.S. Department of Education annually provides states grants to help identify and serve homeless students, which states then sub-grant to school districts. In this context, school districts explicitly compete to receive a grant, based on criteria such as need and capacity. I first rank districts within each state and year for the 2014-18 school years based on their number of students experiencing homelessness and assign each state a percentile of need. I then estimate for each state and year the percentile (threshold) where, within a bandwidth of 5 percentiles, there is the greatest discontinuity in a district's likelihood of receiving a homeless assistance grant. I find grants do not explain the increase in homelessness, using the thresholds in a fuzzy regression discontinuity design to see the effect of receiving a grant on the identification of students. The findings therefore show that worsening economic conditions likely explain the increase and policy should address this increase in housing insecurity. I also find the grants do not increase the share of homeless students scoring proficient on state tests, possibly due to a decrease in local funds.

The third chapter estimates how experiencing homelessness in high school relates to rates of high school graduation and college-going. I find that students have lower graduation rates even after adjusting for observable characteristics, using administrative data from an anonymous school district. However, the magnitude differs depending on how one considers past experiences of homelessness. Additionally, students experiencing homelessness in high school have a small gap in 2-year college going, but large disparities in 4-year college going. These results imply a tradeoff between including students who may have lasting negative effects from a past experience of homelessness and focusing on those for whom likely have the greatest negative impact from homelessness. Further, results suggest difficulty in comparing homelessness-housed educational disparities across districts and states using different definitions. Identifying districts with the greatest educational needs for their housing insecure students requires a unified measurement or adjustments based on definition used.

Overall, the following chapters provide evidence that federal-local partnerships and collaboration may not lead to desired outcomes, particularly when there exist incentives to compete for resources. This challenges a large portion of the collaborative governance literature, often assuming collaboration to improve outcomes and thus focus on how to maintain collaborations. One reason for my alternative findings is the lack of quasi-experimental studies comparing many jurisdictions in the collaborative governance literature. Many focus on short-term outcomes in a descriptive or qualitative framework and study one jurisdiction. My dissertation instead suggests that, particularly from a federal policy point of view, that encouraging for collaboration does not improve outcomes when simultaneously imposing strong financial incentives for collaboration, such as for Continuum of Care grants or McKinney-Vento Education for Homeless Children and Youth grants.

CHAPTER 2. *Working Together: The Impact of Merging Continuums of Care on Homelessness and Homeless Services*

2.1 *Introduction*

Few programs reflect the federal government's prioritization of coordination in public service provision more than homeless services (Hambrick and Rog 2000). Homeless services, starting as largely independent organizations, created formal and informal networks when the federal government began providing funding in the 1980s. Federal funding also led to the creation of about 480 Continuums of Care (CoCs) in 1994: local planning districts where individual homeless service providers within each geographic region must formally coordinate service provision with each other and the federal government (Burt 2002). The number of people experiencing homelessness – sleeping in a place not meant for human habitation or a shelter – on a given night had been decreasing since 2007.[1] However, it has recently increased each year since 2016 to about 550,000 persons in 2019, increasing the need for effective services (Department of Housing and Urban Development 2020). Despite CoCs' creation about 25 years ago to benefit from coordination, analysis of CoCs' ability to solve homeless services' coordination problem and improve outcomes is limited (Mosley and Jarpe 2019; Valero and Jang 2016).

Since at least 2009, HUD has strongly encouraged CoCs to merge to further increase coordination among service providers, with about 101 CoCs merging into 46

[1] Specifically, a homeless person lacks a fixed, regular, and adequate nighttime residence. This includes those living in a shelter. It also includes those imminently losing their primary nighttime residence and those fleeing domestic violence or other dangerous situation. Unlike some other countries, such as the United Kingdom and Australia, homelessness in this context does not include those sharing housing due to economic hardship or a similar condition, i.e., doubled-up.

between 2000 and 2018 (Federal Register 2009). Merging may increase coordination, economies of scale, and internalization of negative externalities by forcing CoCs to have a larger network and share resources. Alternatively, CoCs may also lose specialization and responsiveness to stakeholders by needing to serve a broader population as well as positive effects of interjurisdictional competition, meaning merging may not lead to improved outcomes or service provision. In a guidance issued to CoCs in February 2018, HUD states merging CoCs can lead to improved coordination of services, more efficient resource allocation and planning, and increase competitiveness for resources (Department of Housing and Urban Development 2018). HUD further explicitly states potential benefits and challenges to CoCs' merging, shown in Figure 2.1, similar to other arguments for the consolidation of government jurisdictions such as cost-savings and improvement in quality (Duncombe and Yinger 2007; Gordon and Knight 2008; Taylor et al. 2017). This resource provides possible goals of merging to measure its effect. For example, merging could increase a CoC's competitiveness for annual funding, so federal CoC awards can be a possible outcome. Despite the possible benefits of merging CoCs for homeless services, few scholars have studied whether these benefits exist in practice as opposed to solely in theory and outweigh potential costs, such as lack of responsiveness to the community. Thus, an empirical evaluation of merging's effect on homelessness and operational outcomes can better inform the welfare implications of merging given the conflicting effects on responsiveness, economies of scale, and externalities.

***** Insert Figure 2.1 *****

I ask if merging CoCs decreases homelessness and improves operational outcomes, using panel data from 2007-2017 and comparing variation over time in homelessness and operations in merging CoCs to ones that did not merge or merged at different times. To do so, I estimate the effect of merging CoCs on homelessness and operational outcomes using a generalized difference-in-differences design in an event study context, aggregating CoCs to post-merger boundaries and analyzing changes after merging. Contrary to the policy's goal of decreasing homelessness, I find merging CoCs does not decrease homelessness within the merged geographic area. Results instead suggest a statistically significant increase of about 40% of the pre-treatment mean in chronic homelessness, persons with a disability and persistent homelessness.[2] In terms of operations, although merging caused a long-term increase in participation in homeless management information systems for permanent supportive housing (PSH), a sign of increased coordination, and federal award per service provider, merging also decreased PSH beds, the main form of homeless services.

Because more efficient homeless services could attract homeless persons from nearby CoCs, I also estimate the effect of a CoC's merging on nearby CoCs' outcomes to further explore the mechanism of consolidation's effect and the contradictory finding of an increase in chronic homelessness. Chronic homelessness does not decrease and possibly increases in nearby CoCs, suggesting within-CoC effects are not driven only by migration from nearby CoCs.

I contribute to the literature by conducting empirical analyses to study an inter-governmental public service provision system and special districts created to maximize the benefits of coordination while limiting costs from both coordination and externalities,

[2] Chronically homeless persons have a disability and have experienced homelessness for one or more years or experienced at least four episodes of any of the three categories in the last three years for a year combined.

which also incentivizes both coordination and competition (Hambrick and Rog 2000; Feiock 2007; Bel and Warner 2015; Hawkins et al. 2016). I also contribute to the literature on how regional government formats affect outcomes by showing consolidation does affect outcomes and operations (Lee et al. 2012; Chen et al. 2016; Hawkins et al. 2016; Taylor et al. 2017). Most previous related studies have analyzed school districts or local governments, which have different incentives for coordination and competition as well as revenue sources (Duncombe and Yinger 2007; Gordon and Knight 2008; Hawkins et al. 1991; Roesel 2017; Taylor et al. 2017). I lastly add to the literature on the economics of homelessness by showing that merging CoCs likely does not lead to improved outcomes by internalizing externalities.

2.2 *The Continuum of Care (CoC) Program*

2.2.1 *Structure and Responsibilities of Continuums of Care*

A CoC, by its namesake, prioritizes coordination. Originally a term from healthcare services, a medical continuum of care links all treatments and services to create a holistic plan to address the problem, share information among stakeholders, and reduce gaps in service provision. Similarly, HUD requires CoCs to address multiple aspects of homelessness and create a system of services. HUD allowed communities to organize CoCs themselves to maximize the benefits of coordination. CoCs could take any format, so long as every individual service provider (ISP) receiving federal funds was within a CoC and the geographic boundaries of a given CoC were the size of a Community Development

Block Grant district at the smallest, e.g. downtown Chicago, and state at the largest, e.g. Wyoming.[3]

Homeless services generally take one of four forms through ISPs that serve homeless persons (Department of Housing and Urban Development 2017). Emergency shelters offer only temporary shelter. Transitional shelters provide shelter and some services like continuing education for a period of up to two years to prepare homeless persons for independent housing. Permanent housing provides long-term housing with many services, often providing a “Housing First” approach offering housing without prior conditions such as sobriety. HUD considers homeless persons in permanent supportive housing no longer homeless. Lastly, services may not provide shelter, but instead, support or resources to help homeless or homeless-at-risk persons like rent support.[4] Other types of services exist, such as rapid rehousing, but generally fall into one of these four categories. ISPs also have varying characteristics and their own goals, such as focusing on a specific homeless subpopulation – e.g., single men, women and children, or persons with a mental illness (O’Flaherty 1996).

CoCs differ in coordination formats for their ISPs and leadership styles, following similar patterns to network governance structures in general (Provan and Kenis 2008). First, CoCs have varying levels of formal coordination. While ISPs within some CoCs have many formal interactions, such as monthly meetings, ISPs in others only interact to apply for annual funding from HUD (Burt 2002). Second, a CoC’s lead organization is typically a nonprofit, government office, or public-private partnership. The ability to have different

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[3] See Appendix Figure B1 for maps of CoC boundaries.
[4] See Appendix C1 for definitions of each service type.

leadership structures allows CoCs to make formal coordination networks fit their needs and priorities (Valero and Jang 2016). The freedom of organization would ideally allow each CoC to lower costs and identify externalities imposed or resource misallocation, while maximizing responsiveness to its stakeholders, lacking when HUD initially began funding homeless services.

2.2.2 *Coordinating ISPs*

HUD created CoCs in 1994 in response to the high coordination costs in homeless services. HUD originally funded thousands of ISPs using about $350 million through the McKinney-Vento Homeless Assistance Act of 1987 (M-V). Practitioners soon realized externalities and coordination costs from the disorganization in homeless services hindered them from effectively serving homeless persons (Burt 2002). First, organizations impose externalities on each other from not considering other actors. For example, if an organization decreases its service quality, it may lead to users going to other organizations, increasing their costs (O'Flaherty 2003). Further, aggregate service provision may be below optimal if each ISP acts independently by benefiting from the publicness of services (Samuelson 1954). Second, ISPs competing for federal funds expend resources on applying for the funds instead of providing services. HUD only necessitates showing need instead of effectiveness or coordination, meaning ISPs are not directly incentivized to be more effective. Third, the diversity of services complicated evaluating services and determination of funding for HUD because of unstandardized outcomes and a large number of applicants, typically thousands each year (Burt 2002). Funding, for example, would potentially compare a large shelter for single men in New York City to a small transitional housing organization for young mothers in the Midwest, despite different goals, services, and contexts.

These costs and externalities could be lowered by ISPs' coordinating using joint decision making or compensating each other for externalities imposed, so long as transaction or coordination costs are low (Coase 1960). Scholars suggest coordination can internalize externalities and increase goal congruence between actors (Hawkins et al. 1991; Norris 2001; Bel and Warner 2015; Chen et al. 2016; Shresthaa et al. 2014). Feiock (2007) finds coordination to be a dynamic process, with both informal and formal actions related to costs. Four costs exist: information, negotiation, enforcement, and agency. Information costs refer to the costs of gathering information on preferences of stakeholders, and possible benefits and resources available. Negotiation costs refer to work dividing the benefits gained. Enforcement costs refer to costs related to maintaining agreements. Lastly, agency costs refer to being responsive to constituents' and clients' needs and preferences. Unlike externalities, however, coordination costs are usually accounting costs because they result from direct actions, such as negotiating with another service provider, and can be directly monetized, either as labor or capital costs. Coordination in homeless services may be negotiating with other ISPs what services a given ISP will offer or how to divide federal grants to improve overall outcomes.

As coordination costs exist, limiting externalities would need to be balanced with coordination costs. Coordinating ISPs through CoCs could lower coordination costs while still receiving its benefits by being local planning bodies for homeless services. CoCs would decrease the number of applications HUD reviewed annually for funding to a few hundred, while forcing ISPs to create community homeless assistance plans, ideally leading to lower homelessness with more efficiency (Department of Housing and Urban Development 2009). Thus, HUD and CoCs have a symbiotic relationship, where HUD needs CoCs to coordinate ISPs and serve homeless persons while CoCs rely on HUD for a stream of resources. CoCs, however, also made inter-jurisdictional competition as well as creating incentives both for competition and coordination within each.

2.2.3 *Coordinating CoCs*

Despite its intention of increasing coordination in homeless services, HUD also induced interjurisdictional externalities by having CoCs compete for federal funds. For example, when evaluating CoCs' funding applications, HUD rates CoCs based on their improvement in outcomes for homeless persons, such as decreasing levels of homelessness, and coordination efforts of stakeholders. Lee (2019) finds CoCs with larger decreases in homelessness compared to other CoCs receive more funding from HUD. In evaluating CoCs' performances, however, HUD does not consider how interjurisdictional externalities produced by homeless services may affect other CoCs' performances (Department of Housing and Urban Development 2009). As the level of funds HUD can distribute is fixed in the short-term, CoCs have little incentive to internalize the performance of others. Externalities produced by services, positive or negative, like the migration of homeless persons from one CoC to a neighboring one, are not internalized.

Figure 2.2 shows the format CoCs created: a hierarchy of funding and goals where externality and coordination costs occur, requiring a balance between the benefits and costs of consolidation. First, HUD at the top funds CoCs that then fund ISPs. The hierarchy can also be viewed as a map of each actor's goal. HUD desires lower homelessness across the country. However, as CoCs cover distinct geographic areas, they desire lower homelessness only within their area. ISPs desire lower homelessness from the narrowest geographic or service area. Lastly, HUD, CoCs, and ISPs all benefit from more efficient service provision, freeing resources for other purposes.

***** Insert Figure 2.2 *****

Coordination is represented by the solid lines. ISPs must coordinate with their CoC, which must then coordinate with HUD. Coordination costs decrease for HUD compared to the period before CoCs as HUD decentralized costs to CoCs. As coordination costs include costs from gathering information on and monitoring performances of CoCs (Feiock 2007), the costs have a direct relationship with the number of agents being funded. Cutting the number of agents from several thousand to a few hundred should drastically cut coordination costs for HUD. CoCs incur coordination costs as they must gather information on ISPs and monitor outcomes, but only coordinate with an average of about nineteen ISPs, meaning costs should not be as high as in the pre-CoC format (Department of Housing and Urban Development 2017).

Externalities occur through competition as ISPs compete within their CoC for funding (inter-organizational competition) while CoCs compete for HUD funding (inter-jurisdictional competition), represented by dashed arrows. At one extreme, if only one CoC exists, it internalizes all externalities, so it recognizes any increase in homelessness. If two CoCs exist, each does not internalize externalities, meaning if one's actions, service levels, or policies affect homelessness in the other CoC it does not consider the externality of increased homelessness. Externality costs continue increasing with additional CoCs (Hawkins et al. 1991). The externality from an increase in homelessness, public costs of serving homeless persons such as healthcare and shelter, could increase these interjurisdictional costs with estimates of the annual cost of homelessness generally ranging between $5,100 and $38,000 (Flaming et al. 2015; Evans et al. 2016; Hunter et al. 2017). ISPs and CoCs must then balance the tension between accomplishing shared network goals through coordination while achieving individual goals, which consolidation could influence by affecting coordination and externalities (Piatak et al., 2018).

First, merging may increase coordination by having additional organizations within one CoC network as opposed to spread over several jurisdictions. Sharing the same CoC resources as more ISPs and working with more organizations could lead to better coordination as opposed to being split over multiple CoCs. I measure coordination through the participation rate in the CoC's Homeless Management Information System (HMIS) for organizations providing permanent supportive housing (PSH). Although not a direct measure of coordination, HMIS is a unified data system for all service providers in the CoC that collects client-level data and can lead to information sharing. A larger percentage of organizations participating in the system would suggest coordination increased in the CoC after merging.

Merging may also increase economies of scale by reducing duplicative services, leading to more efficient services and being more competitive for CoC grants. I first measure efficiency and competitiveness through the amount of federal CoC grants in dollars, or award, the CoC receives from HUD for homeless services as HUD states in its guidance merging may lead to more efficient services and therefore funding. I then see the effect of merging on the number of homeless service providers in the CoC receiving HUD CoC grants as removing duplicative services and increasing inter-organizational competition may decrease the number of organizations. Consolidation may increase economies of scale by allowing services to better use shared infrastructure or administrative resources (Duncombe and Yinger 2007; Gordon and Knight 2008). I thus combine the two measures into federal award per grant recipient to capture a form of economies of scale. More efficient services may also lead to more service provision by increased inter-organizational competition requiring more services. I measure service provision through the total number of beds and PSH beds. I separate PSH beds as its use and funding have increased dramatically over the period and become the preferred form of service.

On the other hand, merging may decrease the benefits of decentralized service provision. More CoCs covering smaller geographic areas may have benefits as programs can be more responsive to their stakeholders and tailored for each CoC's needs (Gordon and Knight 2006). For example, some CoCs, particularly in warmer climates, have a higher proportion of unsheltered homeless persons (Corinth and Lucas 2018). As unsheltered and sheltered homeless persons have different needs, namely shelter, ISPs in a more responsive CoC can offer more services for unsheltered persons, ideally being more effective and lowering homelessness. Merging CoCs may decrease responsiveness by forcing service providers to cover a larger area and broader population. As such, I first estimate the effect of merging on the diversity of service provision through a Herfindahl-Hirschman Index (HHI). To create an HHI, I use six service types: the number of beds for emergency shelters, transitional housing, and PSH, further split by whether beds are for households with or without children. Similarly, people experiencing homelessness come from various backgrounds and have different experiences while homeless. As merging could make CoCs less responsive to the needs of people they serve, it may not decrease homelessness in less common subpopulations. In addition to testing merging's effect on overall homelessness, I estimate the effect of merging on four separate subpopulations: unsheltered, sheltered, chronic, and non-chronic.

Unsheltered homeless persons are those sleeping on streets or places not meant for human habitation, whereas sheltered homeless persons stay in a place meant for human habitation, such as a shelter or hotel. Chronically homeless persons are those who experience several bouts or extended periods of homelessness, which is generally considered one of the most severe cases of homelessness. Every homeless person is either sheltered or unsheltered and chronic or non-chronic.

Differential policy effects between chronic and non-chronic homelessness are notable in previous literature as chronically homeless persons often have disabilities and are cost-drivers of services despite being only being about 18% of the homeless population (Corinth 2017). Given their additional vulnerability and high resource-use, policymakers may weigh effects on chronically homeless persons more than non-chronic persons, as shown by ending chronic homelessness's being a priority of HUD (Department of Housing and Urban Development 2017). At the community level, if consolidating CoCs decreases chronic homelessness, it would be a signal that CoCs became more efficient and have lower public costs.

By creating CoCs, HUD introduced coordination to help limit negative inter-*organizational* externalities, while also introducing inter-*jurisdictional* externalities. HUD's encouragement of merging CoCs to increase coordination is meant to decrease these new negative externalities through internalization and ultimately decrease homelessness. Given the conflicting effects on responsiveness, economies of scale, and externalities, I, therefore, ask if consolidation achieves the goal of improving coordination and operations and decreasing homelessness.

2.3 *Empirical Strategy*

2.3.1 *Generalized Difference-in-Differences in an Event Study Context*

To empirically study the effect of consolidating CoCs on homelessness, I use a generalized difference-in-differences (DiD) in an event study context design, estimating both average and time-varying treatment effects. The unit of observation is the CoC level, based on 2016 boundaries. Aggregating historical CoCs to their 2016 boundaries allows analysis of changes in outcomes within those geographic areas, where CoCs never merging have the same boundaries in 2016 as before.[5] As control variables are at the county level, but some CoCs are smaller than a county, I population-weight controls for these CoCs and aggregate to cover the county (Appendix Table C2).[6]

As data are for 2007-2017, I limit possible treatment years to 2010-2013 to ensure adequate pre- and post-treatment years and a balanced panel with respect to years from merging, ensuring each treated CoC has an equal share of periods spent untreated and treated in the estimation sample and inference is only drawn from equal times from merger (Goodman-Bacon 2019).[7] For example, even though I have a full, balanced sample from 2007-2017, a CoC merging in 2010 has an analysis sample of 2007-2014, whereas a CoC

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[5] By *historical CoCs*, I refer to the geographic boundaries of CoCs before their merging.

[6] For example, there is a CoC for Cook County, IL, as well as one for "downtown" Chicago. I population weight control variables, such as the poverty rate, and aggregate to create one CoC covering all of Cook County. Variables that are counts, such as the number of homeless persons, are only aggregated and not weighted.

[7] I also estimate a sensitivity check using a balanced panel by time as opposed to by years from treatment, finding similar results (Figure B11).

merging in 2013 has an analysis sample of 2010-2017. I identify mergers based on reporting in HUD's point-in-time count resource which states which CoCs merged and when (Appendix Table C3). CoCs merging in other years are dropped from the sample. Lastly, although some CoCs merged multiple times over the period, I consider treatment the first merger for main models, while re-estimating models dropping CoCs merging multiple times as a sensitivity check. About five mergers occurred per year on average during treatment years (Appendix Figure B2). This creates a panel of 43 CoCs merging into 18 for analysis.

First consider a more traditional, two-period DiD in Equation (1) where *PostMerger* is an indicator taking the value of one in years, *t*, after a CoC, *c*, has merged the first time and zero otherwise.

(1) $$Homeless\ Outcome_{c,t} = \beta_1(PostMerger)_{c,t} + \theta X_{c,t} + \alpha_c + \gamma_t + \varepsilon_{c,t}$$

β_1 is the effect of a CoC's first time merging on homeless outcomes compared to the period before merging and outcomes of the control group, CoCs never merging or merging at other times. I include CoC fixed effects, α_c, to control for all time-invariant characteristics of the CoC, including whether it is a treatment or control CoC. Year fixed effects are γ_t, controlling for any unobserved characteristic in a given year which applies to all CoCs, such as the national economy. Standard errors, $\varepsilon_{c,t}$, are clustered at the CoC level in all models as error terms likely correlate within a CoC over time.

$X_{c,t}$ is a vector of time-varying covariates at the CoC level controlling for economic and demographic factors, with each relationship with the outcome in vector θ interpreted as within-CoC relationships.[8] I choose control variables based on previous research of what may affect homelessness. As economic factors correlate with homelessness (Byrne et al. 2013), I control for per capita income, unemployment rate, labor force per capita, poverty rate, fair market rent for zero-bedroom housing, and new low-income housing tax credit units. I also control for demographic factors, including population density and share of the population Black, Asian, and Hispanic as homelessness disproportionately impacts these groups (Department of Housing and Urban Develop 2017). Last, I control for if the state's governor is a Democrat and the state's Temporary Assistance for Needy Families (TANF) benefits for two-person families.

To estimate time-varying effects of merging CoCs on outcomes, I estimate Equation (2) where τ is year of treatment, making *j* years from treatment, which occurs at *j*=0.

$$(2) \qquad Homeless\ Outcome_{c,t} = \sum_{j=-3}^{j=-2} \beta_j (Merged_c * \mathbf{1}[t-\tau=j]) + \sum_{j=0}^{j=4} \beta_j (Merged_c * \mathbf{1}[t-\tau=j]) + \theta X_{c,t} + \alpha_c + \gamma_t + \varepsilon_{c,t}$$

This method compares variation in merged CoCs to variation in CoCs that did not merge or merged at other times, relative to a base year, conditional on CoC and year fixed effects and time-varying control variables (Sun and Abraham 2020). The identifying assumption is the CoCs would have followed a similar trend but for the merging. I create a set of dummy variables for each year from treatment to include in the model except the base year of *j*= -1. The interpretation of a given coefficient is the average treatment effect of merging for each period relative to the difference between treatment and control CoCs in the base

[8] I estimate sensitivity checks with no control variables as treatment could have been correlated with controls, masking the true treatment effect. Additionally, if treatment were random, controls would not be needed econometrically. The results are similar to the main results (Figure B10).

year. As the calendar year for a given *j* differs depending on the event cohort, the control group at *j*= -1 becomes a weighted average of the control group's outcomes, after conditioning on controls, where the weight is the number of CoCs merging in that year (Sun and Abraham 2020). For example, five of the eighteen mergers occurred in 2010, so control group outcomes in 2009, the calendar year when *j*= -1 for this cohort, has a weight of 27.8%. As the panel is balanced by years from treatment and there are exclusive dummy variables for $j \neq -1$, the conditional average difference between treatment and control CoCs in *j*= -1 becomes standardized to zero as the reference group. When $j \neq -1$, β_j is then the differences between treatment and control CoCs for a given year from treatment, implicitly weighted by the number of treatments in a given calendar year as before, relative to the reference difference, and thus a DiD.[9]

Equation (2) also allows tests for a placebo effect when $j<0$ as there should be no effect before treatment (Granger 1969). As a preview, results cannot find statistically significant evidence against the parallel trends assumption, as coefficients before treatment are statistically insignificant and generally close to zero. I also conduct a Fisher unit root test for all outcomes and reject the null hypothesis that a unit root exists in all panels.

***** Insert Table 2.1 *****

2.3.2 *Data Sources*

All outcomes are listed in Table 2.1 under "Operational Variables" and "Homelessness Variables." Data on CoC mergers and the number of homeless service beds come from HUD's 2007-2017 Housing Inventory Count reports. I additionally gather

[9] I additionally follow Sun and Abraham's (2020) method of an interaction-weighted estimator for estimating a DiD, which estimates the event study by treatment cohort and weights coefficients by each cohort's share of total treatments, finding results almost exactly the same as main results shown.

information on HUD CoC grants from HUD Exchange's awards and allocation page. All outcome variables except HMIS participation rate and HHI are in rates of per 10,000 CoC residents.

For homelessness outcomes, I use the number of persons counted as homeless on a given night per capita. Since 2005, CoCs collect data on the number of homeless persons annually one night every January following a national definition and procedure. Although counts existed before 2005, most were done by local governments or organizations, and thus not comparable across localities or over time. HUD point-in-time (PIT) counts, on the other hand, follow a definition and methodology, providing comparable estimates of homelessness across local communities and time (Department of Housing and Urban Development 2014). Having them occur at the same time also helps prevent duplicate counts of persons experiencing homelessness. However, I drop data for 2005 and 2006 as they were the first years of the counts and lack accuracy. While other measures, such as time from entering shelter to exiting or percentage of formerly homeless persons experiencing homelessness again would also provide information on the effectiveness of merging, these measures have only been available at the CoC level since 2015. The average number of homeless persons per 10,000 CoC residents was about 20, and chronically homeless about 4, although large variation also exists (Table 2.1).

Data on per capita income come from the Bureau of Economic Analysis. The unemployment rate and the labor force are from the Bureau of Labor Statistics. Data on new low-income housing tax credit developments and fair market rents come from HUD's low-income housing tax credit database. State-level variables are from the University of

Kentucky Center for Poverty Research. All other control variables come from the American Community Survey.

2.4 *Results*

2.4.1 *Descriptive Analysis*

I first create descriptive trends (Appendix Figure B3) to show average outcomes between treated and control CoCs after merging by years from merging (see Appendix A for a description of the weighting mechanism). Although not causal, these figures provide information about how the levels and trends of outcomes differed between merging and never-merging CoCs. Inter-organizational coordination as measured by the HMIS participation rate is of particular interest, while starting about eighteen percentage points lower than never-merging CoCs, has drastically increased by about twenty percentage points, or 35% (Panel A). Additionally, although homelessness decreases in merging CoCs after merging, never-merging CoCs also have a decrease, including chronic homelessness, suggesting merging may not be the cause (Panels H-L).

2.4.2 *Homelessness Outcomes*

***** Insert Table 2.2 and Figure 2.3 *****

I present the average effects of the generalized DiD for homelessness outcomes in Table 2.2. Average effects are the average difference in the outcome after merging compared to CoCs never merging or merging at different times. Along with point estimates, I present changes as the percentage change in pre-treatment means. Consolidation likely did not decrease homelessness. On average, the rate of homelessness increased by about 0.8 persons per 10,000 population, a 6% increase from the rate of homelessness in merging CoCs before they merged. The lower bound of the confidence interval of reduction in overall homelessness is 13%, meaning merging at best led to a small decrease in homelessness. Contrary to the stated goal, merging increased chronic homelessness, significant statistically, by about 0.7 persons per 10,000 capita or 40%. However, while there is evidence for an increase, there is large variation and standard errors, meaning the magnitude of the increase is less clear.

Figure 2.3, corresponding to Equation (2), plots coefficients as solid bullet points by years from treatment, where the base year is j= -1, with shaded areas showing 95% confidence intervals, and provides further support by showing effects of merging over time. These add to the understanding of the effect of merging as the effect may increase over time or fade out, also allowing to see pre-trends. Chronic homelessness shows an immediate increase that stays constant in the long-term. While individual coefficients are not significant, they are jointly significant, providing evidence merging increased chronic homelessness. Additionally, differences before treatment (left of the vertical blue line) are insignificant and close to zero, suggesting chronic homelessness in merging and control CoCs followed similar trends before merging.

Other populations show no change, not changing at any point as its coefficients are almost always near zero.[10] Although overall homelessness decreased after merging as shown in the descriptive analysis, merging was likely not the cause.

2.4.3 *Operational Outcomes*

***** Insert Table 2.3 and Figure 2.4 *****

[10] As I test several hypotheses, it could be the significant results are from repeated use of the same data. To correct for this, I use a Bonferroni correction in each domain of homelessness and operations. This involves dividing the critical p-values by the number of hypotheses, five and seven, respectively, or new statistically significant p-values of 0.01 and 0.0071. Chronic homelessness is no longer significant when using the correction due to the large variation in effects. Evidence may therefore be limited.

I present the average effects of merging on operational outcomes in Table 2.3, with treatment effects by year from treatment in Figure 2.4. First, merging increased participation in HMIS for PSH servicers by an average of 13 percentage points, suggesting merging did increase coordination among ISPs. This effect increased over time, rising to about 30 percentage points five years after treatment (Figure 2.4, panel A). The increasing coordination through sharing information may therefore take time and not be a sudden change. Control and merging CoCs also had similar trends before merging, suggesting coordination was not already increasing at the time of the mergers.

Merging also decreased the number of service providers on average and increased the federal CoC grant award per provider relative to control CoCs by 27% and 14%, respectively, providing evidence that merging increased economies of scale by having resources concentrated in fewer organizations. However, time-varying effects suggest the number of organizations was already decreasing, breaking the parallel trends assumption. On the other hand, award per service provider does not break the assumption, although it seems to have increased within two years after which then stayed constant (Figure 2.4, panel F).

PSH also decreased after merging, although the large standard errors suggest the magnitude of the decrease ranges from small to large. Time-varying effects show the large variation in average effects likely results from later years after merging. Figure 2.4, panel C shows an immediate decrease in PSH beds with a more precise estimate between 4% and 36%. PSH continues to decrease to an average of 30% two years after treatment. The effect of consolidation becomes insignificant after three years due to variation in effects across CoCs, although coefficients are similar. Pre-trends also seem to support the parallel trends assumption and identification. Merging then likely decreases PSH beds in the short-term with a more ambiguous long-term effect.

As the HHI did not significantly change, CoCs may have increased economies of scale by removing duplicate services after merging. On the other hand, service provision as measured by beds did not increase and instead decreased in PSH beds, meaning increased funding per service provider did not translate into increased service provision as measured by the number of beds.[11]

2.4.4 *Interjurisdictional Effects*

To further explore the mechanism of the increase in chronic homelessness and possible migration, I extend the DiD to interjurisdictional effects through changes in outcomes in the rest of the state's and neighboring CoCs. As operations and homelessness in a given CoC may have spillover effects, analyzing the possibility can lead to a fuller understanding of merging's causal impacts. For example, a neighboring CoC may increase its services as a response to a merging CoC's decrease in services. Alternatively, if merging decreased homelessness in another CoC, aggregate outcomes would improve. Lastly, if nearby CoCs saw an equal decrease in chronic homelessness then migration could explain the increase within merging CoCs.

I first compare CoCs in the rest of the state as all CoCs within a state share the same state government and have overlapping stakeholders. To create the outcomes, I total the outcomes for the state's CoCs and subtract the CoC's levels. Second, I compare neighboring CoCs as a unit as neighboring CoCs likely have similar markets and clients

[11] All effects on operational outcomes are still significant after using a Bonferroni correction with a new p-value of 0.0071.

and increases the likelihood of migration by homeless persons. To construct these outcomes, I use Census records of neighboring counties and then aggregate to the CoC level. I then total the outcomes for all neighboring CoCs for each CoC.

First, almost no interjurisdictional effects seem to exist for operational outcomes for the rest of state or neighboring CoCs (Appendix Tables B1/B2 and Figures B4/B5). Second, nearby CoCs cannot fully explain the increase in chronic homelessness in merging CoCs, which is evidence against full migration (Appendix Tables B3/B4). Merging had no or little effect on chronic homeless in CoCs in the rest of the state. Estimates for neighboring CoCs suggest an increase in chronic homelessness or a very small decrease at most. Additionally, whereas chronic homelessness increased immediately in merging CoCs, it is a steady increase in neighboring CoC (Figure B7, Panel D). Third, while statistically insignificant, neighboring CoCs see a decrease in total and non-chronic homelessness of at most less than 1%, with up to a possible increase of 17%. Last, effects on neighboring CoCs' homelessness tend to be higher than the rest of state homelessness, suggesting neighboring CoCs to be more strongly connected than CoCs in the rest of the state. Overall, merging seems to have not improved overall welfare in terms of the number of homeless persons and chronically homeless persons' possible migration to merging CoCs does not explain their increase.

2.4.5 *Sensitivity Checks*

I also estimate five sensitivity checks: CoCs only merging once, dropping CoCs which may have not conducted a homelessness count, dropping control variables from regressions, and using a panel balanced with respect to calendar years instead of time from treatment. First, as some CoCs merge multiple times, but I only consider the first merger as treatment, I re-estimate event study models dropping CoCs that merged again in future years to keep the intensity of treatment the same across merging CoCs. In theory, merging could change the likelihood of future mergers. Additionally, if merging has short-term costs, such as decreased beds, which revert to normal in the long-run, multiple mergers could mask these differences. Appendix Figure B8 shows the estimates. Results are similar to main models. Changes in chronic homelessness are no longer statistically significant, but are also not statistically different from main models.

Second, some CoCs only conduct counts of homeless persons every other year for subpopulations. Although most CoCs conduct counts every year currently, this was not always the case, meaning measurement error through using a previous year's count could bias results. I re-estimate event study models for outcomes dropping observations where the change in the subpopulation was zero, as these are likely CoCs that did not count that subpopulation that year. Results are again very similar to main models (Figure B9).

Third, I re-estimate models dropping time-varying control variables. I do this as treatment could have been correlated with controls, masking the true treatment effect. Additionally, if treatment were random, controls would not be needed econometrically. No results change in statistical or practical significance. Figure B10 shows that results are robust and still do not violate the parallel trends assumption. Interestingly, the estimate for chronic homelessness is much more statistically significant and some by-year coefficients are even significant, although the magnitudes are almost the same as main models.

Last, I re-estimate time-varying models using an unbalanced panel, meaning additional pre- and post-treatment years are included but coefficients are only representative of late or early mergers. While providing more years of information, other factors correlated with merging and outcomes, particularly further away from treatment, may influence results. However, most results are still similar to main regressions, although results do suggest effects to persist over time (Appendix Figure B11). For example, the number of PSH beds per 10,000 population decreases by about two immediately after merging and this decrease continues for seven years on average (Panel C). This provides evidence against the decrease being only an implementation issue and instead a long-lasting change in services. Additionally, it seems in Panel J that chronic homelessness continues to increase over time. Taken together, main estimates may understate the true effect of merging on the decrease in PSH beds and chronic homelessness.

2.5 *Discussion*

Based on these analyses, I find that merging CoCs does not achieve the policy goal of reducing homelessness and instead decreases PSH beds and likely increases chronic homelessness in the long-term. While homelessness decreased in CoCs after merging as shown in descriptive analyses, merging does not seem to have been the cause. Analyzing how merging affected nearby CoCs also alleviates concerns of full migration as nearby CoCs did not see a decrease in homelessness. Last, merging does seem to have increased coordination, shown by the increased participation in HMIS and an increase in federal award per service provider.

In terms of policy implications, merging, at best, leads to a small reduction in homelessness and leaves behind people experiencing severe cases of homelessness, i.e., chronic homelessness. Migration from neighboring CoCs could only explain 29% of the increase on chronic homelessness, found by dividing its lower bound by the average effect of merging in chronic homelessness. The mechanism is then likely a slower rate of placing chronically homeless persons into independent housing, more persons experiencing long or chronic cases of homelessness, or fewer services. Following estimates of the impact of PSH beds on chronic homelessness in Corinth (2017), one additional bed decreases chronic homelessness by 0.24 persons. Applied to my results by multiplying the average change in PSH beds by 0.24 and dividing that by the average change in per-capita chronic homeless, merging's decreasing PSH beds would explain about 56% of the increase in chronic homelessness. As there is no evidence non-chronic homelessness decreased, the remaining mechanism is then likely a slower rate of placement from PSH into independent housing (Corinth 2017).

In terms of welfare, Evans et al. (2016) estimate, including private costs to the homeless person, homelessness to cost society $20,548. Hunter et al. (2017), in evaluating costs of PSH, found the average homeless person, 83% of whom were chronic and higher-users of health services in the study, to have public service costs of $38,146, largely from healthcare costs, without housing. As chronic homelessness increased an average of 0.719 per 10,000 population in the CoC after merging, this would be an increase in chronic homelessness by 619 persons as pre-treatment average population was 8.615 million. This translates into an increase in costs of about $12.7 and $23.6 million based on the two estimates. As the average federal CoC award pre-treatment was $8.8 million, this would be a large increase in costs relative to annual federal funding. Factoring in PSH costs from Hunter et al. (2017) of $15,288 per person and the average decrease post-merging, would suggest savings from fewer PSH beds of $25.6 million. If no interjurisdictional effect exists and there are only these two costs, there is likely some welfare improvement. However, consolidation's effect on total homelessness in neighboring CoCs would only need to be an increase of over 0.22 persons per 10,000 population, given the pre-treatment average population of 29 million and using the Evans et al. (2016) cost estimate, for consolidation to harm welfare.

Implementation issues may also limit the effectiveness of merging as a decrease in permanent supportive housing beds exists. A short-term decrease in service provision could have long-term impacts on homelessness within the CoC, which future studies can further analyze. CoCs may consider planning for costs to prevent lapses in service provision that may occur during the implementation phase.

Lastly, as merging does increase some operational measures related to coordination, such as increasing the HMIS participation rate and award per service provider, while not decreasing specialization of service type, policy related to merging could focus on turning increased coordination into improving outcomes. While increasing coordination is an intermediate goal, its ultimate goal is to decrease homelessness and do so more efficiently.

The findings also suggest the public administration literature must consider how merging jurisdictions, regardless of coordination, affects outcomes, particularly over time. As estimates control for how coordination-prone CoCs are through fixed effects and outcomes still change after merging, studies analyzing interjurisdictional coordination must consider how incentives affect outcomes. Additionally, studies must consider how changes in jurisdictions dynamically affect outcomes through short- versus long-run effects and subpopulations as results differ. CoCs, a unique government district prioritizing coordination, provide an apt example of how merging in an area affects outcomes given their inherent nature of coordination and externality costs imposed.

Some limitations remain, particularly related to mechanisms connecting merging to outcomes. More detailed data on individual service providers can reveal variation in the specialization of services, especially between CoCs that merged and did not. Expanding the analysis of interjurisdictional effects to other forms, such as metropolitan statistical areas, can also show how the number of CoCs in a geographic area changes outcomes. Lastly, more detailed data on the migration of homeless persons across CoCs can better explain behaviors of persons experiencing homelessness and how different subpopulations react to changes in services dependent on the number of CoCs.

2.6 *Tables and Figures*

Table 2.1 Summary Statistics

	Obs.	Mean	S.D.	Min	Max
Operational Variables					
Total Homeless Service Beds per Capita*	3,872	20.83	18.91	0	218.5
Permanent Support Housing Beds per Capita*	3,872	8.259	10.32	0	101.6
Grant Recipients per Capita*	3,836	0.305	0.248	0.0044	2.006
Federal Award per Capita*	3,836	49,631	53,336	143.4	469,004
Award per Service Provider	4,175	175.1	127.5	2.490	1,173
Herfindahl-Hirschman Index	4,544	0.245	0.0670	0.131	1
HMIS Rate for PSH	4,514	0.757	0.280	0	1
Homelessness Variables					
Total Homeless Persons per Capita*	3,874	19.56	20.85	0.133	202.0
Unsheltered Homeless Persons per Capita*	3,874	7.724	16.10	0	171.0
Sheltered Homeless Persons per Capita*	3,874	11.83	10.39	0	117.9
Chronically Homeless Persons per Capita*	3,874	3.682	6.233	0	106.0
Non-Chronically Homeless Persons per Capita	3,874	15.88	16.10	0.088	153.8
Control Variables					
Unemployment Rate	3,876	6.799	2.787	2.8	28.90
Poverty Rate	3,876	14.11	4.713	2.2	39.20
New Low-Income Housing Tax Credit Units*	3,876	2.396	3.948	0	61.53
Per Capita Income	3,876	43,534	12,109	12,056	124,552
Fair Market Rent, 0 Bedrooms	3,876	646.3	206.9	247.5	1,915
Population Density	3,876	0.000415	0.00104	0	0.0146
Share Black	3,876	12.85	12.29	0.2	66.10
Share Hispanic	3,876	11.69	12.72	0.6	83.40
Share Asian	3,876	4.498	5.345	0	46.32
TANF 2-Person Benefit	4,219	714.9	151.9	426	1,259
Governor is a Democrat	4,207	0.486	0.500	0	1
Labor Force per Capita*	3,876	4,969	542.2	3,708	8,087

Notes: Data are at the CoC level and from 2007-2017. * indicates the rate per 10,000 population. All operational homelessness variables are used as outcomes.

	Total Homeless	Unsheltered	Sheltered	Chronic	Non-Chronic
Post-Merger	0.790	0.968	-0.094	0.719*	0.071
Standard Error	(1.350)	(0.917)	(0.635)	(0.352)	(1.254)
Observations	3,863	3,863	3,863	3,863	3,863
Number of CoCs	359	359	359	359	359
Pre-Treatment Mean	13.98	5.873	8.107	1.866	12.11
Lower Bound Pct of Mean	-13.35%	-14.23%	-17.33%	1.39%	-19.77%
Point Estimate Pct of Mean	5.65%	16.48%	-1.16%	38.53%	0.59%
Upper Bound Pct of Mean	24.64%	47.19%	12.93%	75.67%	20.95%

Notes: Standard errors clustered at the CoC level in parentheses. Control variables per capita income, unemployment rate, new low-income housing tax credit units, share of the population black, Asian, and Hispanic, population density, poverty rate, 0-bedroom fair market rent, if the governor is a Democrat, TANF 2-person benefit, state labor force per capita, and CoC, and year fixed effects are included in all models. Model is two-way fixed effects with "post-merger" a variable taking the value of one for a CoC after it merges and zero otherwise. Lower/Upper Bound Pct of Mean are lower and upper bounds of the 95% confidence interval in units of percent of the pre-treatment mean. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. *** p<0.001, ** p<0.01, * p<0.05

Table 2.2 Estimates of Homelessness Measures

	HMIS PSH Rate	Total Beds	PSH	HHI	Award	Service Providers	Award per Provider
Post-Merger	0.135**	-1.347	-1.676**	0.009	-1,577	-0.043***	22.64***
Standard Error	(0.051)	(0.815)	(0.635)	(0.009)	(1,759)	(0.010)	(7.167)
Observations	3,830	3,861	3,861	3,845	3,825	3,825	3,825
Number of CoCs	358	359	359	358	360	359	359
Pre-Treatment Mean	0.574	12.51	4.081	0.234	25,358	0.158	158.9
Lower Bound Pct of Mean	6.10%	-23.59%	-71.67%	-3.85%	-19.86%	-39.24%	5.38%
Point Estimate Pct of Mean	23.52	-10.77%	-41.07%	3.85%	-6.22%	-27.22%	14.25%
Upper Bound Pct of Mean	40.94%	2.05%	-10.46%	11.54%	7.43%	-15.19%	23.12%

Notes: Standard errors clustered at the CoC level in parentheses. Control variables per capita income, unemployment rate, new low-income housing tax credit units, the share of the population black, Asian, and Hispanic, population density, poverty rate, 0-bedroom fair market rent, if the governor is a Democrat, TANF 2-person benefit, state labor force per capita, and CoC, and year fixed effects are included in all models. Model is two-way fixed effects with "post-merger" a variable taking the value of one for a CoC after it merges and zero otherwise. Lower/Upper Bound Pct of Mean are lower and upper bounds of the 95% confidence interval in units of percent of the pre-treatment mean. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

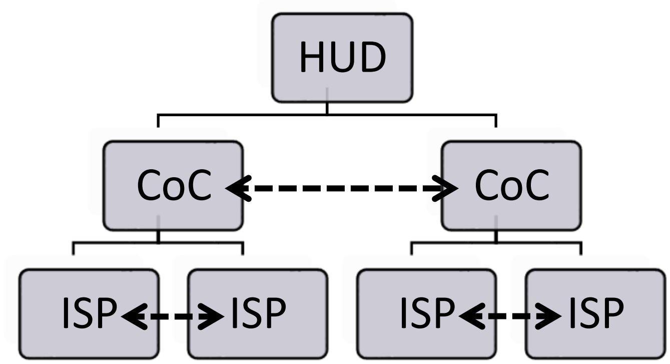
Table 2.3 Estimates of Operational Measures

Figure 2.1 Benefits and Challenges Table form HUD's "CoCs Mergers – What to Consider?"

Potential Benefits	**Potential Challenges**
Increased ability to focus on coordination in a multi-county consortium	Planning efforts will increase significantly for a larger jurisdiction
Economies of scale: 1 annual CoC Program Competition application required instead of 2 or more	Potential reduction of local control over program decision-making
Maximize use of CoC Program funds	The transition of activities will require coordination among the CoCs
Performance and competitive edge likely to improve for metro region	A new project review and priority-setting process will be needed for the new multi-jurisdictional CoC
Regional planning enhanced	

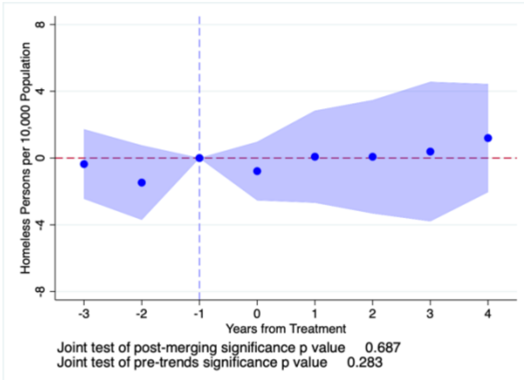
Notes: Published 6 February 2018 by Office of Special Needs Assistance Programs.

Figure 2.2 Continuum of Care Relationship Model

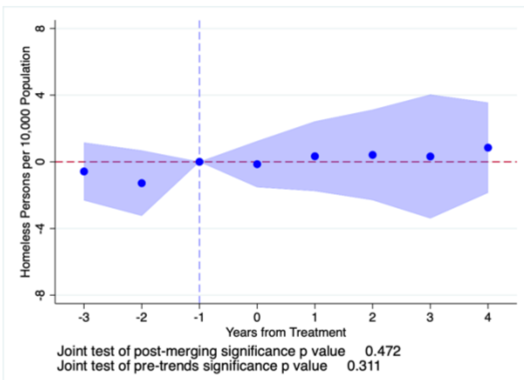


Notes: Solid lines represent coordination. Dashed arrows represent competition. ISP stands for Individual Service Provider, CoC for Continuum of Care, and HUD for Department of Housing and Urban Development.

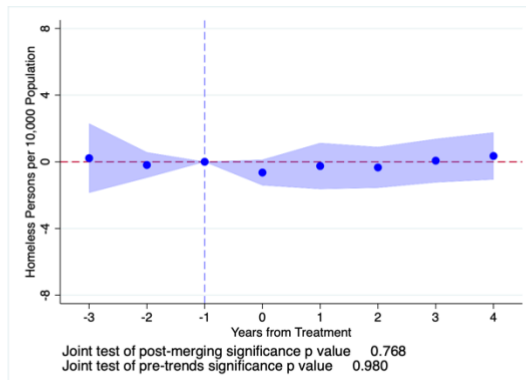
Figure 2.3 Time-Varying Generalized Difference-in-Difference – Homelessness Measures
Panel A. Total Homeless



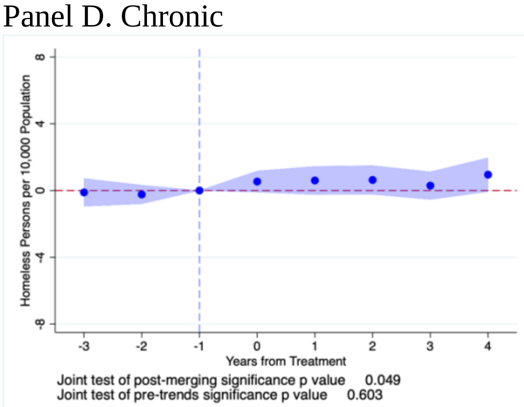
Panel B. Unsheltered



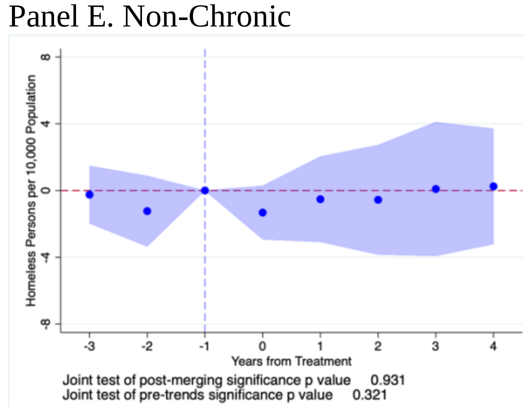
Panel C. Sheltered



Panel D. Chronic



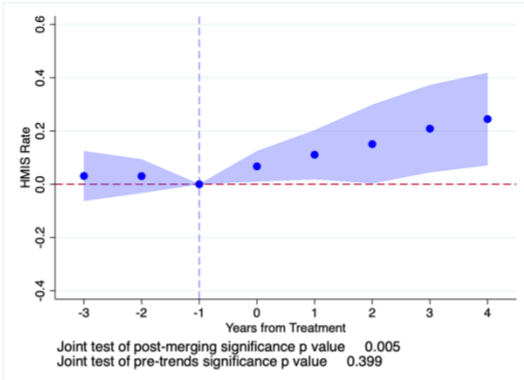
Panel E. Non-Chronic



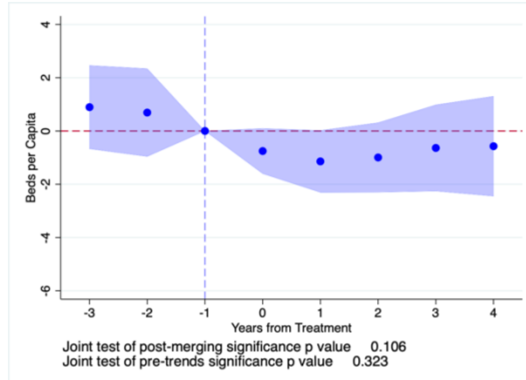
Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables per capita income, unemployment rate, new low-income housing tax credit units, the share of the population black, Asian, and Hispanic, population density, poverty rate, 0-bedroom fair market rent, if the governor is a Democrat, TANF 2-person benefit, state labor force per capita, and CoC, and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017 CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment.

Figure 2.4 Time-Varying Generalized Difference-in-Difference – Operations Measures

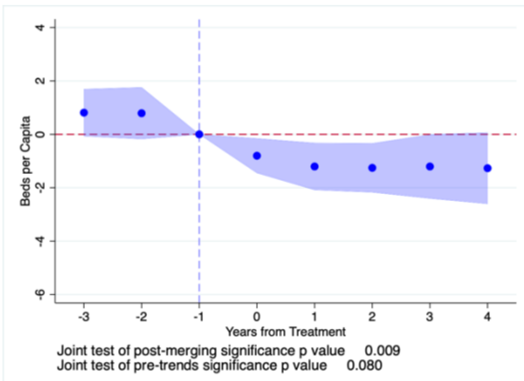
Panel A. HMIS PSH Rate



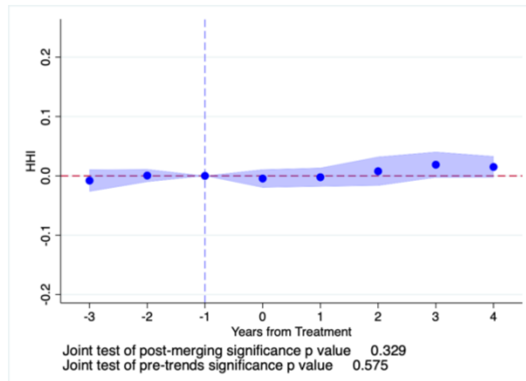
Panel B. Total Beds



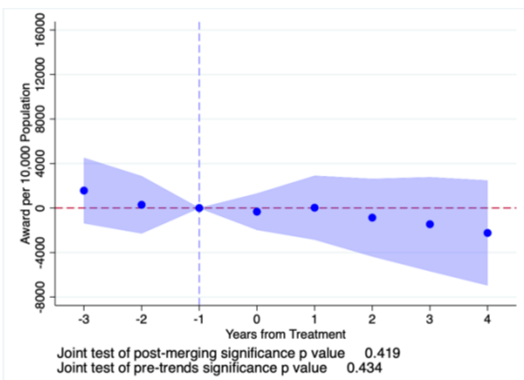
Panel C. PSH Beds



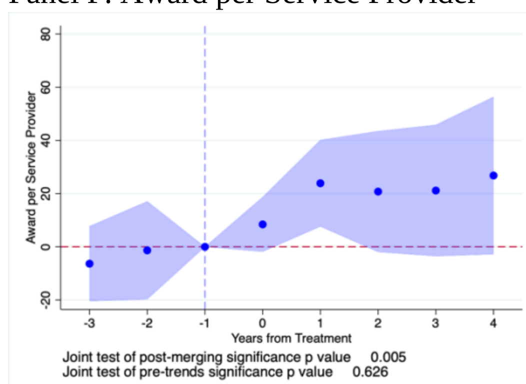
Panel D. HHI



Panel E. Award



Panel F. Award per Service Provider



Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables per capita income, unemployment rate, new low-income housing tax credit units, the share of the population black, Asian, and Hispanic, population density, poverty rate, 0-bedroom fair market rent, if the governor is a Democrat, TANF 2-person benefit, state labor force per capita, and CoC, and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment.

CHAPTER 3. *Helping Homeless Students Succeed: Impacts of McKinney-Vento Grants on the Identification of Homeless Students and Student Achievement*

3.1 *Introduction*

Reported student homelessness has almost doubled over the past decade, from about 0.8 million students in 2008 to 1.5 million in 2019. The growth in homelessness is a product of worsening underlying economic conditions faced by many families but may also reflect school district administrators' responses to financial incentives to identify a larger number of students or increased resources. As students experiencing homelessness tend to have lower test scores and worse educational outcomes, significant resources through Title I Part A and the McKinney-Vento Homeless Assistance Act (M-V) have been devoted to removing academic barriers these students face (Cowen, 2017; Darolia and Sullivan, 2021; Miller, 2011). However, every school district does not receive these funds – only 31% of districts receive M-V grants – with grants typically going to districts with higher rates of homelessness. As opposed to only latent homelessness increasing, the increase in student homelessness may be through the identification of students. The situation raises two pertinent questions. First, do intergovernmental M-V grants increase the identification of students experiencing homelessness? Second, do M-V grants lead to an increase in the share of homeless students proficient on standardized tests, a main outcome stated for grants (Department of Education, 2016)?

Similar questions have been asked in public finance and policy, most notably the identification of special education students. Several studies find an increase in funds for special education students correlates with an increase in the share of students identified as having special education needs (Kwak, 2010; Morrill, 2018). For example, Cullen (2003) finds almost 40% of the increase in special education students could be explained by financial incentives, using variation in Texas's funding. While financial incentives exist for the identification of students experiencing homelessness and administrators have discretion in identifying students, almost no studies have looked at the impact of these incentives.

Also stemming from the availability of M-V grants, the increase could be from better identification of homeless students. The definition of student homelessness is a child or youth lacking "a fixed, regular, and adequate nighttime residence" (42 U.S.C. §11434A(2)(A)). Student homelessness includes those in households sharing housing because of economic hardship or similar reason, about 76% of students experiencing homelessness, as well as those living on the streets or in homeless shelters (Department of Education, 2018; Harvey, 2020; Pavlakis, 2018). Resource-constrained district administrators may have difficulty identifying all students experiencing homelessness as the reason for doubling-up may be unclear or households have several reasons for doubling-up (Cunningham et al., 2010; Department of Education, 2016). Receiving a grant could increase the number of students identified as homeless by providing additional resources to administrators, meaning the number of students experiencing homelessness has stayed constant, but administrators better identify students eligible for services.

To better understand how districts respond to intergovernmental grants for identifying homeless students, I estimate the impact of a school district's receiving a federal McKinney-Vento homeless assistance grant on the number of students identified as homeless and their academic achievement using a school district-level fuzzy regression discontinuity design and state-level interrupted time series. Most states subgrant M-V funds to districts based in part on a district's number of homeless students, but the actual threshold and formula is unknown to fund applicant districts. I therefore estimate implicit homeless student count thresholds in each state and year based on the distribution of homeless students across districts. For example, in 2018 South Carolina in Figure 3.1, the x-axis plots each district's state-by-year percentile its number of homeless students places it into, where the district with the percentile of 100 has the most students identified as homeless. Within a bandwidth of five on either side of the 56th percentile, assignment of grants follows close to a ranking system of the number of homeless students. At least visually, many states follow a similar pattern where, within a bandwidth, there is a discontinuity in the likelihood of receiving a grant, although the discontinuities occur at different points and are unknown without empirical investigation.

I find that receiving a grant did not increase the share of students identified as homeless, meaning identification likely did not cause the increase in homelessness; the increase instead likely represents a true increase in housing insecurity for children and youth. I do so by estimating state-by-year discontinuities in the probability of receiving a M-V grant based on districts' percentiles of homeless students. The discontinuity is then used in a fuzzy regression discontinuity design to plausibly identify the effect of receiving a grant on the identification of homeless students and their test scores, comparing variation in outcomes for districts just above and below the thresholds. Additionally, I find a district's receiving a grant decreased the share of homeless students scoring proficient on state math tests by about 17%. A decrease in revenue from city and county governments may explain the lack of a change in identification and the decrease in achievement.

*****Insert Figure 3.1*****

To further explore if identification increases from amplified funds, I analyze the effect of increased funding availability through the American Recovery and Reinvestment Act (ARRA), using an interrupted time series at the state level from 2005-2017. ARRA doubled the funding available for districts to serve their students experiencing homelessness for one year in 2010, shown in Table 3.1. Additional funding could allow districts to more accurately identify and serve students while simultaneously incentivizing districts to report more homelessness to receive funds. While the number of identified doubled-up students increased after ARRA, it follows the same trend as before ARRA with no discontinuity in 2010, suggesting identification did not cause the increase.

*****Insert Table 3.1*****

I present some of the first evidence that the large increase in student homelessness is likely not driven by identification, either from increased resources or financial incentives. Unlike the identification of special education students, the increase is instead likely driven by economic factors related to homelessness, such as less affordable housing. This may result from funds being less than those for special education. I further show that estimating implicit thresholds can be used to provide causal evidence for policy questions, particularly when there is an implied ranking or treatment begins to happen after a certain point in a given bandwidth (Brunner et al., 2019; McEachin et al., 2020; Porter and Yu, 2015). Estimating implicit thresholds in a fuzzy regression discontinuity design can then provide strong evidence for the effectiveness of policies.

3.2 *Education for Homeless Children and Youths Grants for State and Local Activities*

The U.S. Department of Education (ED) annually gives state education agencies McKinney-Vento (M-V) homeless assistance funds to sub-grant to school districts, with the goal of removing academic barriers students experiencing homelessness face. States have discretion in how many districts to subgrant to and how many funds will go to a given district, so long as at least 75% of the state's funds are distributed to school districts. ED only requires states to consider need and quality of applications on a competitive basis (Department of Education, 2016). Figure 3.2 shows school districts ranked from least homeless students to most within each state and year. Dots are the share of districts receiving a grant for each 0.5 percentile of state-by-year homelessness. A district's probability of receiving an M-V grant positively correlates with its state-by-year percentile of homelessness. For example, only 30% of districts at the median within a state and year receive a grant, 40% of districts at the 80^{th} percentile, and about 50% for the districts with the most. There is consequently variation in the distribution of funds, particularly across states, although need positively relates with the likelihood of receiving a grant.

*****Insert Figure 3.2*****

Allowing states to decide where to distribute grants and giving receiving districts discretion in the use of grants can balance keeping local control of grants' specific uses while increasing equity and directing resources to places with more need (Musgrave, 1959; Oates, 1972, 1999; Cascio et al., 2013). Students experiencing homelessness have heterogenous needs and contexts, facing different barriers to educational achievement, meaning services may take different forms. Pavalkis (2018) describes several housing insecure situations, such as a student in a homeless shelter's main educational barrier as lacking basic necessities like food and safety whereas a student living doubled up may have the challenge of stressed intra-household relationships and transportation to school and services. Decentralized grant distribution lets districts choose services to best help their students' particular needs.

Every school district has a homeless liaison to identify and aid students experiencing homelessness, regardless of whether the district receives an M-V grant. Past studies suggest liaisons have severely limited resources and discrepancies exist across districts in their capacity to identify students (Cunningham et al., 2010; Jozefowicz-Simbeni and Israel, 2006). Homelessness includes students doubled up as well as those living in hotels/motels, a homeless shelter, or are unsheltered. Identification of students doubling up can be particularly malleable as each liaison determines whether economic hardship or loss of housing determined the living situation. Additionally, identifying students who live doubled up generally involves awareness campaigns, student housing questionnaires, and referral forms, leaving much of identification out of the liaisons' and instead in families' control (Cunningham et al., 2010).

Being identified may remove some academic barriers through services faced by students experiencing homelessness. Barriers include stressors such as increased residential mobility, trauma, unstable and unsafe living environments, and increased health issues (Cowen, 2017). Districts must provide identified students with several services, including transportation and expedited enrollment without proof of residency until determination of homelessness is settled. Identified students, particularly in districts receiving M-V grants, often receive other services: tutoring, medical referrals and other educational services that help homeless children and youth reach state standards (Department of Education, 2016).

Additional resources from receiving an M-V grant could allow districts to more accurately identify and serve students by increasing the quality and quantity of reach-out efforts to identify students, the first step in connecting students experiencing homelessness with services. In practice, resources for identification could be more outreach materials, professional development for liaisons and teachers, hiring a social worker for students, or events connecting the community and housing services to district officials. Ideally, these increased efforts would connect students experiencing homelessness to school resources, leading to increased enrollment, attendance, and academic outcomes (Cunningham et al., 2010).

The potential for receiving a grant could also increase identification by incentivizing district administrators to be more lenient in determining whether a given student fits the federal definition and guidelines of homelessness when their status is ambiguous. Being more lenient in identification would increase the perceived need and the thus likelihood of receiving an M-V grant in the future. Administrators could also have a tendency to consider a student whose housing status is unclear as homeless to ensure every student possibly homeless is identified and receives services. However, as identifying a student as homeless comes with essentially unfunded mandates, such as providing transportation, there could be a financial disincentive. Grants would partially offset the disincentive for identification but are small. For example, only $85 were provided through M-V grants per homeless student in districts receiving grants in the 2017-18 school year. The relationship between financial incentives and number of students identified is thus ambiguous without further empirical study.

3.3 *Empirical Approach*

3.3.1 *Fuzzy Regression Discontinuity*

I estimate the effect of receiving a M-V grant on the identification of students experiencing homelessness and the share of homeless students scoring proficient on tests using a fuzzy regression discontinuity (FRD) design (Imbens and Lemeiux, 2008). Since grants on average go to districts with more students experiencing homelessness or achieving the goal of higher test scores, using an FRD can create a quasi-experimental setting to control for these confounding factors as districts just above and below the threshold would be similar but for the increased probability of receiving a grant. As the cutoff for awarding grants to districts is unknown without empirical investigation, I estimate state-by-year thresholds to then compare districts just below and above the cutoff to estimate the impact, as being awarded a grant would be like-random near the threshold. Little room for manipulation by districts exists as the threshold is unknown and informal.[12] Porter and Yu (2015) propose a method to find an unknown discontinuity by estimating where along a running variable the discontinuity is the largest and using that point as the group-by-time threshold. A regression discontinuity is then used to estimate treatment effects within a bandwidth around the estimated point. This method has been used in recent policy literature in the FRD framework, notably Brunner et al. (2019) and McEachin et al. (2020), whose methods I broadly follow.

Many states prioritize need in distributing grants by using the number of students identified as homeless in the districts. As states then conceivably distribute funds based on

[12] Visual tests of manipulation strongly suggest none is present as the distribution within the bandwidths follow a uniform distribution, shown in Figure 3.4.

an implicit ranking by need, it follows that the number of students identified as homeless would positively influence the likelihood of a district's receiving a grant and that, within a state and year, an unknown rank in the number of homeless students would cause a discontinuity within a bandwidth. The percentile is the district's state-by-year percentile of homeless students where a percentile of 100 has the most homeless students. As the number of homeless students may be endogenous to receiving a grant the prior year, the outcome is receiving a grant the following year.

I first estimate, shown in Equation (1), state-by-year linear probability models for the probability of receiving a grant (T_{dsy}) for each state *s* in year *y* for district d.[13] I do this around bandwidths (BW) of 5 percentiles for each whole-number percentile 5 through 95, referred to as ω_{sy}.[14,15]

(1) $$T_{dsy} = \alpha_{isy} d(\omega_{sy} < X_{dsy-1}) + \theta_{11}(X_{dsy-1} - \omega_{sy}) + \theta_{12}(X_{dsy-1} - \omega_{sy}) d(\omega_{sy} < X_{dsy-1}) + \varepsilon_{1dsy}$$

where the sample is $X_{dsy} \in [\omega_{sy} \pm BW]$. The threshold is chosen based on the value of X_{dsy-1} when $\tilde{\alpha}_{isy} > \alpha_{jsy}$ where *j* is all possible thresholds besides *i*. $\tilde{\alpha}_{isy}$ is then the most positive coefficient for that state and year, meaning, for districts within the bandwidth, being above that percentile has the largest discontinuous increase in the probability of receiving a grant for that state and year.

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[13] Appendix B Figure B1 shows the distribution of estimated thresholds.

[14] As sensitivity checks, I also use a bandwidth of 10 and use a quadratic model.

[15] At least two districts must be on either side of the threshold within the bandwidth. 14 states had too few districts or too little variation in percentiles and were thus dropped from estimation: Alaska, Colorado, Delaware, District of Columbia, Hawaii, Louisiana, Nebraska, Nevada, North Dakota, Oklahoma, Puerto Rico, Rhode Island, South Dakota, and Vermont. Only Hawaii, Oklahoma, Puerto Rico, and Vermont are dropped in estimations using a bandwidth of 10.

$d(\omega_{sy} < X_{dsy-1})$ is an indicator variable taking a value of one if the district's percentile is more than the threshold, meaning it had more homelessness than districts below ω_{sy}. $(X_{dsy-1} - \omega_{sy})$ is how many percentiles away the district is from the threshold, with θ_{11} being the relationship between the distance and receiving a grant. θ_{12} is the change in the probability of receiving a grant based on having more homelessness than the threshold and percentiles away. Including these terms allows for different functions on either side of the threshold. ε_{1dsy} are robust standard errors. α_{isy} is the coefficient of interest as the percentile maximizing the discontinuity in a given state and year, $\widehat{\omega}_{sy}$, will be the implicit threshold. Percentiles across districts are then centered by $\tilde{X}_{dsy} = X_{dsy} - \widehat{\omega}_{sy}$, meaning a district with a positive $\tilde{X}_{dsy}$ is above the discontinuity and has a higher probability of receiving a grant.

As the discontinuity is not sharp, meaning some districts above the threshold do not receive grants and vice versa, I use an FRD through a two-stage least squares (2SLS) where Equation (2) is the first stage predicting receiving a M-V sub-grant:

$$(2) \qquad G_{dsy} = \tilde{\alpha} d(0 < \tilde{X}_{dsy}) + \theta_{21}\tilde{X}_{dsy} + \theta_{22}\tilde{X}_{dsy} d(0 < \tilde{X}_{dsy}) + \pi_{2s} + \tau_{2y} + \delta_{2sy} + \lambda_{2dy} + \varepsilon_{2dsy}$$

If $0 < \tilde{X}_{dsy}$, then the district's percentile is above the state-by-year threshold, meaning its probability of receiving a grant increases by $\tilde{\alpha}$, the sample average of being above the threshold. δ_{2sy} are state-by-year fixed effects, controlling for any characteristic common to all districts within that state and year, such as total funding or state policy. π_{2s} are state-level fixed effects, controlling for anything unobserved common to all districts in a state over the years, such as political ideology. τ_{2y} are year fixed effects, controlling for anything common to all districts in a given year, such as the national economy. λ_{2dy} are observable district-level control variables. Although in theory districts just above and below the discontinuity should be similar, I add control variables as some other characteristics, such as being an urban district, could bias results by being related with receiving a grant and the outcome. I include whether the district is urban, suburban, or town relative to being rural, enrollment, share of students identifying as Black/African American or Hispanic, and estimated share of youth in the district living in poverty. Last, ε_{2dsy} are standard errors clustered at the district level.[16]

The probability of receiving an M-V grant then goes into the second stage, Equation (3):

$$(3) \qquad Y_{dsy} = \beta \hat{G}_{dsy} + \theta_{31}\tilde{X}_{dsy} + \theta_{32}\tilde{X}_{dsy} d\left(0 < \tilde{X}_{dsy}\right) + \pi_{2s} + \tau_{2y} + \delta_{3sy} + \lambda_{3dy} + \varepsilon_{3dsy}$$

β is the ultimate parameter of interest as it is the effect of being just above the threshold for receiving an M-V grant on the outcome. Additionally, the local average treatment effect is districts just above the threshold compared to those just below as opposed to the general effect of receiving a grant. The effect is then more applicable to the marginal district receiving a grant than in general.

3.3.2 *Jump Process and American Recovery and Reconciliation Act (ARRA)*

To further study how intergovernmental grants affect the identification of students experiencing homelessness, I study changes in the number of students identified as

[16] For robustness and as districts are assigned with states and years, I re-estimate models first clustering by state and then by state and year, finding similar results.

homelessness after ARRA, estimating a time series jump process at the state level, shown in Equation (4). γ_y are indicators for each year, controlling for any characteristic in a given year common to all states. The omitted year is 2009, the year before ARRA M-V funding takes effect. $\beta_{1,y}$ is an intercept change related to being in a given year, relative to 2009. $\beta_{1,2010}$ is of particular interest as it is the year states received the additional funding. X_{sy} are observable, time-varying state characteristics to control for other possible sources of changes in the identification of homeless students. π_s are state fixed effects, controlling for time-invariant characteristics of states such as political ideology. ε_{sy} are robust standard errors clustered at the state level as errors are likely correlated over time within states.

$$(4) \qquad Y_{sy} = \sum_{y=2007}^{y=2008} \beta_{1,y} * \gamma_y + \sum_{y=2010}^{y=2017} \beta_{1,y} * \gamma_y + \theta X_{s,y} + \pi_s + \varepsilon_{s,y}$$

Although every state received additional funding, each conceivably had three choices: only provide districts already receiving funding more revenue, increase the number of districts receiving a grant, or a combination of the two. A state that increased the number of districts receiving grants could experience alternative outcomes as more districts could have additional resources to identify students. Additionally, it could also signal to districts that funds are available for districts in need, increasing leniency in identification. As shown in Figure 3.3, the share of districts receiving a grant increased sharply after 2009, meaning a change in funding led to changes in how many districts received grants. To exploit the difference in reactions to increased funding, I create an indicator, ϑ_{sy}, shown in Equation (5), for if a state increased the share of districts receiving a grant by at least ten percentage points in 2010, of which 17 states did so. I then interact this indicator with each year indicator. Each $\beta_{2,y}$ is the relationship with being in that year relative to 2009 and states not the share of districts receiving a grant within the state. States increasing the share of districts between zero and ten percentage points are dropped from the sample (22 states).

$$(5) \qquad Y_{sy} = \sum_{y=2007}^{y=2008} \beta_{2,y} * \gamma_y * \vartheta_{sy} + \sum_{y=2010}^{y=2017} \beta_{2,y} * \gamma_y * \vartheta_{s,y} + \sum_{y=2007}^{y=2008} \beta_{3,y} * \gamma_y + \sum_{y=2010}^{y=2017} \beta_{3,y} * \gamma_y + \theta X_{s,y} + \pi_s + \varepsilon_{s,y}$$

*****Insert Figure 3.3 Here*****

3.4 *Data*

3.4.1 *Outcomes*

My first outcome for the FRD analysis is the share of enrolled public-school students identified as homeless by primary residence (doubled up/hotel/sheltered/unsheltered). This provides a measure of homelessness and if incentives for more students increase the share of students identified. Of interest is potential differences between effects on doubled-up students and sheltered students as homeless liaisons have more discretion in identifying students doubling up as they must determine the reason for the living situation as opposed to only context. My second outcome is the share of homeless students taking state standardized math and reading/English language arts (ELA) tests scoring as proficient as this is a stated goal of the grants and a measure of their effectiveness in removing academic barriers.[17] As effects may differ depending on age or grade, I use the overall share as well as splits for third grade and high school test takers.

[17] The share of homeless students scoring proficient is often provided as a range for districts due to privacy concerns. Models shown use the midpoint of this range, although I re-estimate models using the lower bound and upper bound of the range as well as interval estimation, finding similar results in all.

3.4.2 *Sources*

First, district-level homelessness data come from ED's *EDFacts* data files.[18] These cover all districts in the country annually from the 2013-14 school year through 2017-18 and provide information on if the district received an M-V sub-grant and the number of students identified homeless by residence. The number of homeless students is unduplicated within district, but if a student moves districts within a school year and is still identified as homeless in the new district they could be duplicated. The data thus likely over-count the number of identified homeless students on a state and national level.

Data on assessments also come from *EDFacts* and data on enrollment from Common Core of Data. While homelessness and test proficiency are available for the 2013-18 school years. The estimated share of students in poverty come from the U.S. Census Bureau Small Area Income and Poverty Estimates (SAIPE). These data are collected through Urban Institute's Education Data Portal.[19] Last, data for state-level analysis come from Section 1.9 of Consolidated State Performance Reports through No Child Left Behind. I collect reports from ED's *EDFacts Initiative* webpage.[20] Data on control variables for states come from the University of Kentucky Center for Poverty Research and include AFDC/TANF recipients per capita, state EITC rate, gross state product per capita, unemployment rate, the number of persons food insecure per capita, poverty rate, if the state has a Democratic governor, fraction of state House controlled by the Democratic

[18] Data are from file specifications C118 and C170.
[19] Education Data Portal (Version 0.6.0), Urban Institute, Center on Education Data and Policy, accessed March, 11, 2020, https://educationdata.urban.org/documentation/, US Department of Education Common Core of Data, the US Department of Education Civil Rights Data Collection, the US Census Bureau Small Area Income and Poverty Estimates, and the US Department of Education EDFacts Initiative.
[20] https://www2.ed.gov/about/inits/ed/edfacts/index.html

party, and fraction of state Senate controlled by the Democratic party.[21] I also include share of the population identifying as white and number of eviction filings from The Eviction Lab data.[22]

3.5 *Results*

3.5.1 *Descriptive Statistics*

Table 3.2 shows summary statistics for all district-level variables for both the full sample as well as the analytical sample for the FRD at a bandwidth of 5 percentiles. On average, 3% of a district's students experience any type of homelessness in a given year, while about 2% experience being doubled-up and 0.4% shelter stays. Only 32% of homeless students score proficient on math and 37% on ELA. The analytical sample is slightly over-representative of districts receiving M-V grants and with higher rates of homelessness, although this is expected as resources follow need.

Figure 3.4 shows that manipulation does not occur around estimated thresholds, with an almost uniform distribution. If there were manipulation around the threshold, one would see a drop in density just below the threshold and a sudden increase in districts just above the threshold. Table 3.3 shows summary statistics for control variables by being above or below the estimated threshold for a bandwidth of 5 percentiles. Most characteristics are similar, although those above the threshold were more likely to be urban as opposed to rural and had higher enrollment. Figure B1 shows the distribution of

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[21] University of Kentucky Center for Poverty Research. (2019, Dec.). UKCPR National Welfare Data, 1980-2018. Lexington, KY. Available at http://ukcpr.org/resources/national-welfare-data.

[22] Eviction filings are from The Eviction Lab at Princeton University, a project directed by Matthew Desmond and designed by Ashley Gromis, Lavar Edmonds, James Hendrickson, Katie Krywokulski, Lillian Leung, and Adam Porton. The Eviction Lab is funded by the JPB, Gates, and Ford Foundations as well as the Chan Zuckerberg Initiative. More information is found at evictionlab.org.

estimated thresholds. Most thresholds are above the 65th percentile, with a high concentration at the upper limit, consistent with states prioritizing need and beginning sub-grants after a certain point of homelessness.

Figure 3.5 shows results from the first stage of the FRD where control variables are also included. Each marker represents the average share of districts in that bin of 0.20 percentiles receiving a McKinney-Vento Grant. The x-axis shows districts' percentiles away from the estimated threshold, the dashed, vertical line. Being just above the threshold is related with about a 43 percentage point increase in the probability of receiving a grant. The Kleibergen-Paap rk Wald F statistic also averages at 284 in models for homelessness or overall test proficiency outcomes, providing evidence for a strong first stage.[23] The following FRD result figures follow a similar format to Figure 3.5, with the p-value for the discontinuity also below each panel. All models for figures include observable, time-varying control variables, as well as state, year, and state-by-year fixed effects.

*****Insert Figures 3.4 and 3.5*****

3.5.2 *Identification of Students Experiencing Homelessness*

In Figure 3.6, receiving a grant likely does not affect rates of students identified as living in a homeless shelter and those living doubled-up.[24] A discontinuity in homelessness does not seem to exist visually for doubled-up homelessness, suggesting statistical insignificance is not solely from wide standard errors but from no true effect. Although the number of students identified as living doubled up increases as the district's state-by-year percentile increases, there is no discontinuity at the threshold, suggesting

[23] Table A6 shows first stage results across other specifications.
[24] All results are available in table format in Appendix A.

receiving a grant does not lead to increased identification, either from increased leniency or better identification. The result for sheltered homelessness is less clear as there is a small visual discontinuity. The estimated coefficient is also 0.13, which is about 24% of the mean for the analytical sample, which would be a sizeable effect if statistically significant.[25]

*****Insert Figure 3.6*****

3.5.3 *Academic Proficiency*

In Figure 3.7, receiving a grant seems to decrease the share of homeless students testing proficient on math by about five percentage points (17% of the mean for the analytical sample). While the share of homeless students proficient on math increases up to the discontinuity, there is a sudden drop at the discontinuity with a flatter slope. Reading proficiency does not have a similar discontinuity or has a negligible effect at most. To see if the result applies to different ages of students experiencing homelessness, I next split proficiency by grade. While statistically insignificant, Figure 3.8 visually suggests the decrease in proficiency may come from earlier grades as opposed to later.[26] The estimated coefficient for 3rd grade math proficiency is a decrease at the continuity by about four percentage points, or 11% of the mean. High school proficiency, on the other hand, has no discontinuity, suggesting no effect of the grants.

*****Insert Figures 3.7, 3.8, and 3.9*****

[25] As this could also suggest an unobserved determinant of increased likelihood in receiving a grant by having more severe homelessness, I re-estimate all models controlling for share of homeless students that are sheltered, finding results to be similar.

[26] Figure 9 shows results for reading proficiency by grade, with no effect.

3.5.4 *Exploring Mechanisms: Alternative Revenues and Behavior*

Several mechanisms could lead to no change in identification and a decrease in proficiency. First, support for students experiencing homelessness may try to help students in ways that do not translate into increased identification or improved proficiency on state standardized tests. Second, a decrease of other resources may mean grants do not lead to more resources as intended. Last, following social isolation theory, a district's receiving a grant and providing services for homeless students may stigmatize them, leading to negative psychological effects which decrease their likelihood of scoring proficient (Aviles de Bradley, 2011).

Exploring the theory of decreased alternative revenue sources, school districts, local governments, and states may respond to receiving grants by decreasing funding through substituting away from other revenue sources. While on one hand, receiving an M-V grant could signal need in a district, amplifying funding to it and any positive effects of more resources (Hines and Thaler, 1995; Cascio et al., 2013). Increased intergovernmental revenue would further incentivize actions increasing the likelihood of receiving a grant. Alternatively, receiving an M-V grant may decrease other sources of funding (Gordon, 2004). A decrease in these other revenues would neutralize positive revenue from the grant and associated increases in capacity to identify students along with incentives to receive a grant. For example, the local government may provide fewer resources to a receiving district as the homeless students already receive revenue from the state to support their education.

I use several financial indicators to test the theory of financial effects. First, I use revenue from the local city and county governments as receiving a new grant to help homeless students within the local boundary could decrease revenue from local governments as they would directly serve these students. Second, as previous literature has found Title I funding to crowd-out state funding, I see the effect on Title I revenue per student. Last, as receiving a grant may change districts' expenditures, I use spending per student on transportation and student support services as these most relate to services for students experiencing homelessness. These data come from the Common Core of Data and are for 2013-2017.

Figure 3.10 provides some evidence toward the alternative revenues theory. Panel A suggests a district that receives a grant obtains half the revenue per student from local city and county governments. Revenue from the city/county government per student steeply increases up to the threshold to about $400 per student, then drastically decreases to between $100 and $200 per student. Panel B on the other hand shows grants do not decrease Title I revenue as there is no discontinuity at the threshold, although there is wide variation. Some evidence therefore exists that M-V grants decrease other sources of revenue. A decrease in alternative revenues means there may be no net financial gain or incentive, possibly explaining some of the lack of a change in identification and decrease in proficiency of homeless students. Additionally, Figure 3.11 suggests no changes in spending on student support services or transportation per student.

*****Insert Figures 3.10 and 3.11*****

Receiving an M-V grant could also impact behavior or discipline in a district. Experiencing homelessness often carries stigma for students, which a district's receiving a grant could exacerbate by drawing attention to these students (Aviles de Bradley, 2011; Miller, 2011). Bullying could then increase from this increased attention. Alternatively, homeless students could be subject to more disciplinary actions due to stressors at home spilling over into school. I therefore estimate the effect on the number of harassment allegations related to race or disability per 1,000 students in a district, using data from the Civil Rights Data Collection. While not directly related to homelessness, race intersects with homelessness, changing how schools support students (Aviles de Bradley, 2015). As worse behavior can lead to more disciplinary actions, I include both in-school suspensions and out-of-school suspensions per 1,000 students. CRDC data only overlaps with homelessness data for 2016.

Figures 3.12 and 3.13 suggest receiving a grant does not change harassment allegations or suspensions per 1,000 students in a district. Although the statistical insignificance could be from a smaller sample size from only having one year, there is no indication a discontinuity exists. Based on the coefficients, the largest changes would be an increase in race allegations by 10% and in school suspensions by about 16% from the mean, although these estimates are very imprecise with large standard errors. Grants could still increase harassment but at a level smaller than a district, such as a classroom. Additionally, stigma could also still increase for homeless students that is unrelated to harassment or discipline but spills over into identification and academic achievement.

*****Insert Figures 3.12 and 3.13*****

3.5.5 *Jump Process Results*

Figure 3.14 shows the results of the jump process around ARRA. Each marker shows the coefficient from that year's indicator with a base year of 2009, with shaded areas showing the 95% confidence intervals. Each coefficient is then the average difference in homelessness across states, after controlling for states' fixed and time-varying characteristics, relative to 2009. Sheltered homelessness initially fell in 2010 and has steadily risen since. The number of students living doubled up increased after ARRA, shown in Panel B. However, ARRA likely did not cause the increase as doubled-up homelessness was already increasing and there was not a jump in 2010.

*****Insert Figure 3.14*****

In Figure 3.15, markers are coefficients showing the difference in the number of students identified as homeless relative to 2009 and states not increasing their share of districts receiving a grant. Sheltered homelessness has an inverse-U shape, initially decreasing in 2010 and then increasing until 2013 when it began decreasing again. Doubled-up homelessness instead increased after ARRA and stayed relatively constant. While it increased in 2010 on average, it peaked in 2011, which is also individually statistically significant. Additionally, there is no evidence of pre-trends in Figure 3.15. This suggests states increasing the share of districts receiving an M-V grant by more than ten percentage points to have had more students identified as living doubled up in 2011 relative to 2009 and compared to those that did not increase the percent of districts receiving a grant. It would also be an increase of 32% from the pre-2010 mean. However, using the point estimate, sheltered homelessness per student is also 18% higher than its pre-2010 mean. Therefore, there is limited evidence states which increased the share of districts receiving a grant also had an increase in homelessness due to identification.

*****Insert Figure 3.15*****

3.5.6 *Sensitivity Checks*

To test the sensitivity of results, I conduct several sensitivity checks, results from which are shown in Appendix A. First, I increase the bandwidths to 10 to test sensitivity to bandwidth. Second, I re-estimate models using quadratic distance terms instead of linear. I also estimate models without controls as they are theoretically not needed if results are like-random in actuality. Results from these specifications are generally similar in statistical significance and magnitude. Table B1 also shows results from non-instrumental variable models, using an indicator for whether the district received an M-V grant, as well as control variables and state, year, and state-by-year fixed effects. Last, I also estimate models using proficiency on tests for all students in a district, finding no effect, providing further evidence the mechanism for the decrease in homeless students' proficiency is related to homeless students, not the district at large.

3.6 *Discussion*

While the number of students identified as homeless has drastically risen since 2007, it has remained ambiguous whether the increase has occurred from identification or an increase in housing insecurity. As financial incentives have been shown in related literature to affect measurement, such as the identification of special education students, it could have been the increase occurred from incentives to receive grants instead of economic reasons. If this were the case, policies would perhaps change to eliminate the incentive and not focus on housing insecurity. Alternatively, grants could increase the identification of students by providing additional resources. The increase would then be in identified homeless students as opposed to an increase in the true number of students experiencing homelessness.

However, I present some of the first evidence that the increase in student homelessness is not from identification, which has two main policy implications. First, past literature on financial incentives' increasing the identification of students with a given condition, such as eligible for special education, may not apply to students experiencing homelessness or similar situations. One reason this could occur is a low financial incentive as relates to serving homeless students relative to other student support services. For example, only $85 million was provided in M-V grants in the 2017-18 school year. This translates into about $21,500 per district receiving a grant or $85 per student identified as homeless in districts receiving grants. As district must provide services for students identified as homeless, such as transportation, even receiving a grant may not be enough to cover the costs of providing each student the required services. Further, if there a decrease in alternative revenues from receiving a grant, such as revenue from city and county governments, then there could even be a financial disincentive to identify homeless students.

Second, as the increase in the share of students experiencing homelessness, particularly students living doubled up, is likely not identification, it is likely housing insecurity has become worse for children and youth over the 2010s. Being doubled up can be a strong signal of a future problem of severe cases of homelessness, such as those living on streets or in shelters (O'Flaherty, 2019). As doubling-up is also a result of extreme poverty as relates to housing costs, it also suggests more support will be needed to students experiencing poverty, as well as housing instability or mobility in general (Cowen, 2017). Policymakers may use this finding as a signal of the problem and design solutions to prevent the problem before it worsens. Additionally, as homelessness and mobility likely decrease educational outcomes for students, increasing housing stability could indirectly increase educational outcomes (Pavlakis, 2018).

Despite being a goal of the M-V grants, I do not find grants to increase proficiency on state standardized tests for homeless students. This could be from several mechanisms. First, as described in the last section, M-V grants do not provide a large amount of additional funding for districts, meaning the resources may be exhausted after providing required services with none left for educational services like tutoring. Substitution away from other revenues could be an unintended effect that thwarts the goal. Second, being identified as homeless could increase stigma from classmates or even teachers (Cowen, 2017). While I provide a limited analysis using CRDC data, a student-level analysis can provide greater inference as to how the stigma of homelessness affects education above housing insecurity. Last, it may be grants direct services to other areas of a student's life than purely education, such as making sure they have additional socio-emotional support, counseling, or material necessities. In the long-term, this may improve test scores for a student who experienced homelessness; my analysis only looked at proficiency for students who were homeless during the year which they were tested. A greater understanding of the dynamics of student homelessness, its long-term effects, and impact of services can provide inference as to the mechanism by which grants decrease the share of homeless students proficient on tests (Darolia and Sullivan, 2021).

Apart from literature on housing insecurity and education, this study also contributes to the greater discussion in public economics and finance of how financial incentives influence actions of government actors. While several previous studies find financial incentives play a large role in the identification of a perceived problem, I suggest this to not always be the case for programs. The difference could result from fewer financial incentives, homelessness's being a relatively rare condition with less than 3% of students experiencing in a year, or some other factor. Future work can explore other contexts through which financial incentives may be unrelated to identification. I also contribute the literature on using implicit thresholds in a regression discontinuity framework to provide causal evidence. Prior work typically used implicit thresholds in a context where there is a clear threshold, but it is unknown, such as test scores to be placed into a program (Brunner et al., 2019; McEachin et al., 2020). I build from this literature to extend it to a context where a discontinuity may not exist. However, I show the methods can be used even when there is only an implied ranking where treatment also occurs after a certain point within a bandwidth, such as states' prioritizing giving grants to school districts with worse homelessness. Future studies can look for additional areas where this method can be applied to increase the knowledge about the effectiveness of policies.

3.7 *Tables and Figures*

Table 3.1 Total McKinney-Vento Funding by Year

Fiscal Year	Grants
2006	$61,871,040
2007	$61,871,040
2008	$64,066,851
ARRA	$70,000,000
2009	$65,427,000
2010	$65,427,000
2011	$65,296,146
2012	$65,172,591
2013	$61,771,052
2014	$65,042,000
2015	$65,042,000
2016	$70,000,000
2017	$77,000,000
2018	$85,000,000
2019	$93,500,000

Notes: Source is U.S. Department of Education, Education for Homeless Children and Youths Grants for State and Local Activities Funding Status page.

	Full Sample			Analytical Sample		
	N	Mean	SD	N	Mean	SD
MV Grant Recipient	61,632	0.308	0.461	4,220	0.415	0.493
Share Homeless	60,994	3.103	5.189	4,217	4.278	4.145
Share Doubled Up	61,015	2.123	4.033	4,218	2.989	3.570
Share Sheltered	61,007	0.380	1.486	4,218	0.535	1.488
% Homeless Proficient on Math	29,809	31.68	19.85	2,885	29.60	18.28
% Homeless Proficient on Math (3rd Grade)	10,964	36.48	21.08	1,354	35.28	20.16
% Homeless Proficient on Math (High School)	8,331	33.69	22.44	1,002	33.75	22.89
% Homeless Proficient on Reading	29,930	36.52	20.59	2,886	33.92	18.47
% Homeless Proficient on Read (3rd Grade)	10,904	35.91	21.49	1,355	33.71	19.96
% Homeless Proficient on Read (High School)	8,616	41.70	23.08	1,038	41.26	23.00
Revenue from City/County Government per student	35,172	331.0	2,290	1,966	188.9	822.4
Title I per student	35,177	308.7	1,212	1,966	336.4	615.4
Transportation Exp. per student	35,176	5,664	60,037	1,966	4,731	5,243
Student Support Exp. per student	35,177	682.0	7,951	1,966	558.0	643.6
ISS per 1,000 students	12,080	55.86	67.90	1,035	61.85	62.65
OSS per 1,000 students	12,080	58.05	87.21	1,035	61.07	75.46
Allegations (Disability) per 1,000 students	12,065	0.512	14.21	1,032	0.281	1.369
Allegations (Race) per 1,000 students	12,065	1.048	14.74	1,032	0.996	4.850
Share in Poverty	51,438	18.33	9.687	3,448	19.47	9.288
City/Urban Locale	47,602	0.0775	0.267	3,365	0.126	0.332
Suburban Locale	47,602	0.263	0.440	3,365	0.269	0.444
Town Locale	47,602	0.210	0.407	3,365	0.231	0.421
Share Enrollment Black	61,091	11.39	21.39	4,218	13.66	22.15
Share Enrollment Hispanic	61,091	15.44	20.92	4,218	17.97	21.65

Enrollment (1,000s)	*61,187*	*3.919*	*12.97*	*4,218*	*5.273*	*12.83*

Notes: Analytic sample includes observations where percentile of doubled-up students is within 5 percentiles of estimated threshold. Observations are school district by year.

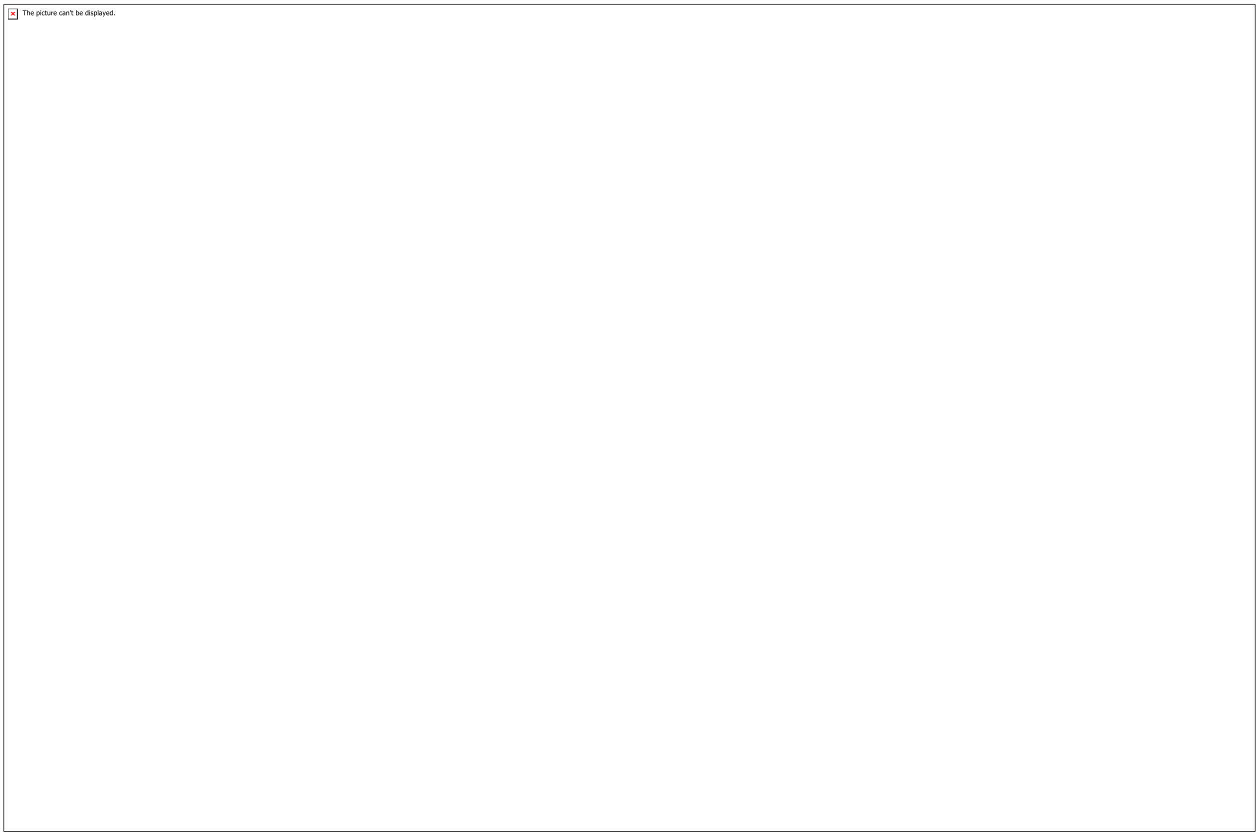
Table 3.2 Summary Statistics

Table 3.3 Summary Statistics by Threshold

	Below Threshold		*Above Threshold*	
	N	Mean	N	Mean
Share in Poverty	1,726	19.2	1,722	19.8
City/Urban Locale	1,686	9.7	1,679	15.5
Suburban Locale	1,686	27.5	1,679	26.3
Town Locale	1,686	24.4	1,679	21.7
Share Enrollment Black	2,117	13.0	2,101	14.3
Share Enrollment Hispanic	2,117	17.3	2,101	18.6
Enrollment (1,000s)	2,117	4.5	2,101	6.0

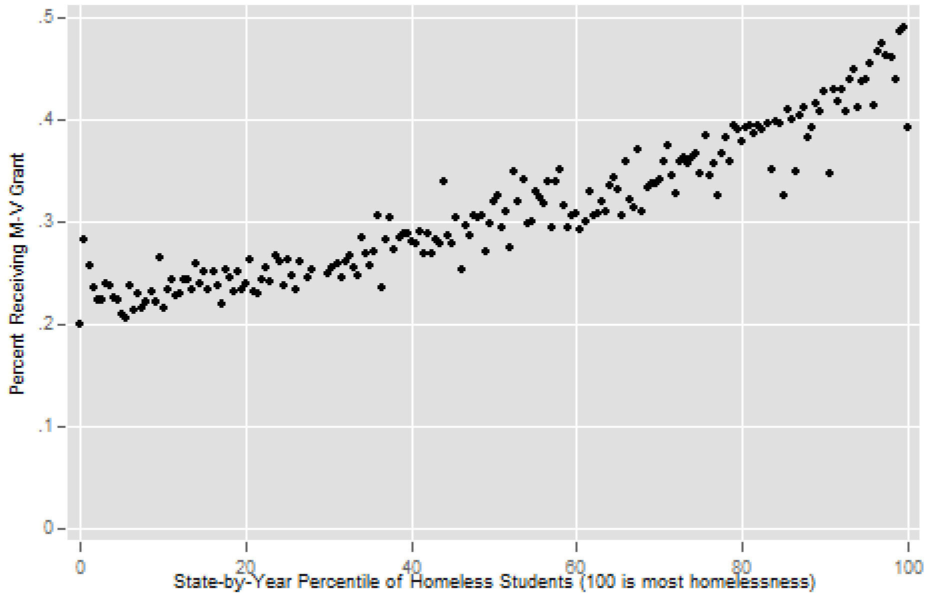
Notes: Analytic sample includes observations where percentile of doubled-up students is within 5 percentiles of estimated threshold. Observations are school district by year.

Figure 3.1 South Carolina 2014 School Districts receiving McKinney Vento Grant by Number of Homeless Students Percentile



Notes: Data come from Department of Education's *EDFacts* database, file specification C118. Graph shows South Carolina school districts ranked from least homeless students to most, by whether it received a McKinney-Vento Homeless Assistance grant in the 2017-18 school year. Each circle is one district.

Figure 3.2 Average number of districts receiving a grant by percentile

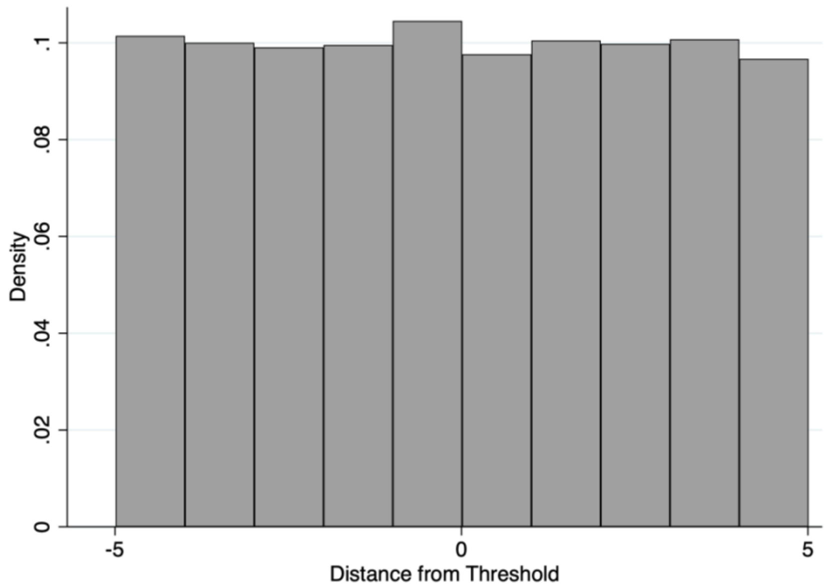


Notes: Data come from Department of Education's *EDFacts* database, file specification C118. Graph shows school districts ranked from least homeless students to most within each state and year. Dots are the percent of districts receiving a grant for each 0.5 percentile.

Figure 3.3 Percent of LEAs Receiving Grants

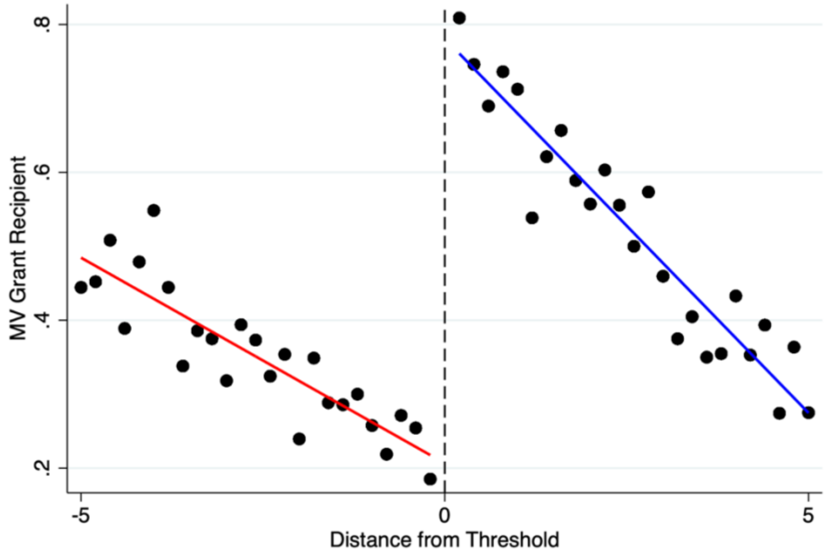
Notes: Data come from Section 1.9 of Consolidated State Performance Reports. Line shows percent of LEAs average number of LEAs receiving a McKinney-Vento grant each year from 2006-2017. Vertical red line shows when the additional $70 million from the American Recovery and Reinvestment Act took place.

Figure 3.4 Manipulation



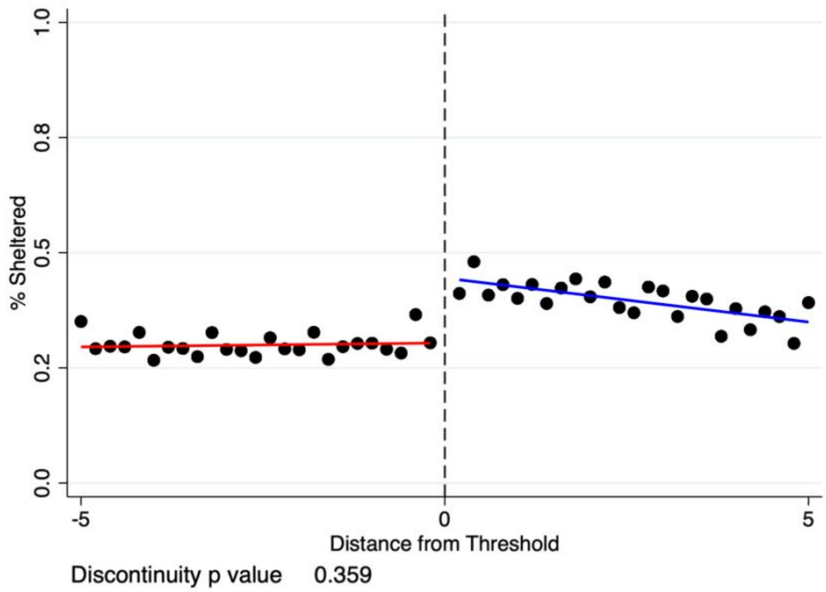
Notes: This graph shows the distribution of districts around the estimated thresholds with a bin width of one percentile. Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The x-axis shows districts' percentiles away from the estimated threshold.

Figure 3.5 Discontinuity

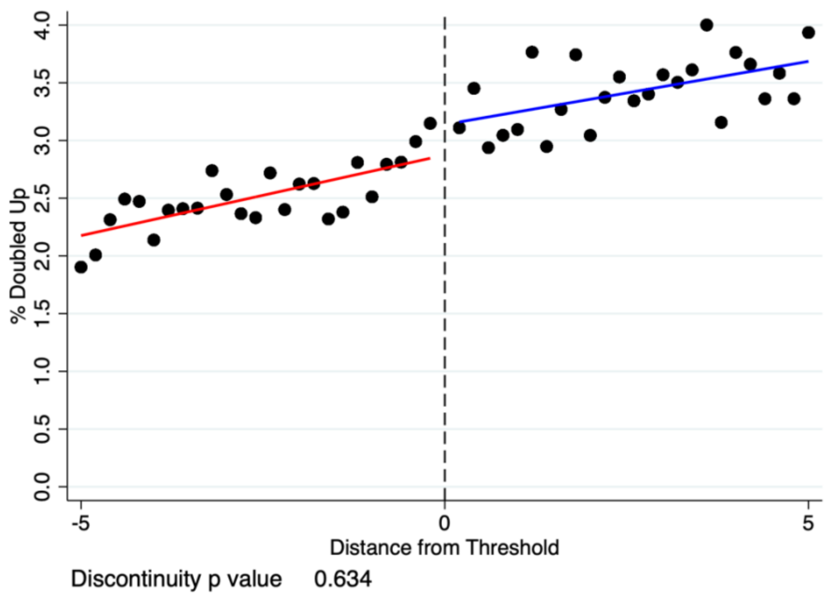


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. This graph shows the first stage of predicting the likelihood of receiving a grant the following year. The x-axis shows districts' percentiles away from the estimated threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles. 3,312 observations.

Figure 3.6 Homelessness
Panel A. Share Sheltered



Panel B. Share Doubled Up

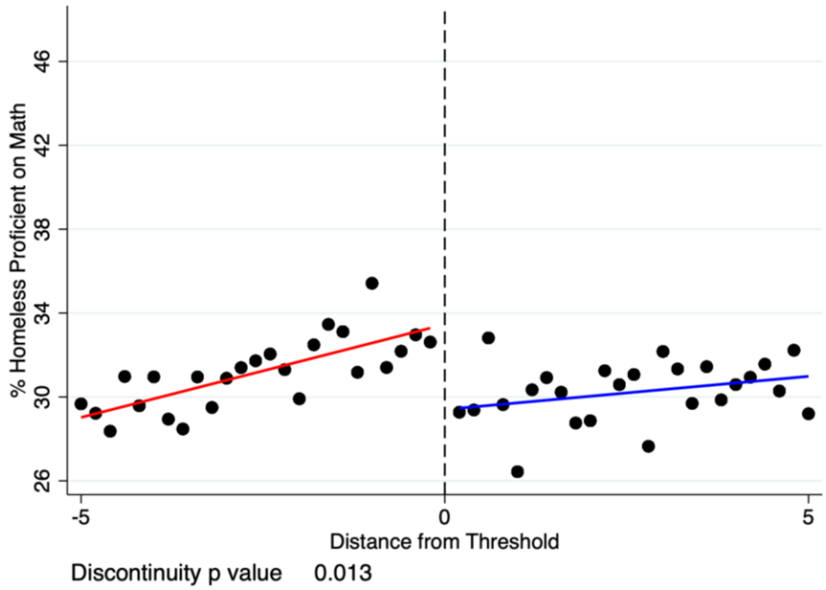


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. The x-axis shows districts' percentiles away from the estimated

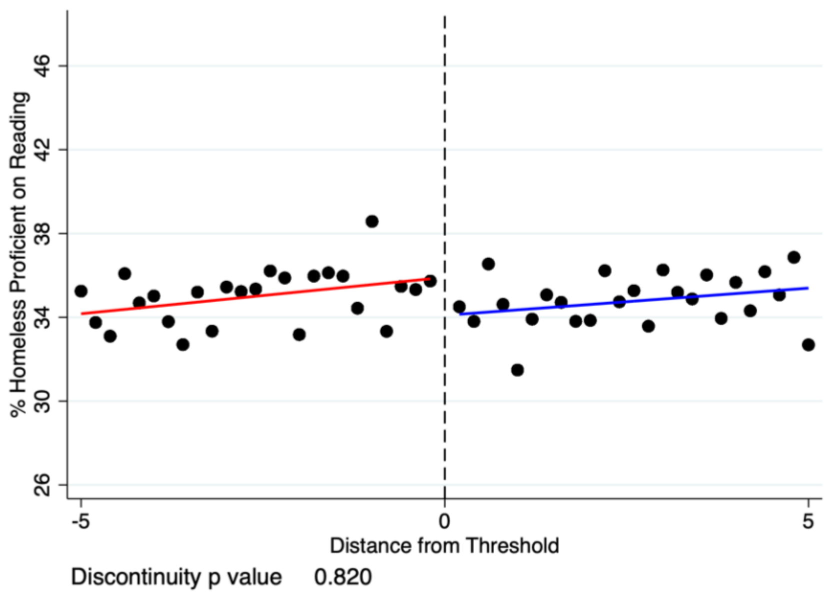
threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles. 3,312 observations.

Figure 3.7 Test Proficiency

Panel A. Percent of Homeless Students Proficient on Math



Panel B. Percent of Homeless Students Proficient on Reading

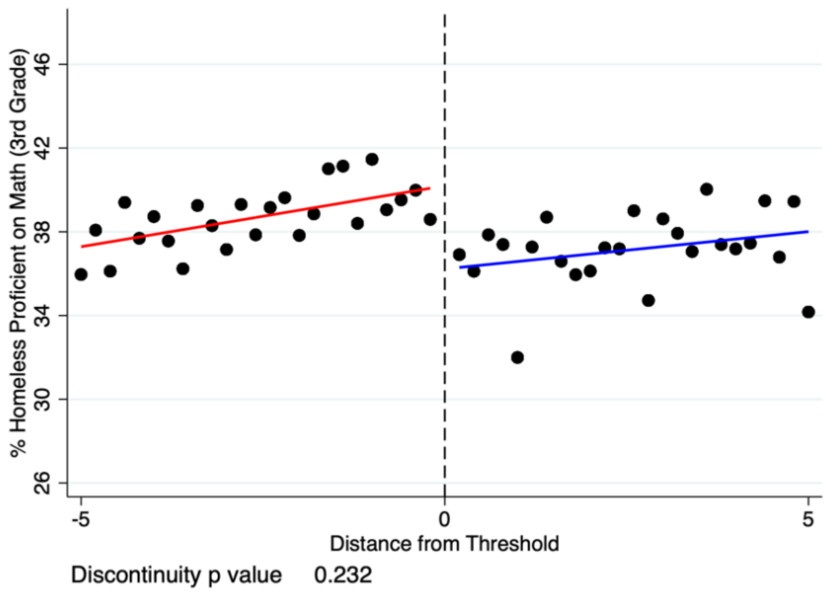


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. The x-axis shows districts' percentiles away from the estimated

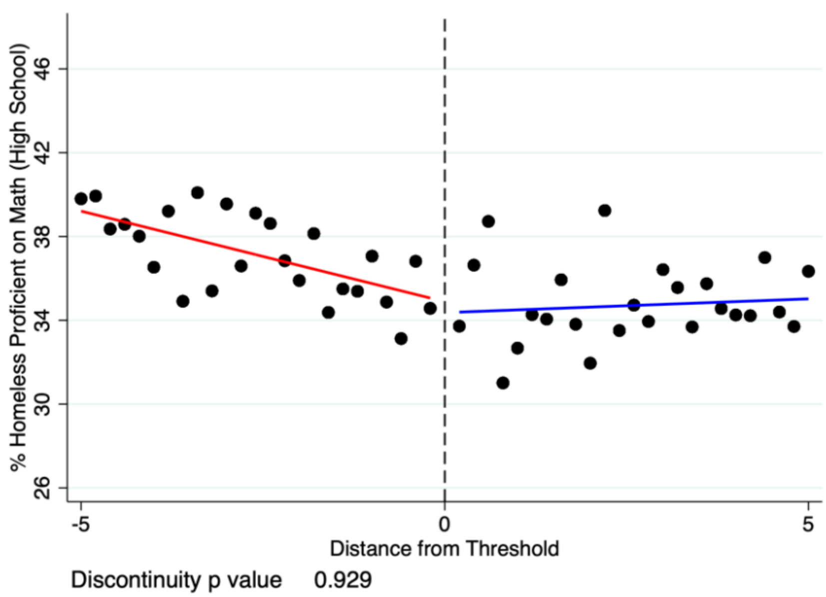
threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles. 2,621 and 2,620 observations, respectively.

Figure 3.8 Math Proficiency by Grade

Panel A. Percent of Homeless Students Proficient on Math – 3rd Grade



Panel B. Percent of Homeless Students Proficient on Math – High School

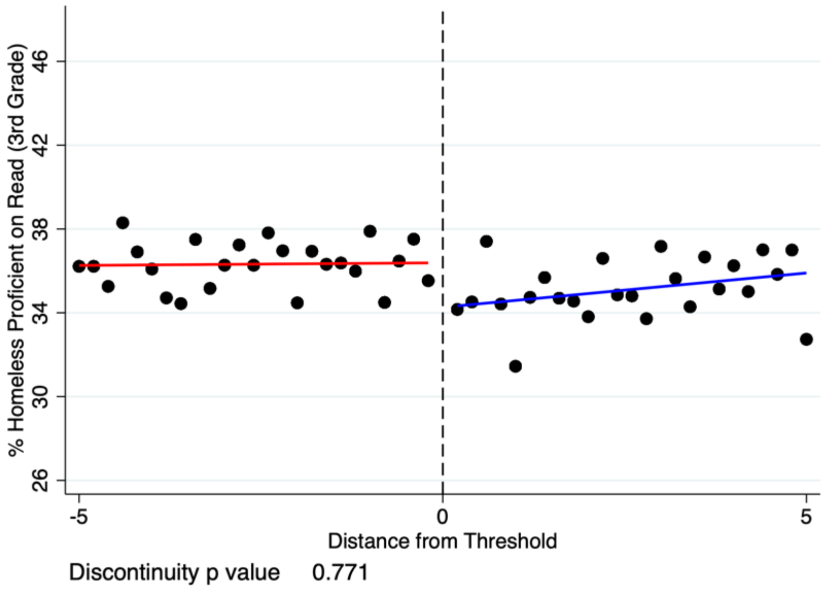


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. The x-axis shows districts' percentiles away from the estimated

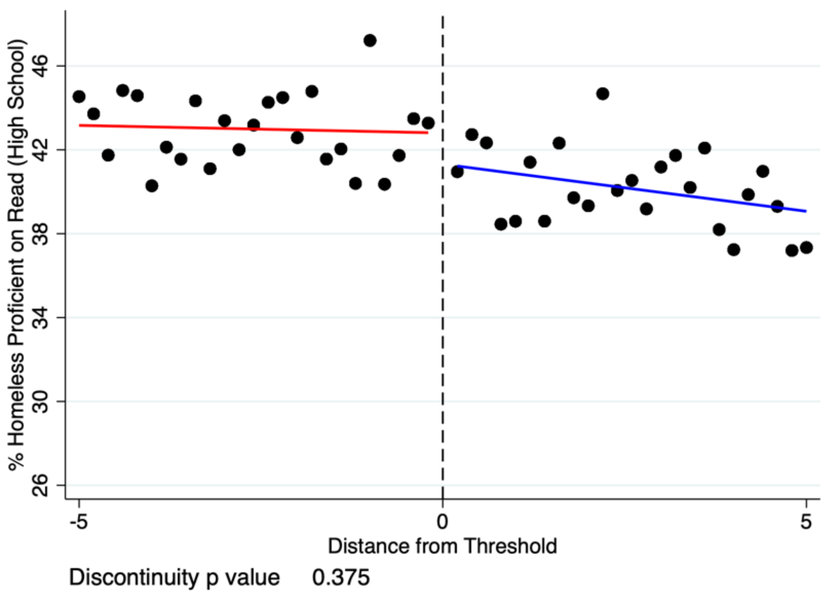
threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles. 1,299 ad 929 observations, respectively.

Figure 3.9 Reading Proficiency by Grade

Panel A. Percent of Homeless Students Proficient on Reading – 3rd Grade



Panel B. Percent of Homeless Students Proficient on Reading – High School

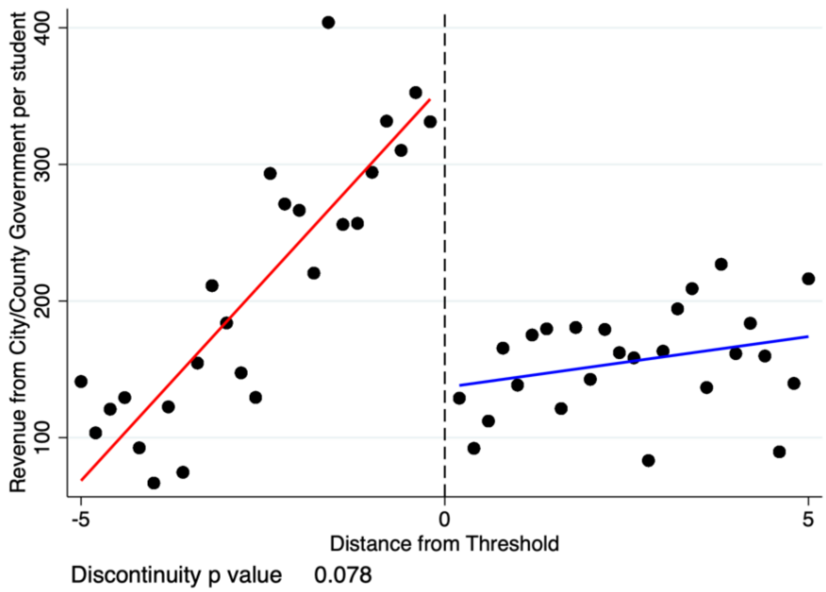


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. The x-axis shows districts' percentiles away from the estimated

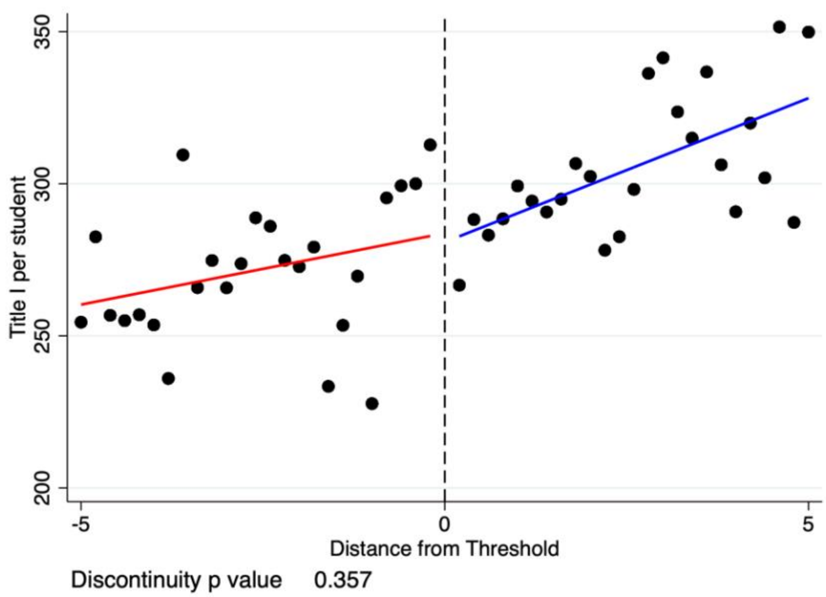
threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles. 1,297 and 957 observations, respectively.

Figure 3.10 Revenue

Panel A. Revenue from City/County Government per Student



Panel B. Title I Funding per student

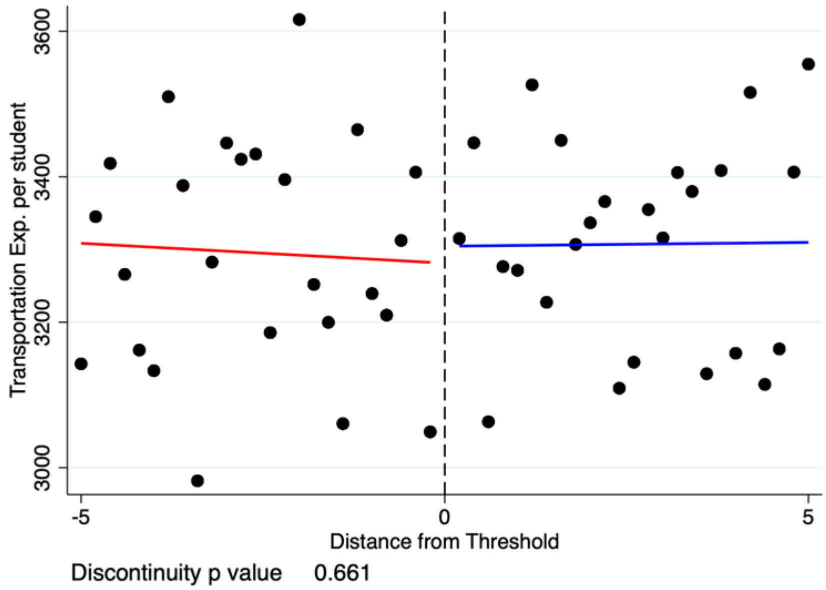


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. The x-axis shows districts' percentiles away from the estimated

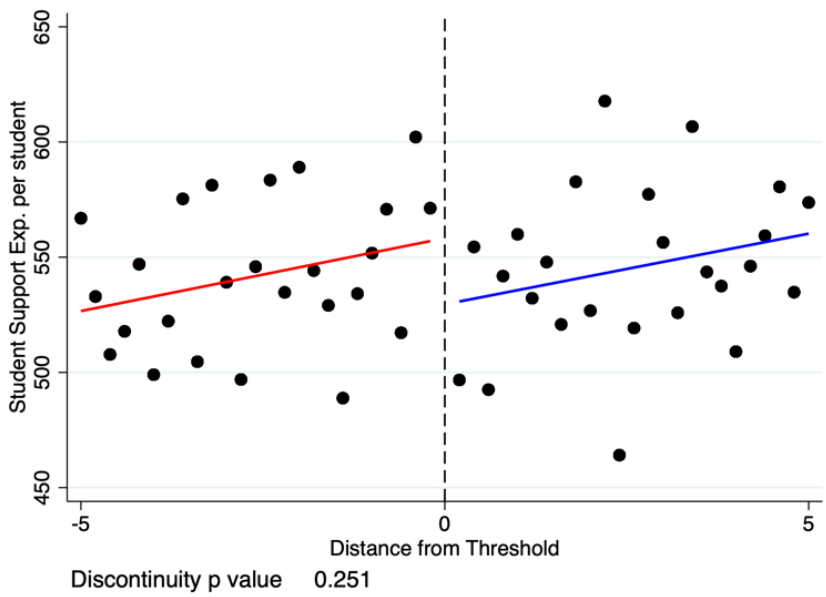
threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles.

Figure 3.11 Expenditures

Panel A. Support Service Expenditures per student



Panel B. Transportation Expenditures per student

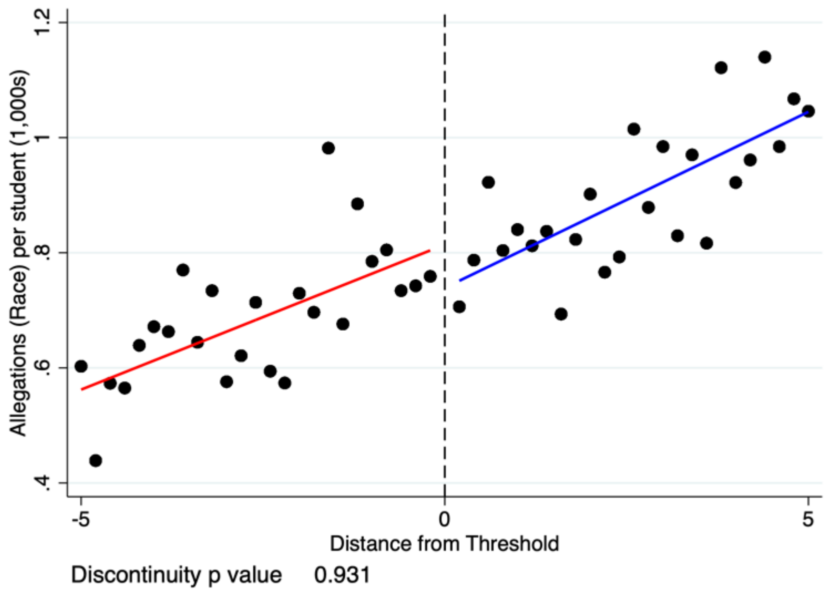


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. The x-axis shows districts' percentiles away from the estimated

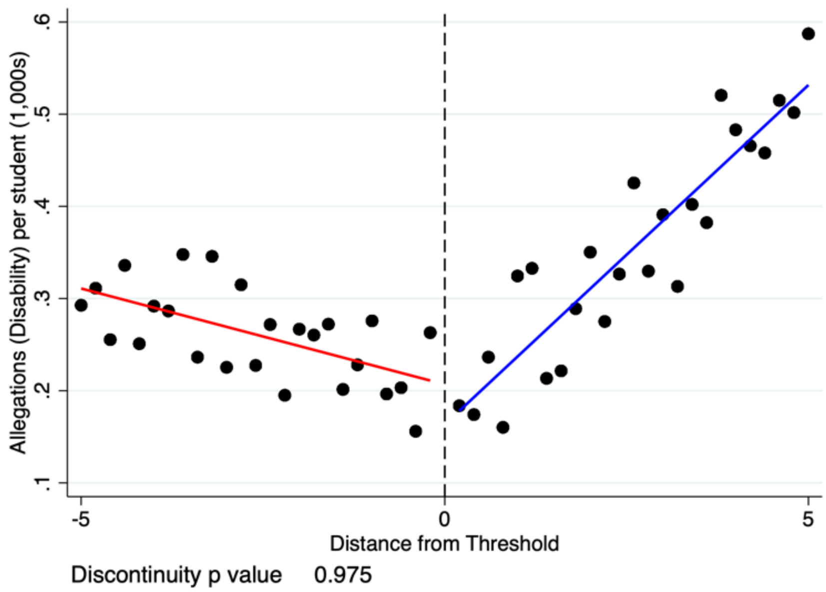
threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles. 1,688 observations.

Figure 3.12 Harassment Allegations

Panel A. Harassment Allegations per 1,000 Students - Race



Panel B. Harassment Allegations per 1,000 Students – Disability

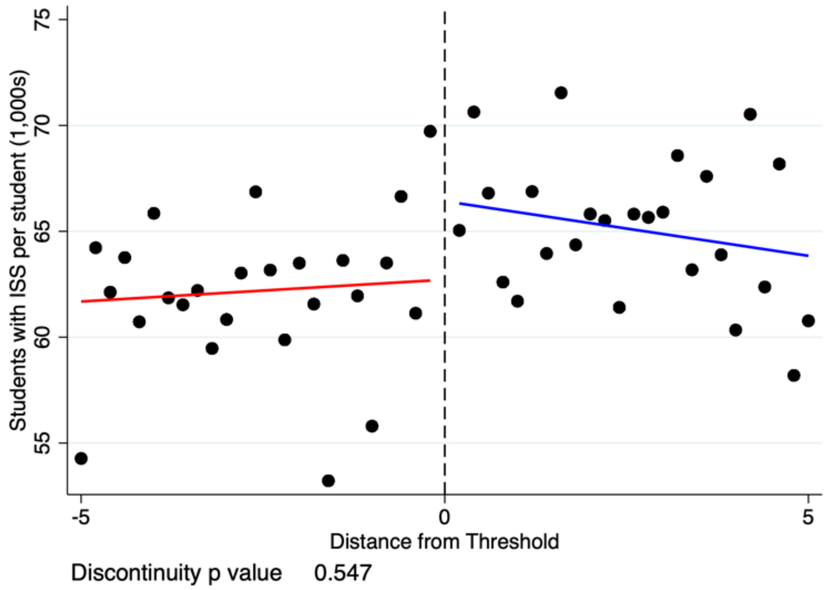


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. The x-axis shows districts' percentiles away from the estimated

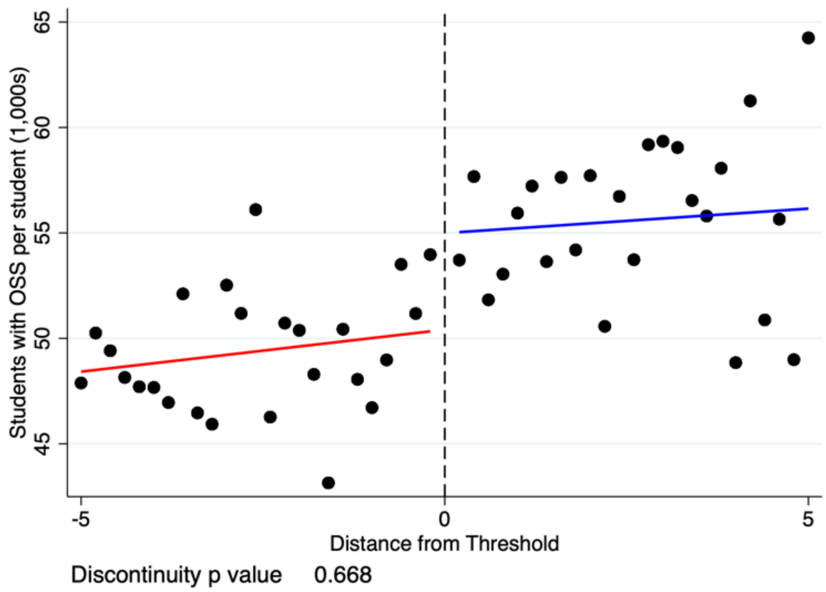
threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles. 842 observations.

Figure 3.13 Suspensions

Panel A. In-School Suspensions per 1,000 Students



Panel B. Out-of-School Suspensions per 1,000 Students

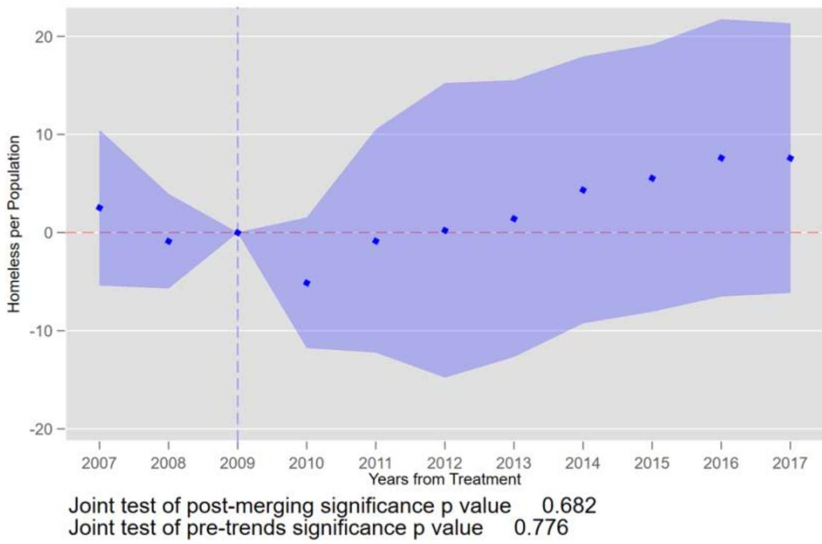


Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. The x-axis shows districts' percentiles away from the estimated

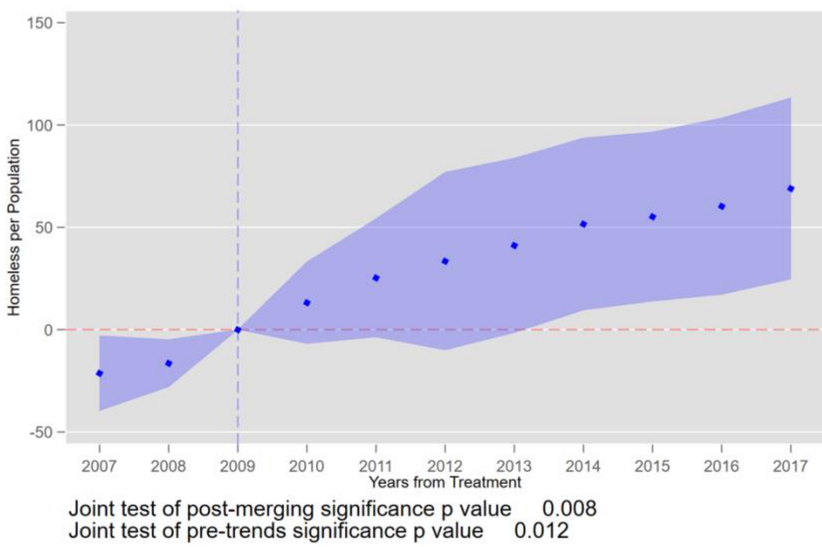
threshold. Markers show average outcomes for districts within the bandwidth, binned, at 0.20 percentiles. 844 observations.

Figure 3.14 Homeless Students per 10,000 Students

Panel A. Sheltered Students per 10,000 Students

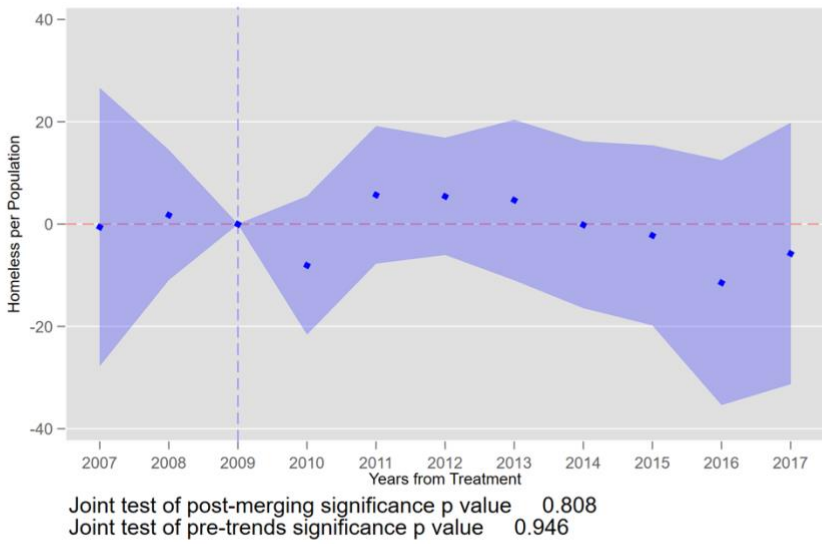


Panel B. Doubled-Up Students per 10,000 Students

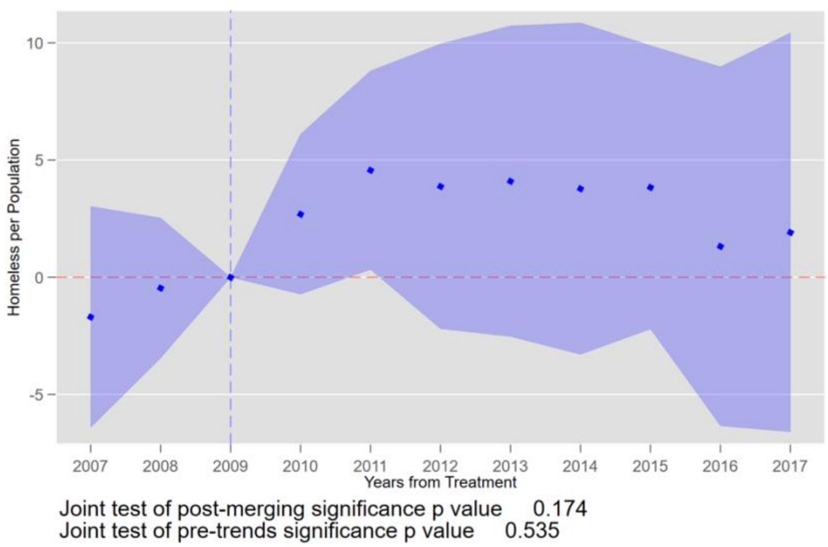


Notes: Data come from Section 1.9 of Consolidated State Performance Reports. Vertical line shows when the additional $70 million from the American Recovery and Reinvestment Act took place. Markers show coefficients from estimated models, with the shaded area showing the 95% confidence intervals. Model is event study framework, with an indicator for each school year with the omitted base year of 2009. Control variables include AFDC/TANF recipients per capita, state EITC rate, gross state product per capita, unemployment rate, the number of persons food insecure per capita, poverty rate, if the governor is Democrat, fraction of state House Democrat, fraction of state Senate Democrat, percent of population identifying as white and number of eviction filings. 528 observations.

Figure 3.15 Interactions with Increasing Percent of Districts Receiving Grants
Panel A. Sheltered Students per 10,000 Students



Panel B. Doubled-Up Students per 10,000 Students



Notes: Data come from Section 1.9 of Consolidated State Performance Reports. Vertical line shows when the additional $70 million from the American Recovery and Reinvestment Act took place. Markers show coefficients from estimated models, with the shaded area showing the 95% confidence intervals. Model is event study framework, with an indicator for each school year interacted with if the district increase the percent of districts receiving a grant by more than 10%, with the omitted base year of 2009. Control variables include AFDC/TANF recipients per capita, state EITC rate, gross state product per capita, unemployment rate, the number of persons food insecure per capita, poverty rate, if the governor is Democrat, fraction of state House Democrat, fraction of state Senate Democrat, percent of population identifying as white and number of eviction filings. 292 observations.

CHAPTER 4. *The Dynamics and Measurement of High School Homelessness and Achievement Disparities*

4.1 *Introduction*

Are students who experience homelessness less likely than their housed peers to graduate high school and attend college? How do estimates of these links change when using different commonly used ways to identify who is homeless? Using administrative student-level data over 12 years from a mid-sized public school district in the Southern United States, referred to as the *District*, we examine the dynamic patterns of student housing insecurity and estimate graduation and college going disparities between students who experience homelessness and those that do not. Our secondary school and transition to college focus is distinct from much of the homelessness-academic outcomes literature that largely concentrates on test scores of primary and middle school students. These studies generally find that homeless students tend to score lower on standardized tests than do housed students (Cowen, 2017; De Gregorio et al., 2020; Obradović et al., 2009; Rafferty et al., 2004). A separate set of studies investigates college students and generally finds that homeless college students face significant barriers related to affording college, meeting basic needs, and receiving housing services (e.g., Broton & Goldrick-Rab, 2018; Crutchfield, 2018; GAO, 2016; Skobba et al., 2018).

We first document the dynamic nature of homelessness among high school students. Homelessness is not a stable characteristic; rather, students can move in and out of experiencing it. Studies of other measures of material insufficiency recognize such dynamics as important; for example, researchers have attempted to understand patterns and consequences for children's being more likely to live in households that transition in and out of food insecurity rather than have persistent food insecurity across their whole lifetime (e.g., Hamersma & Kim, 2015; Rank & Hirschl, 2009). Understanding students' dynamic and diverse homelessness experiences can be important to create supports for housing insecure high school students. Such dynamics also matter because they contribute to differences in how states and researchers "count" homeless students and calculate achievement gaps between homeless and housed students. We show that common approaches to defining homelessness can yield widely different estimates of homelessness-housed high school graduation disparities. Such differences can impede across-state comparisons that contribute to targeted and efficient policymaking and have implications for funding since the federal government targets funds to districts that have the most homeless students and largest achievement gaps (Cunningham et al., 2010).

4.2 *Background and Context*

Students who experience homelessness can face educational challenges. Homeless students often double up—i.e., share housing with another household due to economic hardship or related reason—which can shape students' educational experiences and cause absences through issues like intra-household conflicts, child-rearing responsibilities, lack of study space, and competing demands (Hallett, 2012; Pavlakis, 2018). Homeless students are more likely to move residences and transfer schools, both of which can reduce scholastic engagement, hinder participation in extracurricular activities, or lead students to miss opportunities such as dual-enrollment classes and college counseling (GAO, 2016; Cowen, 2017). Further, homelessness is commonly accompanied by poverty and food insecurity which can negatively affect academics and limit students' ability to afford postsecondary expenses (e.g., GAO, 2016; Harvey, 2020; Heflin, Darolia, & Kukla-Acevedo, 2020; Michelmore & Dynarski, 2017; Pilkauskas et al., 2014; Miller, 2011).

Housing insecurity and homelessness are difficult to measure in part because it is complicated to disentangle the deleterious effects of homelessness from other factors related to poverty and material insufficiency. Moreover, housing security is best characterized as existing on a spectrum ranging from secure—where a student has access to fixed, regular, and suitable housing—to insecure, where housing is less stable, more variable, and less adequate; homelessness occurs at the severe insecurity end. This range presents difficulty in pinpointing students' places on a multifaceted scale, especially with incomplete information. For example, districts (including the data we use from the *District*) often capture only a dichotomous measure of homelessness and do not observe circumstances such as rent burden and overcrowding.

Identifying students experiencing homelessness has likely been exacerbated from remote learning induced by the COVID-19 pandemic. Districts often rely on surveys of students living situation at time of enrollment and on-the-ground identification by school personnel including bus drivers, teacher, and staff. Remote learning presents additional challenges in measuring students' housing insecurity as districts have even less information to go on, losing informal observations by personnel. In the case of the District, looking at the cumulative number of students identified by school week suggests potentially large under-identification, shown in Figure 4.1. In the 2020-2021 school year in which the district had been entirely remote learning due to the COVID-19 pandemic, the number of students identified as homeless has been well below the number in previous years. As of March, the number of students identified is about half of the number as the 2019-2020 school year and a third of the 2018-2019 school year's number. While the number of students identified has been catching up to the average number across the other three years, it has only increased from 66% lower in September 2020 to 53% lower in March 2021. In theory, there could be fewer students experiencing homelessness in the district. However, this seems unlikely given the large economic downturn, increase in unemployment, and lack of a significant change in the District's enrollment. The difficulty in measuring homelessness is thus a likely cause of the perceived decrease in homelessness.

We focus on the temporal aspect of homelessness in this paper, which further impedes districts' ability to consistently measure homelessness (Aviles de Bradley, 2011; Hallett, 2012). Students can cross into and out of what is considered homelessness repeatedly, which is one reason scholars and practitioners characterize homelessness as an experience rather than a permanent condition (O'Flaherty, 2019). Students experiencing homelessness commonly transition back to being housed, although the barriers faced during homelessness—e.g., lack of resources and instability—often persist. The US Department of Education (ED) recognizes this phenomenon, requiring districts to continue providing services for the entire school year even if a homeless student becomes housed (NCHE, 2020).

This dynamism contributes to a lack of clear consensus on how to measure homelessness in high school. Consider three different definitions of homelessness based on common state practices (Low et al., 2017; NCHE, 2020) illustrated in Table 4.1. Students in categories A, B, C, and D completed all four years of high school, whereas students in categories E, F, and G dropped out before 12th grade. First, consider the *Ever Homeless* definition, which includes students who districts identify as homeless at any point in high school. In the table, this means that the graduation of students in categories A, B, C, E, and F are compared against students considered housed in categories D and G. Next, consider the *District*'s definition, *Last Status*, which is based on the final observed status of students, including those who dropped out. In this definition, graduation of students in categories A, B, and E is compared against students considered housed in categories C, D, F, and G. In other words, students who were housed in 12th grade, but homeless in a prior grade (category C) are considered homeless in the *Ever Homeless* definition but considered housed in the *Last Status* definition. Similarly, students who dropped out before 12th grade, whose last status was housed, but were homeless at some point earlier in high school (category F), are considered homeless in the *Ever Homeless* definition but considered housed in the *Last Status* definition. Finally, consider the *12th Grade Status* definition – in this scenario, students who drop out before 12th grade are not included in the sample (categories E, F, and G). Relative to *Last Status* and *12th Grade Status*, *Ever Homeless* is the most inclusive in which students count as homeless.

4.3 *Dynamics of High School Homelessness*

Our analysis sample includes all roughly 21,300 students who entered 9th grade in the *District* from the 2007-08 to 2013-14 academic years and follows students for six years. About 2.1% of students in our sample are identified as being homeless at some point in their high school careers, which is close to national estimates of 2.3% of high school students experiencing homelessness in a given year (NCES, 2017). After 12th grade, we observe whether students graduated or enrolled in a postsecondary institution based on a National Student Clearinghouse match.

In Figure 4.2, we display the dynamics of high school homelessness among the 2.1% of students in our data experiencing homelessness at some point during high school. Starting at the far left of the graph is students' 9th grade status: by construction, every student is either homeless (46%) or housed (54%) to start the year. From the start of 9th to the start of 10th, 11th, and 12th grades (moving from left to right on the graph), students can belong in one of four mutually exclusive categories: continued to the next grade and is housed, continued to the next grade and is homeless, dropped out, or transferred to another district. For these latter two categories, conceivably a student could return to school or transfer back in, but we never observe these actions in our data. For students that repeat grades (33% of ever homeless students), we use the last observed housing status.

Roughly half of the students experiencing homelessness each year become housed the following year. Homeless students who do not become housed the next grade have about an equal likelihood of still experiencing homelessness the next year, dropping out of school, or transferring to another district. Among students who experience homelessness in high school and stay in school for four years, only <1% of students are homeless all four years in high school, 3% are homeless 3 years, 16% are homeless 2 years, and 81% are homeless 1 year. Among those with two years of observed homelessness in high school, 89% experience in consecutive years, while 11% have a break of at least a year between recorded homelessness. Homeless students drop out or transfer at a higher rate than housed students. About 38% of the students that experience homelessness at some point in grades 9-11 drop out or transfer before 12th grade, as compared to about 17% of always housed students.

These observed dynamics of homelessness demonstrate the fluctuation in housing circumstances students experience as they transition in and out of observed homelessness over time in high school. Resultingly, how districts measure and consider previous experiences of homelessness can change which students count as homeless and the supports for which students qualify. For example, under the McKinney Vento Act, the federal government requires districts to provide homeless students resources such as transportation, expedited enrollment, tutoring, assistance with participating in school programs, and other academic supports and social services (Cunningham, et al., 2010).

4.4 *Homelessness, High School Graduation, and College Going*

We next consider how using the different ways to measure homelessness result in different estimates of the links between homelessness and high school graduation or college going within six years of starting high school. We separately estimate these outcomes, *Y*, for each student *i* as a linear function of homelessness, *H*:

$$Y_i = \alpha + \gamma H_i + \eta X_i + \varepsilon_i$$

Here, we use the three definitions described in Table 1 and estimate separate regressions for each definition. In some specifications, we control for observed student 9th grade characteristics in the *X*-vector: sex (male/female/other), race/ethnicity (Black/Asian/Hispanic/American Indian/Native Hawaiian/White/Multiple/Other), school attended, school year first enrolled in 9th grade, and zip code of the students' residence; we also include indicators for whether in high school the student ever qualified for free/reduced-price lunch, had an individual education plan, was identified as an English language learner, or was identified as gifted/talented. Our results should not be interpreted as estimates the effect of homelessness on outcomes; rather, they are useful to illustrate how homelessness definition differences affect estimates of homelessness-housed achievement disparities, while conditioning on factors that districts can reasonably collect. We exclude students who transfer out of the district from our analysis in this section and consider students who drop out as not graduating. Using a logit yields similar results that are available upon request.

We display estimates of the unconditional relationship between homelessness in high school and graduation in the topmost row of Figure 4.3, with bars showing 95% confidence intervals. The magnitude of the homelessness-housed graduation rate gap differs markedly depending on how homelessness is defined. Students considered homeless under the *Last Status* definition (triangle marker) have graduation rates that are 32 percentage points lower than their housed peers; *Ever Homeless* students (circle marker) have graduation rates that are 17 percentage points lower, and *12th Grade Status* (square marker) students have graduation rates 4 percentage points lower (this last estimate is not statistically different than zero). These results mean that homeless student graduation rates are about 61%, 80%, and 96% of the housed student graduation rates for the *Last Status*, *Ever Homeless,* and *12th Grade Status* definitions, respectively. Complicating the interpretation of the magnitude across scenarios is that the composition, and thus graduation rate, of the comparison group differs under each definition (recall Table 1). Graduation estimates conditional on observed covariates are in the second row of the figure. Students experiencing homelessness in high school still have lower graduation rates than housed students, although the conditional gaps narrow, ranging from 2-26 percentage points.

In the bottom half of Figure 4.3, we present results from estimates of enrolling in college within six years after entering high school. In these estimates, we only include students who graduated high school. Estimated parameters are similar across scenarios. In the unconditional estimates in the third row, students who experience homelessness in high school enroll in college at a rate of about 20-24 percentage points lower than housed students. In estimates accounting for student characteristics (bottom row), the gap again narrows; students experiencing homelessness in high school enroll in college at a rate 5-9 percentage points lower than housed peers. In the *Last Status* and *12th Grade Status* scenarios, the 95% confidence interval includes zero.

Figure 4.4 presents results for disparities in college enrollment by 2-year and 4-year colleges. Similar to college enrollment overall, there are only minor differences across definitions when splitting college enrollment by 2-year or 4-year college. Additionally, Only a small homelessness-housed gap exists in 2-year college enrollment rates even when not conditioning on students' observable characteristics. On the other hand, the bottom panel suggests high school homelessness relates with a lower likelihood of enrolling in a 4-year college, averaging about 19 percentage points lower than housed students. Housing security may not be the cause of the lower rate, however, as including controls narrows the gap to close to zero. These results suggest that, although students experiencing homelessness in high school are less likely to enroll in a 4-year college, the supports needed to close the gap may not be unique to housing insecure students.

4.5 *Conclusion*

Homeless students are less likely to graduate high school than consistently housing secure students. Yet, estimates of the magnitude of the disparity differ greatly depending on various commonly used definitions of which students “count” as homeless: our estimates range from a 4-32 percentage points in unconditional comparisons and 2-26 percentage points when taking into account student characteristics that districts commonly record. The use of multiple definitions of homelessness complicates comparisons of homelessness-housed educational gaps across states and districts, impeding a full understanding of the homelessness problem across states and hindering research and practice that can help identify solutions and policies to support housing insecure students.

One way to calculate graduation disparities is to compare homeless students in 12th grade to housed students in 12th grade. This approach likely understates the severity of homelessness in districts because it does not consider students who drop out prior to 12th grade and homeless students are more likely to drop out than housed students. In the *District*, this approach misses about 75% of students who experienced homelessness and results in the smallest graduation gap.

Considering two other common, but more comprehensive ways to define homelessness illustrates a tradeoff between targeting students most at risk for not graduating from high school and being inclusive. The key distinction between these two definitions relates to how to consider students who were homeless but become housed: these students are considered homeless in an *Ever Homeless* approach but housed when recognizing *Last Status*. For this reason, *Ever Homeless* counts the most students as homeless. This can be important because homeless students can continue to face other forms of material insufficiency and stressors after they become housed, and not being homeless is not equivalent to being housing secure. The ED requires districts to continue providing services (e.g., transportation, academic assistance) to rehoused homeless students for the remainder of a school year in recognition of these challenges, but these supports do not persist in subsequent years.

Yet, our findings also suggest that while homeless students who transition to housed are likely to face greater challenges than always housed peers, these homeless-to-housed students are potentially better poised to graduate than peers whose last observed status is homeless. These findings echo those of Cassidy (2020), who finds that homeless students' academic achievement can rebound after becoming rehoused. They likewise are consistent with results from De Gregorio et al. (2020) in finding students to have worse educational outcomes in the year of homelessness as opposed to in years after becoming rehoused. In this way, the *Last Status* definition may be best suited to identify those at most risk of severe negative academic outcomes, even though it is more restrictive than an approach that counts students that ever experience homelessness.

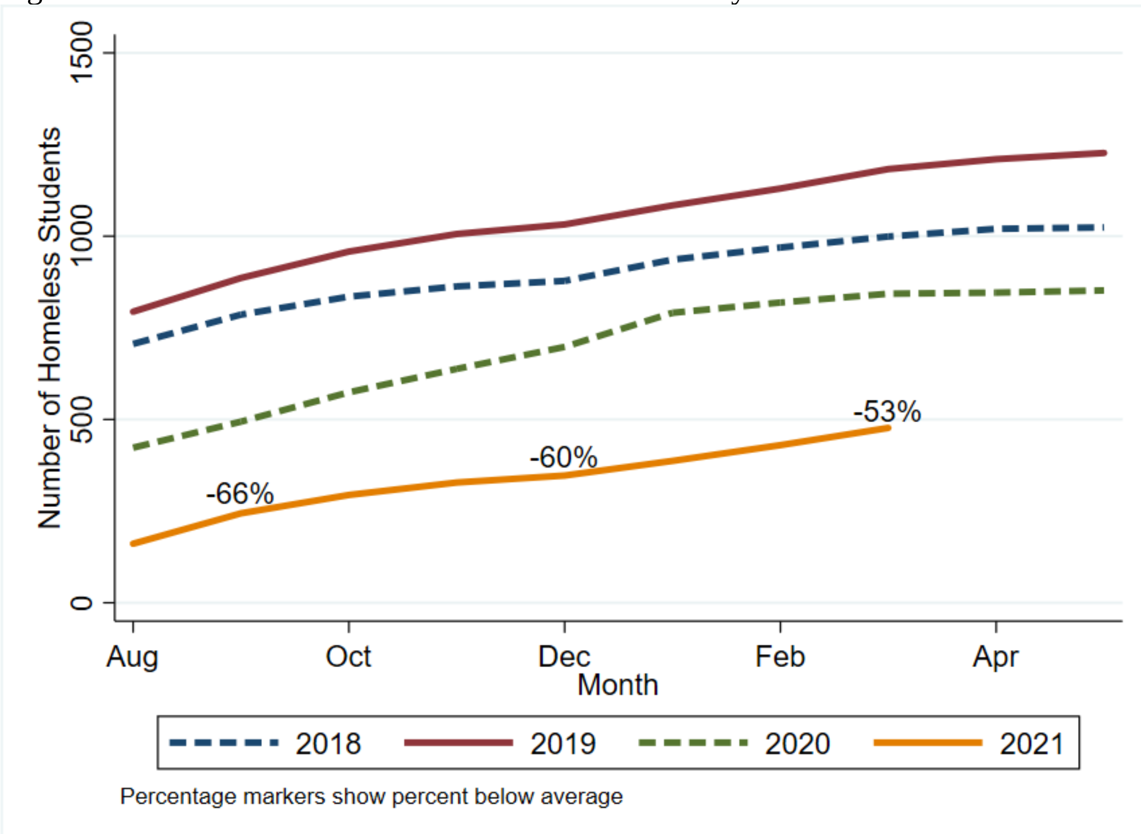
4.6 *Tables and Figures*

Table 4.1 Student Housing Status and Measuring Homelessness

Group	12th Grade Status	Ever Homeless pre-12th	Last Observed pre-12th Status	***Ever Homeless***	***Last Status***	***12th Grade Status***
A	Homeless	Yes	n/a	Homeless	Homeless	Homeless
B	Homeless	No	n/a	Homeless	Homeless	Homeless
C	Housed	Yes	n/a	Homeless	Housed	Housed
D	Housed	No	n/a	Housed	Housed	Housed
E	Not Enrolled	Yes	Homeless	Homeless	Homeless	Not in sample
F	Not Enrolled	Yes	Housed	Homeless	Housed	Not in sample
G	Not Enrolled	No	Housed	Housed	Housed	Not in sample
Homeless Student Graduation Rate				66%	51%	93%
Housed Student Graduation Rate				83%	83%	97%
Homeless Student College Going Rate				43%	47%	45%
Housed Student College Going Rate				67%	67%	67%

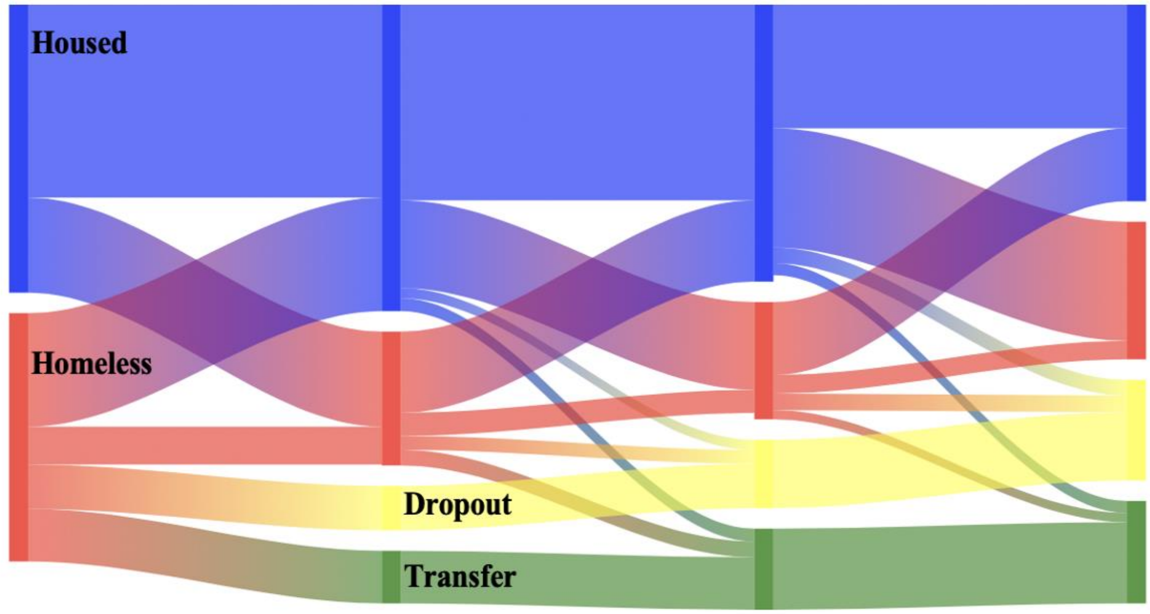
Note: We shade grey categories of students that are identified differently across definitions.

Figure 4.1 Number of Students Identified as Homeless by School Week



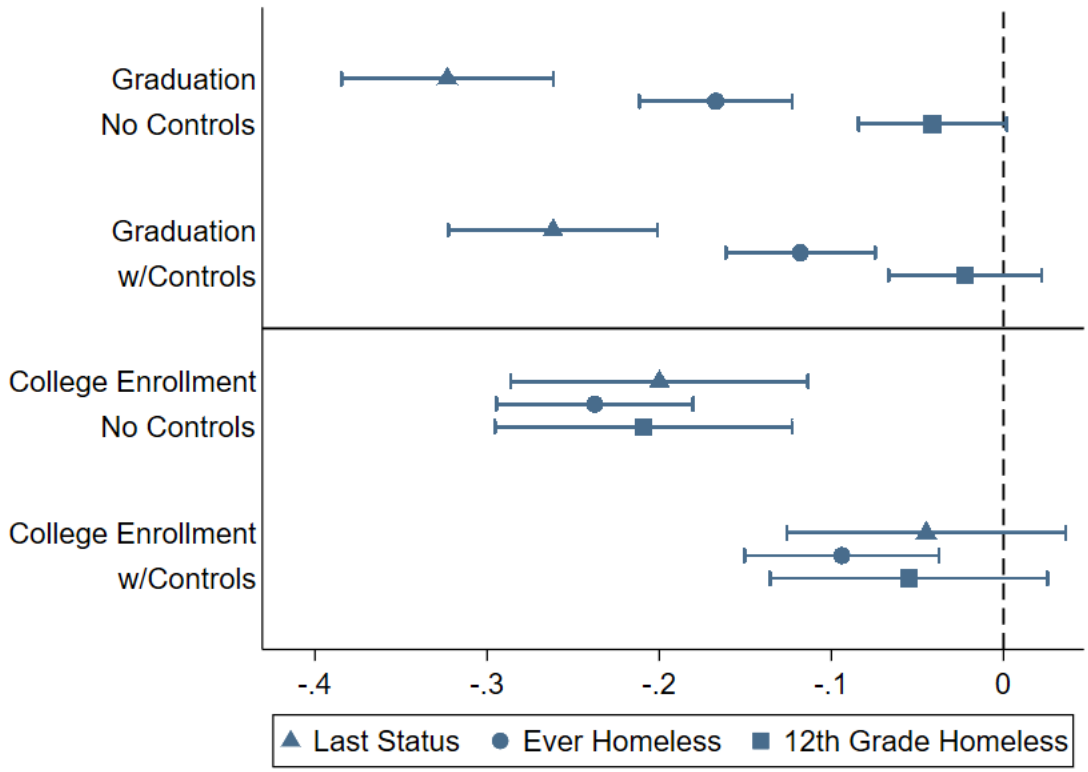
Notes: Figure shows the cumulative number of students identified as homeless at that point in the school year by school year. Percentages shown within the figure is how much lower the number of students identified as homeless in 2020-2021 is relative to the average number by that month across the other three years.

Figure 4.2 Dynamics of High School Homelessness among Students Homeless in High School



Notes: Figure shows the dynamics of homelessness for students observed in 9th grade in the *District* and experience homelessness at some point in grades 9-12. Every student in 9th grade is either homeless or housed. Size of bars is weighted by the proportion of students fitting the categories. Moving from left to right shows the share of students going into other categories between the two nodes. Blue nodes are students that are housed that grade; red nodes are students that are homeless that grade. Yellow and green nodes are for students that drop out or transfer, respectively, at some point between the grade before and that grade.

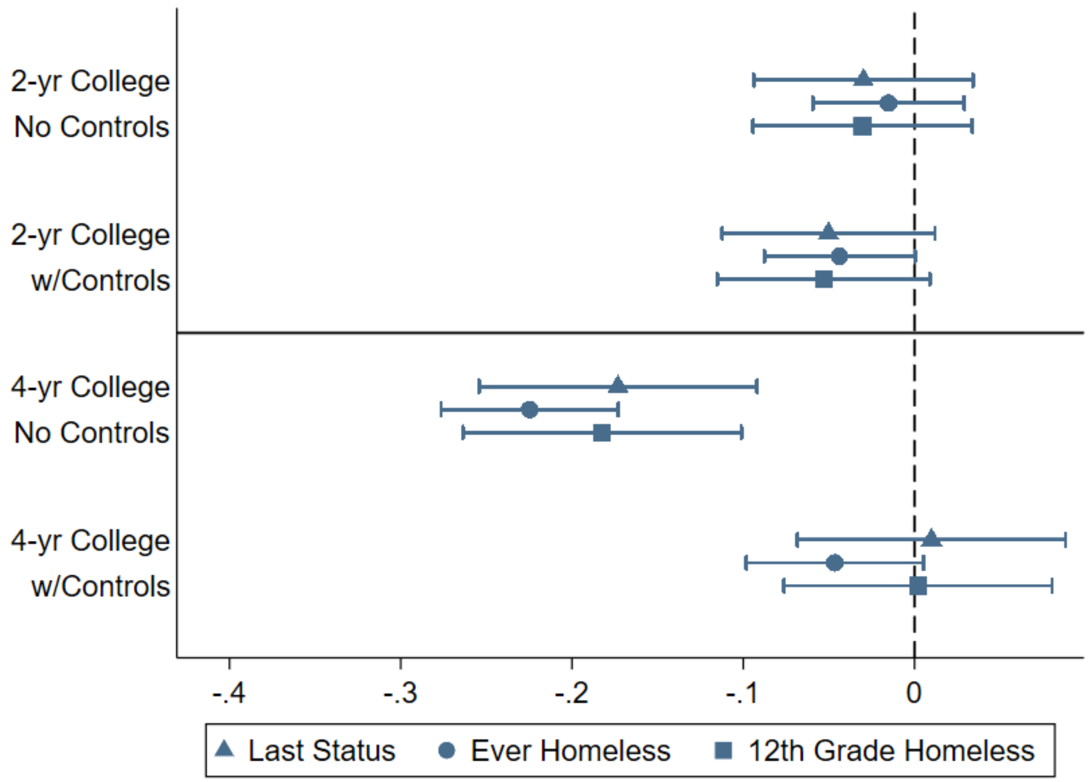
Figure 4.3 Estimates of Homelessness-Housed Gaps in High School Graduation and College Enrollment



Notes: Graph shows relationships between high school homelessness and high school graduation/college enrollment. Each line shows the relationship from a different estimation. Markers show the relationship with experiencing homelessness in that grade relative to students observed that grade not experiencing homelessness. Bars show 95% confidence intervals for robust standard errors for each respective marker. The outcome for the top panel is graduating from high school and for the bottom panel is enrolling in college within two years of leaving high school, taking a value of 1 if enrollment is observed and 0 otherwise. Controls include observed student 9th grade characteristics: sex (male/female/other), race/ethnicity (Black/Asian/Hispanic/American Indian/Native Hawaiian/White/Multiple/Other), school attended, school year the student entered 9^{th} grade, and zip code of the students' listed residence. We also create four variables for whether the student in high school ever qualified for free or reduced-price lunch, had an individual education plan, identified as an English language learner, and identified as gifted and talented. Observations are student level for students observed in 9^{th} grade and did not transfer to another school district during high school. Estimations for college enrollment further limit the sample to students observed graduating from high school. The number of observations are as follows: Graduation, Ever Homeless/Last Status – 21,319; Graduation, 12^{th} Grade Homelessness – 17,750; College enrollment, Ever Homeless/Last Status –

17,590; College enrollment, 12 Grade Homelessness – 17,200. The number of observations are the same for both estimations with and without controls.

Figure 4.4 Estimates of Homelessness-Housed Gaps in College Enrollment by College Type



Notes: Graph shows relationships between high school homelessness and college enrollment. Each line shows the relationship from a different estimation. Markers show the relationship with experiencing homelessness in that grade relative to students observed that grade not experiencing homelessness. Bars show 95% confidence intervals for robust standard errors for each respective marker. Controls include observed student 9th grade characteristics: sex (male/female/other), race/ethnicity (Black/Asian/Hispanic/American Indian/Native Hawaiian/White/Multiple/Other), school attended, school year the student entered 9th grade, and zip code of the students' listed residence. We also create four variables for whether the student in high school ever qualified for free or reduced-price lunch, had an individual education plan, identified as an English language learner, and identified as gifted and talented. Observations are student level for students observed in 9th grade and did not transfer to another school district during high school. Estimations for college enrollment further limit the sample to students observed graduating from high school. The number of observations are 17,590 for Last Status and Ever Homeless estimations and 17,200 for 12th Grade Homeless definitions.

APPENDICES

APPENDIX 1. Weighting Control Trends

First, I create annual averages of the rate of homelessness for control CoCs, where $Outcome_{c,t}$ is the homelessness outcome for CoC *c* in year *t*, if the CoC never merged.

$$Control\_Outcome_t = \frac{1}{N}\sum_{c=1}^{N} Outcome_{c,t}$$

Second, I create a frequency weight for each year that is the number of mergers for that year, divided by the total number of mergers for the balanced panel (eighteen mergers). For example, five mergers occurred in 2010, so 2010 has a weight of 27.8%.

$$Year\_Weight_t = \frac{\sum_{c=1}^{N} Treatment_{c,t}}{\sum_{t=1}^{T}\sum_{c=1}^{N} Treatment_{c,t}}$$

Third, for each year from treatment, *j*, I create a weight from years from treatment that is the year weight depending on years from treatment.

$$Treatment\_Weight_j = \ Year\_Weight_t * \mathbf{1}[t - \tau = j])$$

Fourth, I then multiply each weight for years from treatment by the control's average outcomes for each year.

$$W\_Control\_Outcome_{t,j} = \ Treatment\_Weight_{t,j} * Control\_Outcome_{\boldsymbol{t}}$$

Lastly, I aggregate weighted control outcomes by year to create a weighted control outcome for years from treatment.

$$W\_Total\_Control\_Outcome_j = \sum_{t=1}^{T} W\_Control\_Outcome_{j,t}$$

APPENDIX 2. Appendix for Chapter 2

	Total Beds	PSH	HHI	Award	Service Providers	Award per Provider
Post-Merger	0.144	-0.357	0.033	-905	-0.002	-1.374
Standard Error	(0.728)	(0.328)	(0.028)	(1,236)	(0.007)	(5.275)
Observations	3,794	3,794	3,772	3,758	3,758	3,751
Number of CoCs	353	353	352	353	353	353
Pre-Treatment Mean	19.48	6.46	0.175	42,637	0.188	217.6
Lower Bound Pct of Mean	-6.62%	-15.50%	-12.00%	-7.82%	-9.04%	-4.94%
Point Estimate Pct of Mean	0.74%	-5.53%	18.86%	-2.12%	-1.06%	-0.63%
Upper Bound Pct of Mean	8.09%	4.46%	50.29%	3.58%	6.38%	4.60%

Notes: Standard errors clustered at the CoC level in parentheses. Control variables per capita income, unemployment rate, new low-income housing tax credit units, the share of the population black, Asian, and Hispanic, population density, poverty rate, 0-bedroom fair market rent, if the governor is a Democrat, TANF 2-person benefit, state labor force per capita, and CoC, and year fixed effects are included in all models. Model is two-way fixed effects with "post-merger" a variable taking the value of one for a CoC after it merges and zero otherwise. Lower/Upper Bound Pct of Mean are lower and upper bounds of the 95% confidence interval in units of percent of the pre-treatment mean. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample. *** p<0.001, ** p<0.01, * p<0.05

Table B1. Estimates of Operational Measures – Rest of State

	Total Beds	PSH	HHI	Award	Service Providers	Award per Provider
Post-Merger	-0.153	-0.259	-0.002	-1,002	-0.006	4.430
Standard Error	(0.384)	(0.300)	(0.017)	(1,084)	(0.008)	(5.529)
Observations	3,839	3,839	3,822	3,803	3,803	3,803
Number of CoCs	357	357	357	357	357	357
Pre-Treatment Mean	14.12	5.073	0.258	33,738	0.147	211.2
Lower Bound Pct of Mean	-6.43%	-16.74%	-13.57%	-9.29%	-14.29%	-3.05%
Point Estimate Pct of Mean	-1.08%	-5.11%	-0.78%	-2.97%	-4.08%	2.10%
Upper Bound Pct of Mean	4.26%	6.52%	11.63%	3.35%	6.12%	7.25%

Notes: Standard errors clustered at the CoC level in parentheses. Control variables per capita income, unemployment rate, new low-income housing tax credit units, the share of the population black, Asian, and Hispanic, population density, poverty rate, 0-bedroom fair market rent, if the governor is a Democrat, TANF 2-person benefit, state labor force per capita, and CoC, and year fixed effects are included in all models. Model is two-way fixed effects with "post-merger" a variable taking the value of one for a CoC after it merges and zero otherwise. Lower/Upper Bound Pct of Mean are lower and upper bounds of the 95% confidence interval in units of percent of the pre-treatment mean. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. *** p<0.001, ** p<0.01, * p<0.05

Table B2. Estimates of Operational Measures – Neighbors

	Total Homeless	Unsheltered	Sheltered	Chronic	Non-Chronic
Post-Merger	0.735	0.480	0.255	0.059	0.676
Standard Error	(1.045)	(0.392)	(0.844)	(0.226)	(0.938)
Observations	3,796	3,796	3,796	3,796	3,796
Number of CoCs	353	353	353	353	353
Pre Treated Mean	19.50	6.695	12.80	3.604	15.89
Lower Bound Pct of Mean	-6.77%	-4.33%	-10.98%	-10.68%	-7.35%
Point Estimate Pct of Mean	3.77%	7.17%	1.99%	1.64%	4.25%
Upper Bound Pct of Mean	14.31%	18.69%	14.96%	13.96%	15.86%

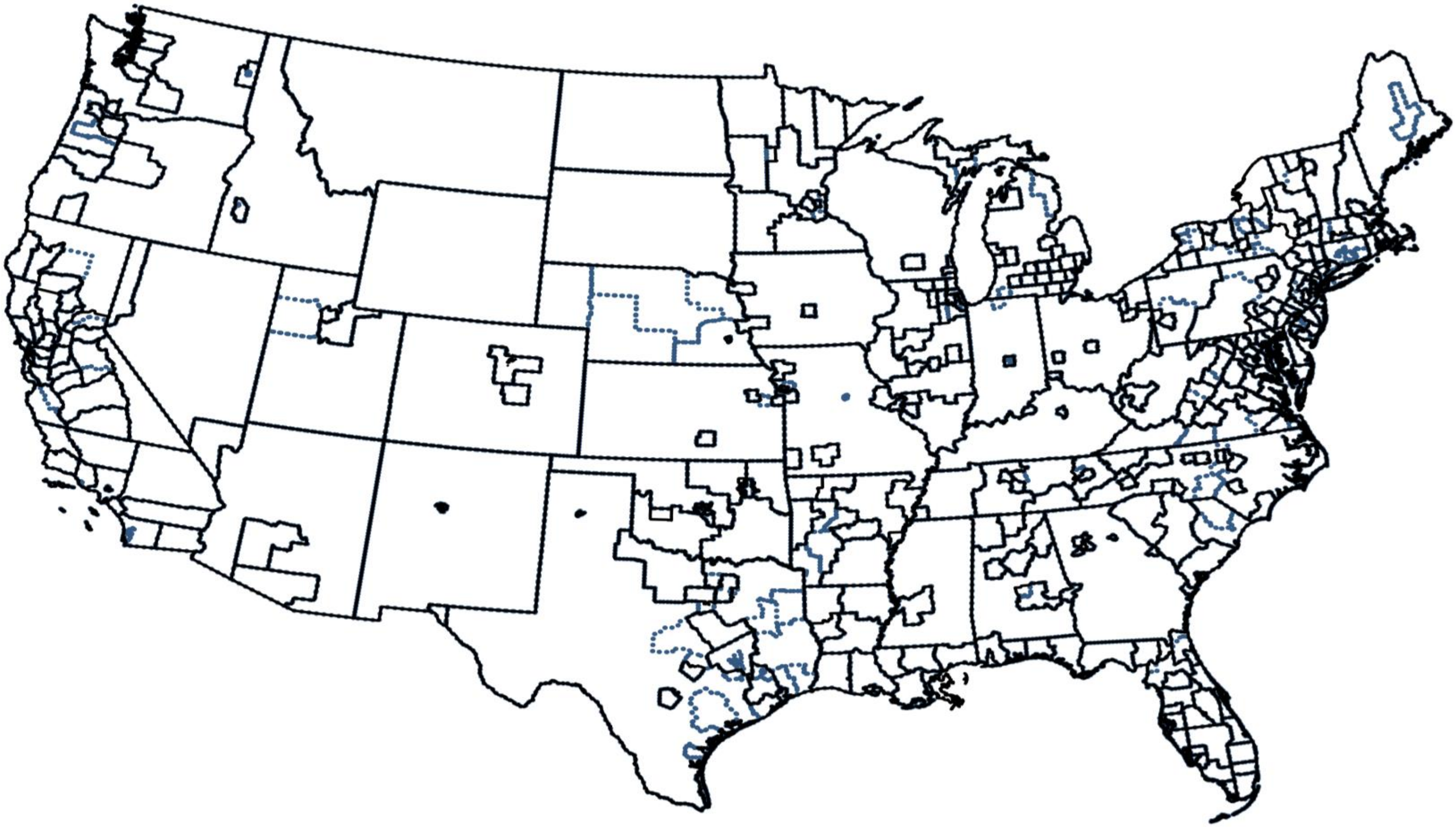
Notes: Standard errors clustered at the CoC level in parentheses. Control variables per capita income, unemployment rate, new low-income housing tax credit units, the share of the population black, Asian, and Hispanic, population density, poverty rate, 0-bedroom fair market rent, if the governor is a Democrat, TANF 2-person benefit, state labor force per capita, and CoC, and year fixed effects are included in all models. Model is two-way fixed effects with "post-merger" a variable taking the value of one for a CoC after it merges and zero otherwise. Lower/Upper Bound Pct of Mean are lower and upper bounds of the 95% confidence interval in units of percent of the pre-treatment mean. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

Table B3. Estimates of Homelessness Measures – Rest of State

	Total Homeless	Unsheltered	Sheltered	Chronic	Non-Chronic
Post-Merger	1.117	1.140	-0.023	0.258	0.859
Standard Error	(0.606)	(0.589)	(0.343)	(0.237)	(0.481)
Observations	3,841	3,841	3,841	3,841	3,841
Number of CoCs	357	357	357	357	357
Pre Treated Mean	12.85	4.654	8.196	2.233	10.62
Lower Bound Pct of Mean	-0.58%	-0.41%	-8.50%	-9.27%	-0.82%
Point Estimate Pct of Mean	8.69%	24.50%	-0.28%	11.55%	8.09%
Upper Bound Pct of Mean	17.96%	49.38%	7.96%	32.42%	17.00%

Notes: Standard errors clustered at the CoC level in parentheses. Control variables per capita income, unemployment rate, new low-income housing tax credit units, the share of the population black, Asian, and Hispanic, population density, poverty rate, 0-bedroom fair market rent, if the governor is a Democrat, TANF 2-person benefit, state labor force per capita, and CoC, and year fixed effects are included in all models. Model is two-way fixed effects with "post-merger" a variable taking the value of one for a CoC after it merges and zero otherwise. Lower/Upper Bound Pct of Mean are lower and upper bounds of the 95% confidence interval in units of percent of the pre-treatment mean. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

Table B4. Estimates of Homelessness Measures – Neighbors

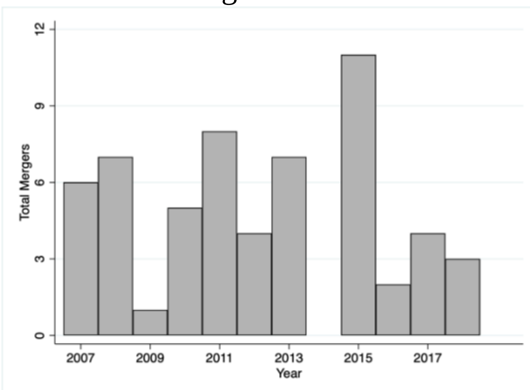


Notes: Dark, solid lines are 2016 CoC boundaries. Dotted, blue lines are historical boundaries prior to merging.

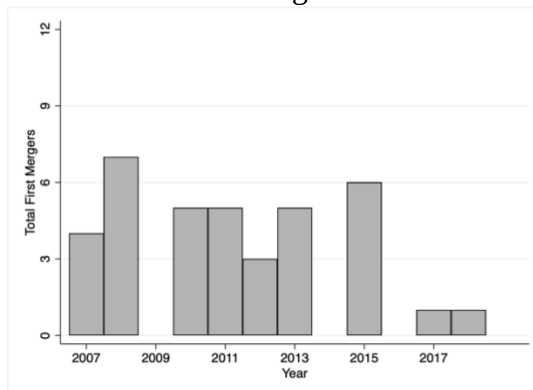
Figure B1. Historical Continuum of Care Boundaries

Figure B2. CoC Mergers

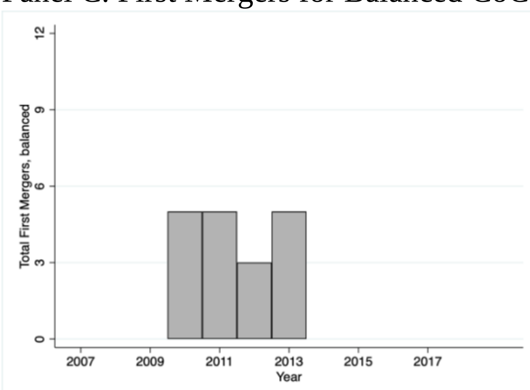
Panel A. All Mergers



Panel B. All First Mergers



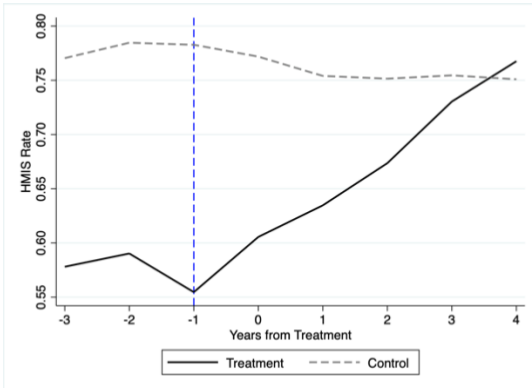
Panel C. First Mergers for Balanced CoCs



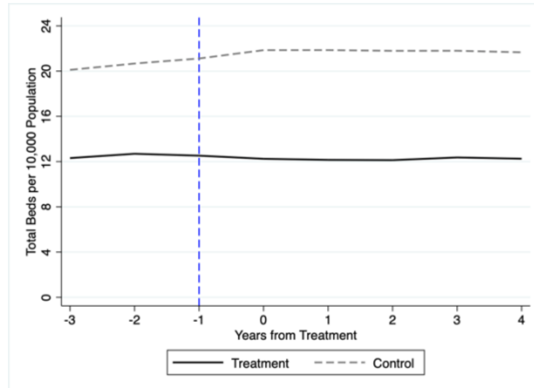
Notes: Panel A shows the total number of mergers by year. Panel B is limited to first mergers. Panel C is further limited to only first mergers for CoC in the balanced panel, which only uses 2010-2013 as treatment years.

Figure B3. Trends between Merged and Control CoCs

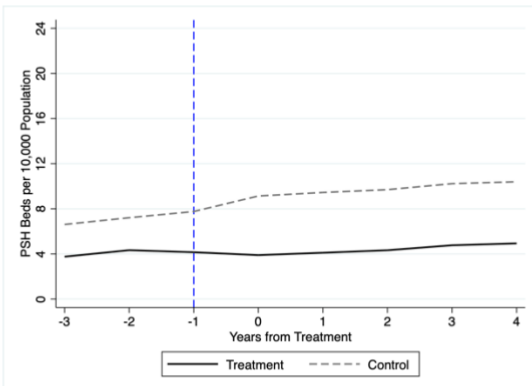
Panel A. HMIS PSH Rate



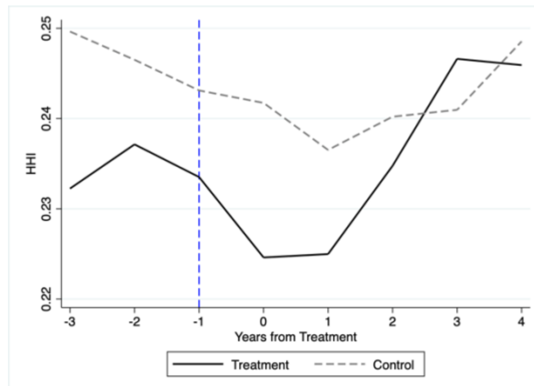
Panel B. Total Beds



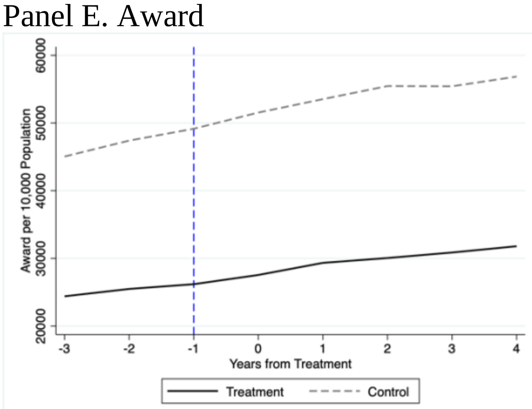
Panel C. PSH Beds



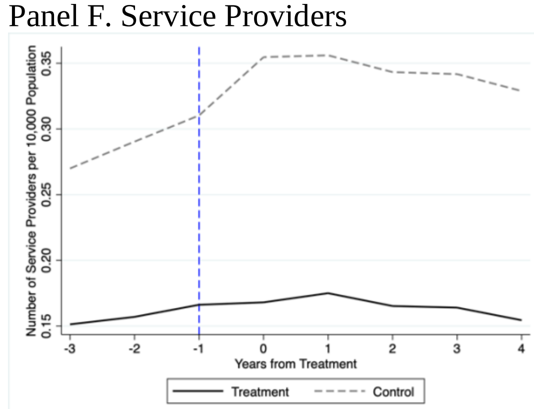
Panel D. HHI



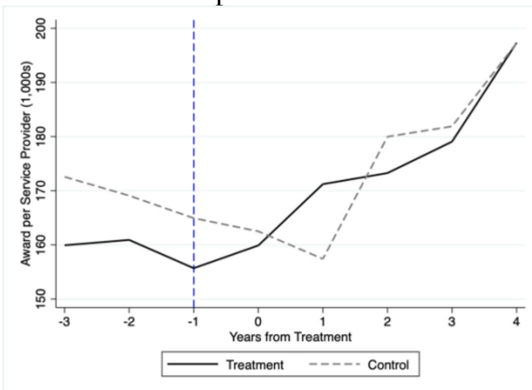
Panel E. Award



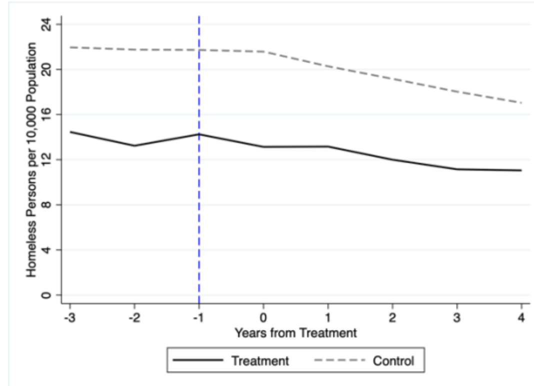
Panel F. Service Providers



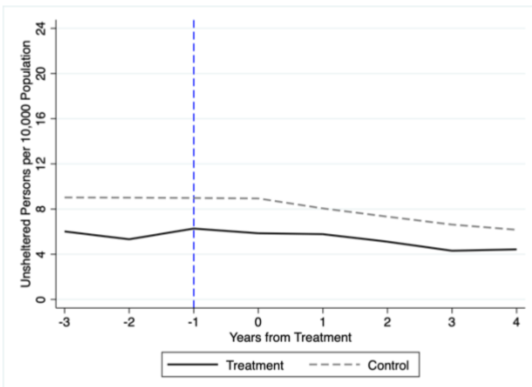
Panel G. Award per Service Providers



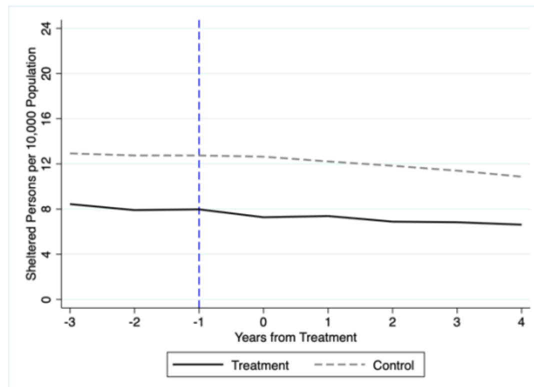
Panel H. Total Homeless



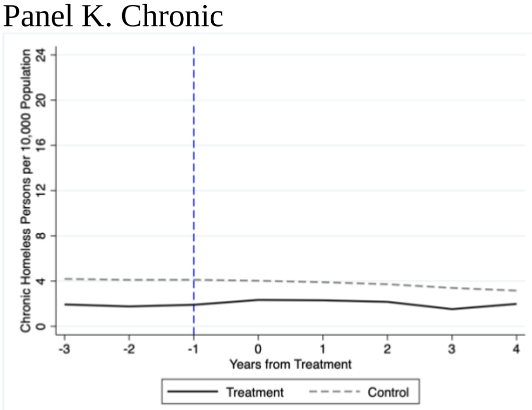
Panel I. Unsheltered



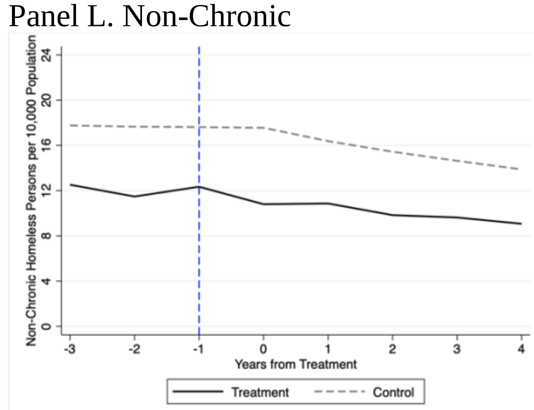
Panel J. Sheltered



Panel K. Chronic



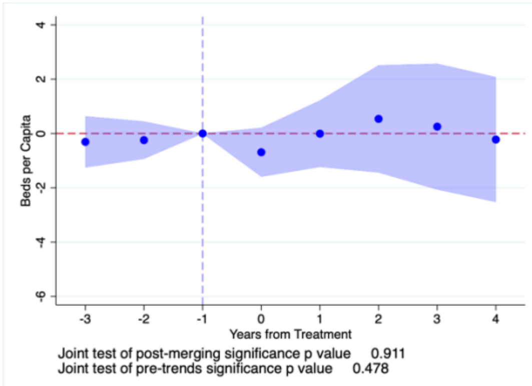
Panel L. Non-Chronic



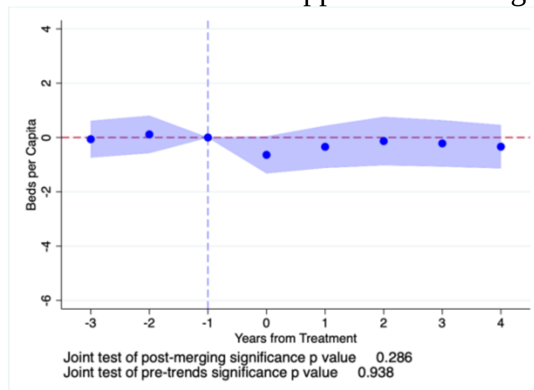
Notes: All panels compare balanced, CoCs merging the first time between 2010-2013, to CoCs that never merged between 2007-2017. Treatment lines are average levels for treated CoCs. Control lines are frequency of treatment weighted averages (See Appendix A for detail).

Figure B4. *Rest of State* Time-Varying Generalized Difference-in-Difference – Operations Measures

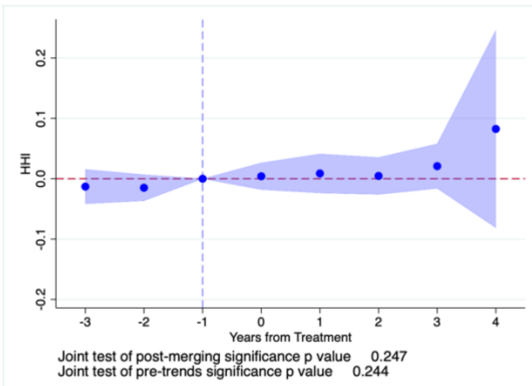
Panel A. Total Beds



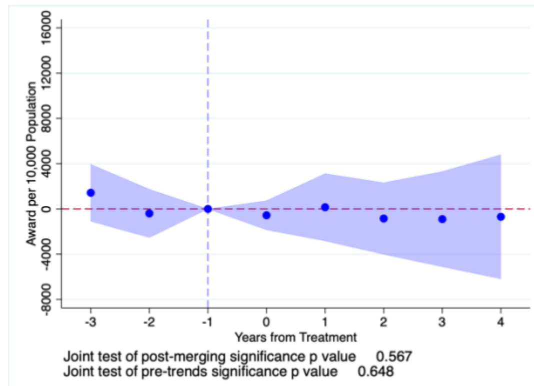
Panel B. Permanent Supportive Housing



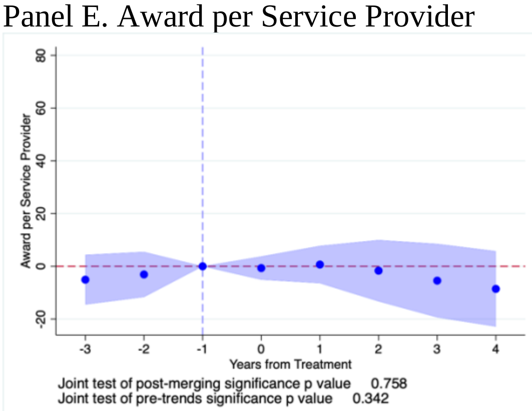
Panel C. HHI



Panel D. Award



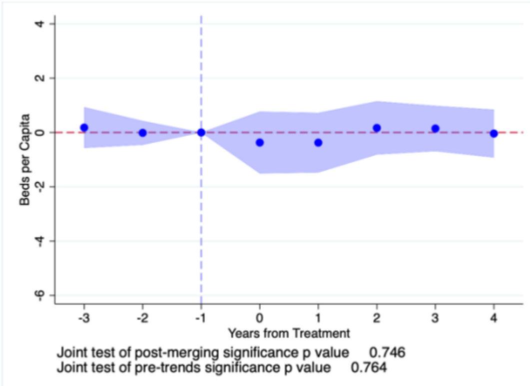
Panel E. Award per Service Provider



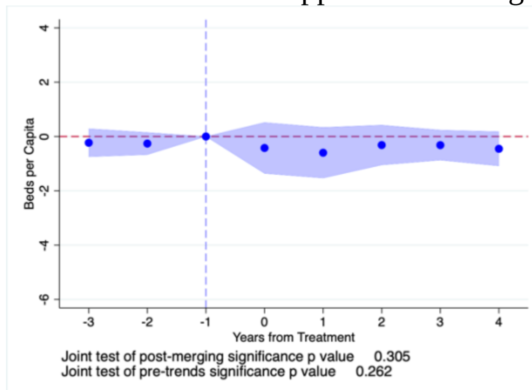
Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables, CoC, and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. Outcomes are for the rest of the state of a merged CoC.

Figure B5. *Neighboring CoCs* Time-Varying Generalized Difference-in-Difference – Operations Measures

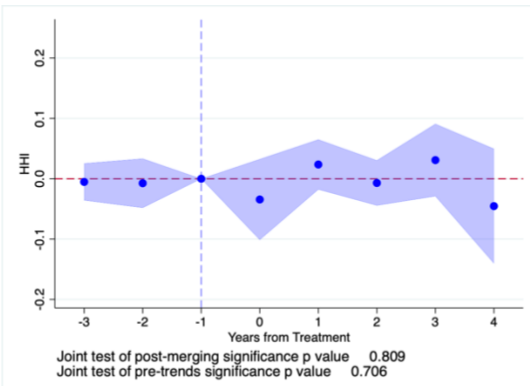
Panel A. Total Beds



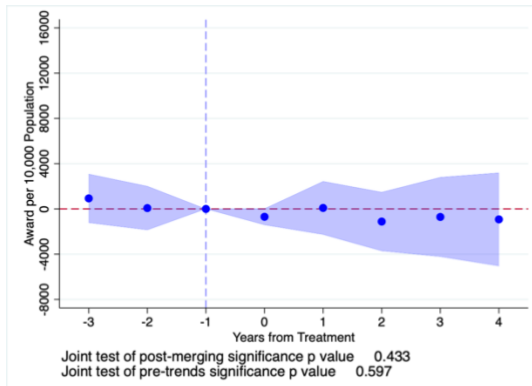
Panel B. Permanent Supportive Housing



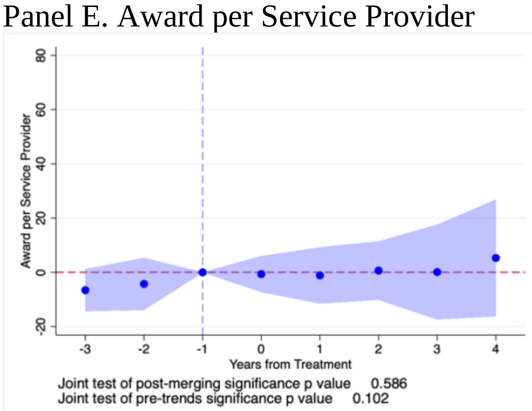
Panel C. HHI



Panel D. Award



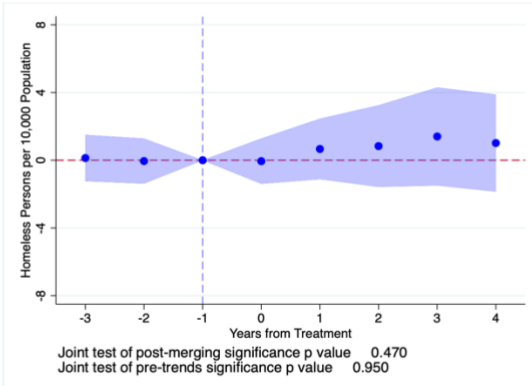
Panel E. Award per Service Provider



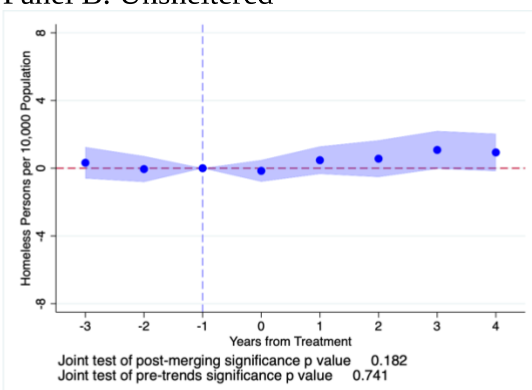
Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables, CoC, and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. Outcomes are for neighboring CoCs of a merged CoC.

Figure B6. *Rest of State* Time-Varying Generalized Difference-in-Difference – Homelessness Measures

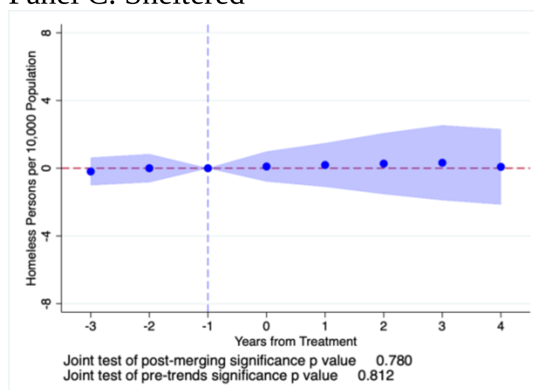
Panel A. Total Homeless



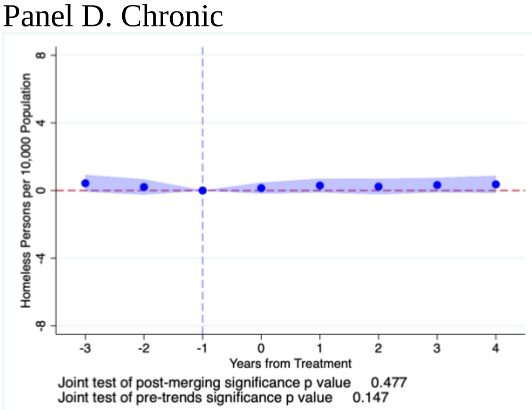
Panel B. Unsheltered



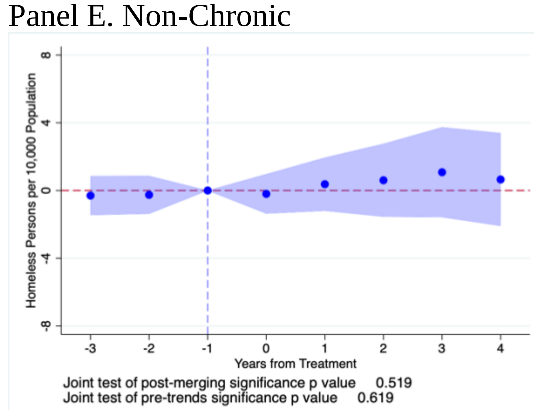
Panel C. Sheltered



Panel D. Chronic



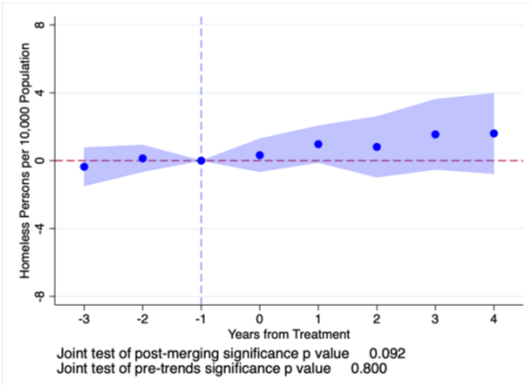
Panel E. Non-Chronic



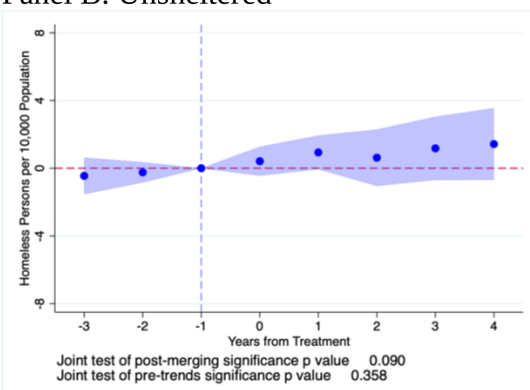
Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables, CoC, and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. Outcomes are for the rest of the state of a merged CoC.

Figure B7. *Neighboring CoCs* Time-Varying Generalized Difference-in-Difference – Homelessness Measures

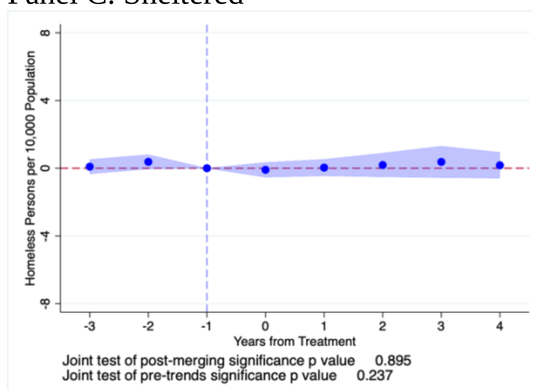
Panel A. Total Homeless



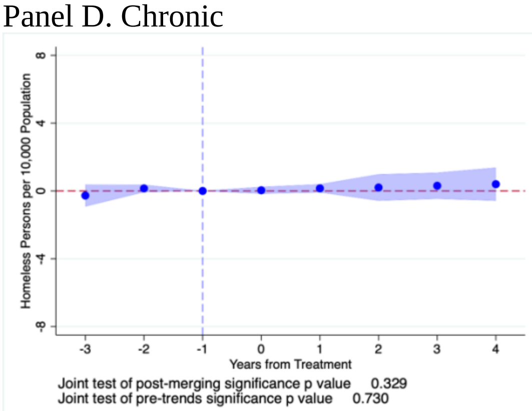
Panel B. Unsheltered



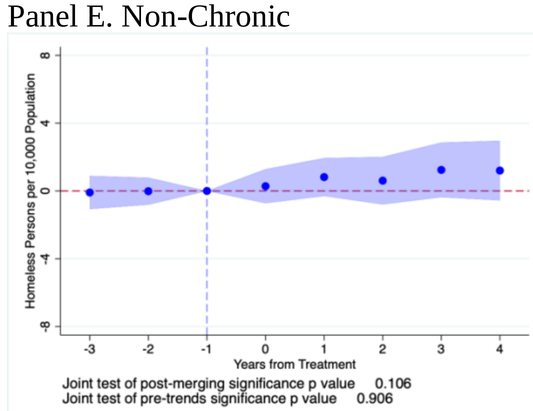
Panel C. Sheltered



Panel D. Chronic



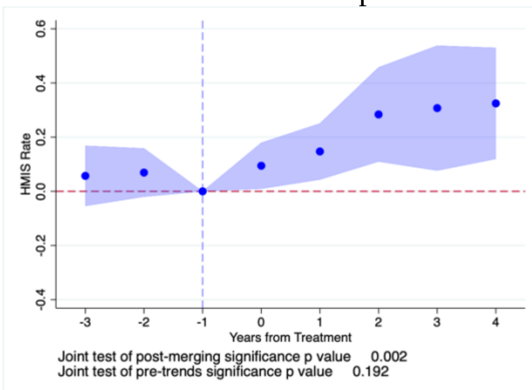
Panel E. Non-Chronic



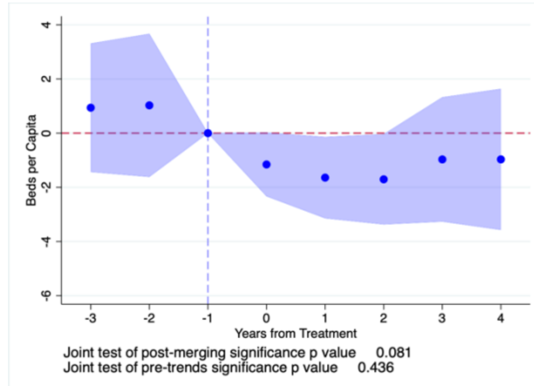
Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables, CoC, and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. Outcomes are for neighboring CoCs of a merged CoC.

Figure B8. Treated Once, Time-Varying Generalized Difference-in-Difference – Operations Measures

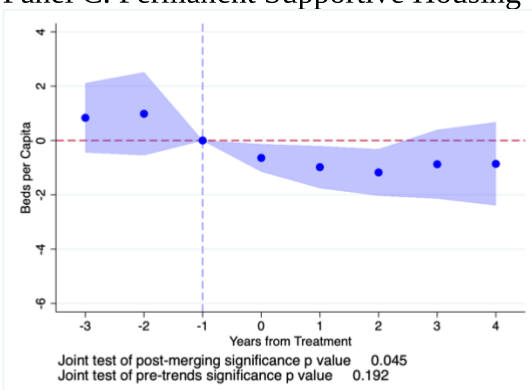
Panel A. HMIS PSH Participation Rate



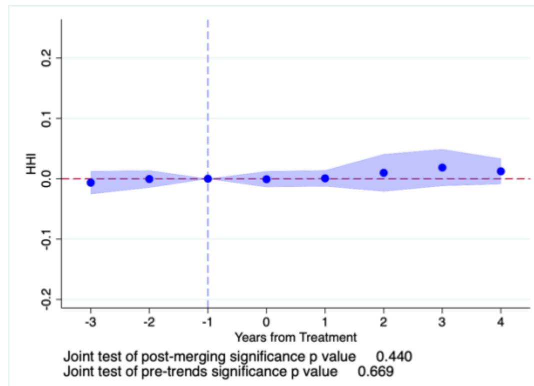
Panel B. Total Beds



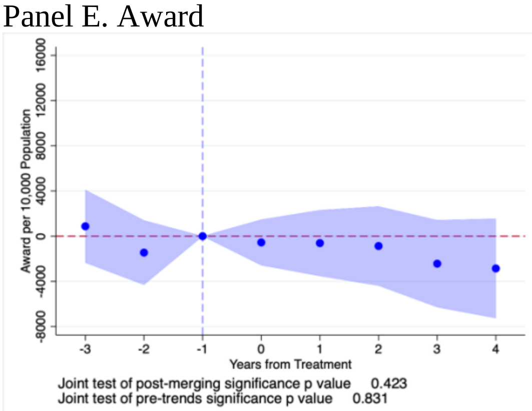
Panel C. Permanent Supportive Housing



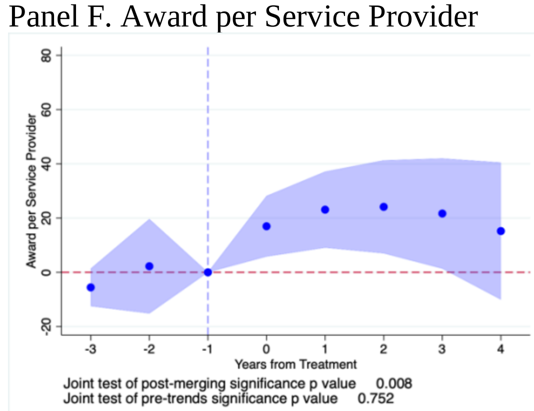
Panel D. HHI



Panel E. Award

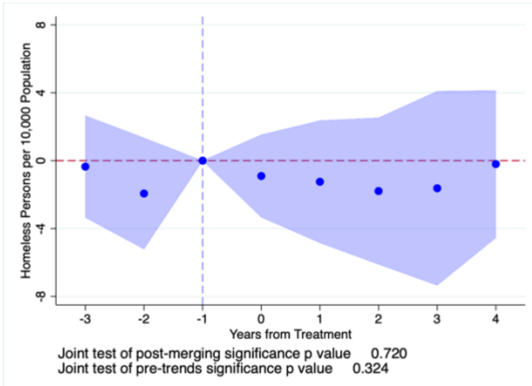


Panel F. Award per Service Provider

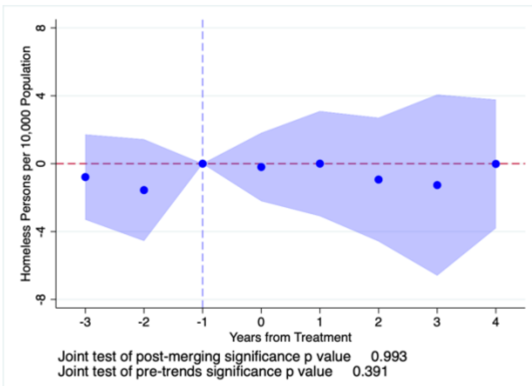


Treated Once Time-Varying Generalized Difference-in-Difference – Homelessness Measures

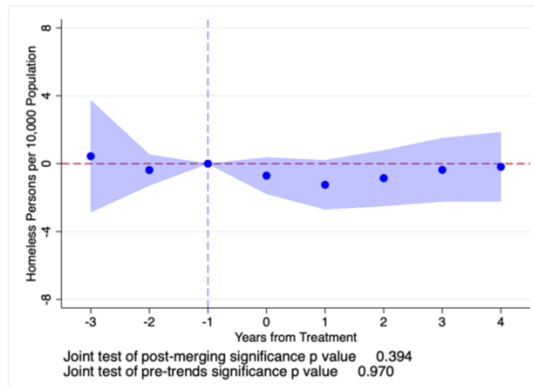
Panel G. Total Homeless



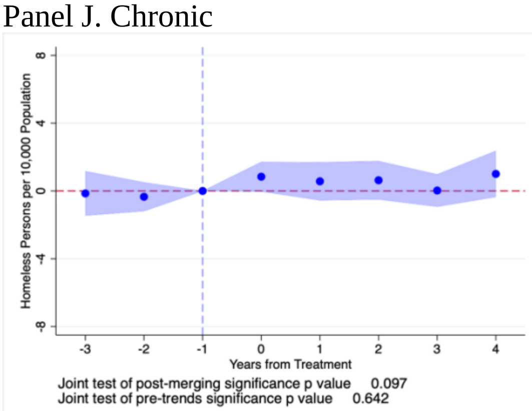
Panel H. Unsheltered



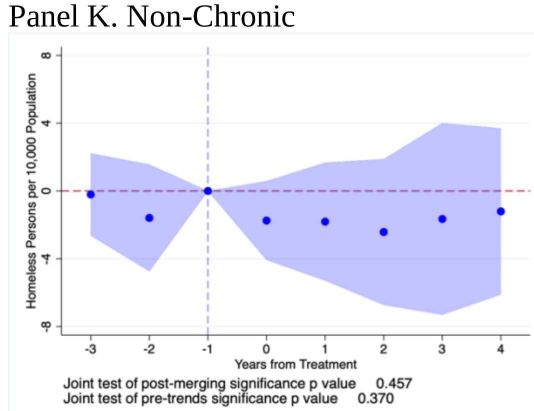
Panel I. Sheltered



Panel J. Chronic



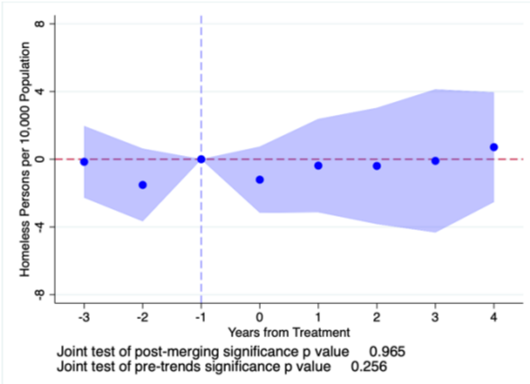
Panel K. Non-Chronic



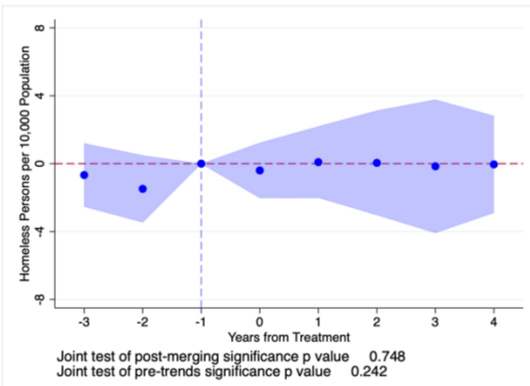
Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables, CoC, and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. CoCs merging multiple times are also dropped from the sample.

Figure B9. *Dropping No Changes* Time-Varying Generalized Difference-in-Difference – Homelessness Measures

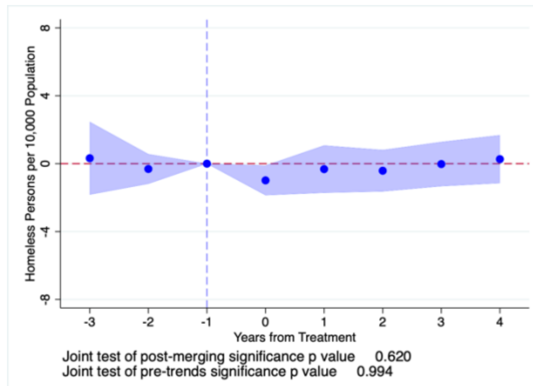
Panel A. Total Homeless



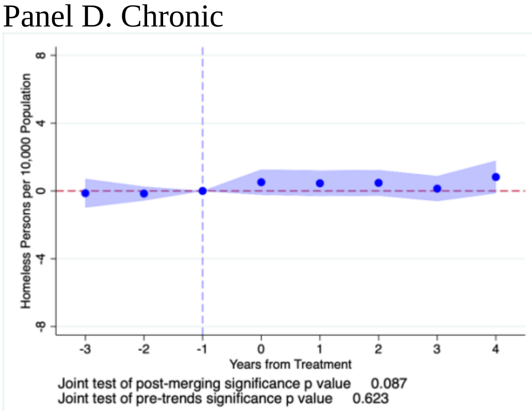
Panel B. Unsheltered



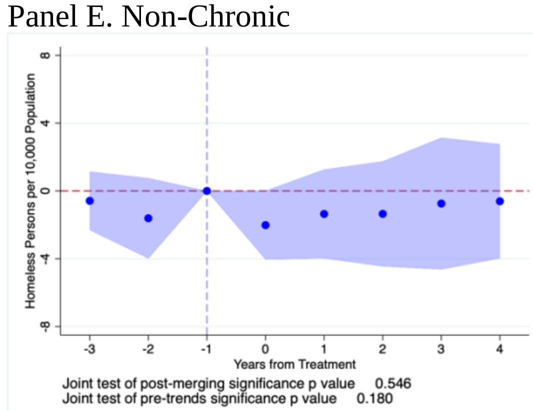
Panel C. Sheltered



Panel D. Chronic



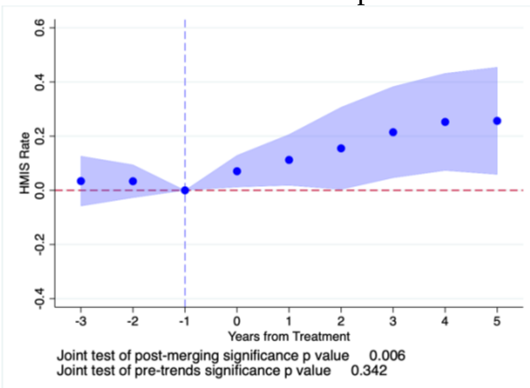
Panel E. Non-Chronic



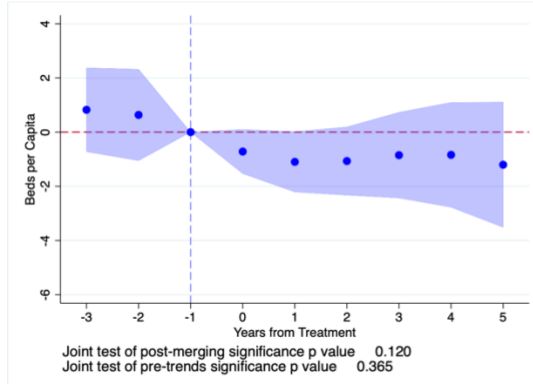
Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables, CoC, and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment. Observations where the change in subpopulation is zero are also dropped.

Figure B10. *No Control Variables*, Time-Varying Generalized Difference-in-Difference – Operations Measures

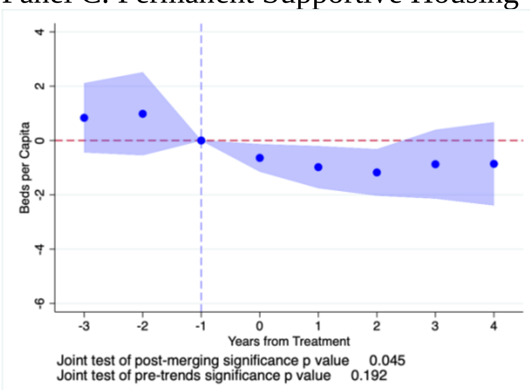
Panel A. HMIS PSH Participation Rate



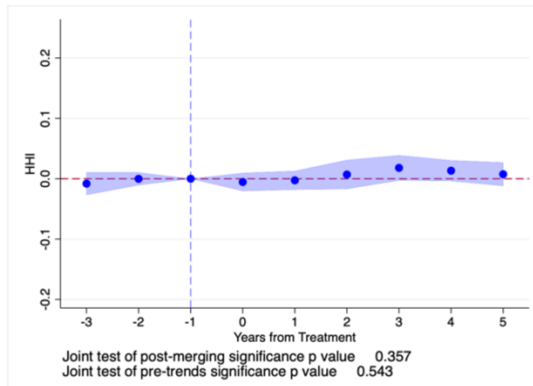
Panel B. Total Beds



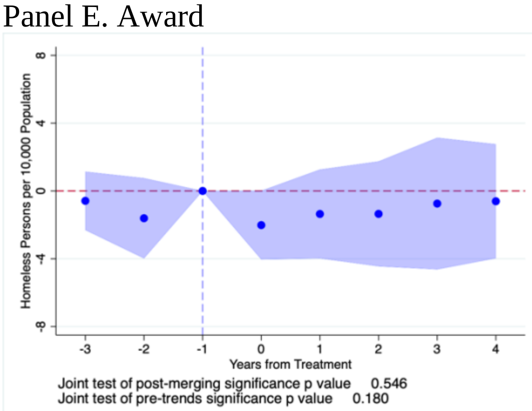
Panel C. Permanent Supportive Housing



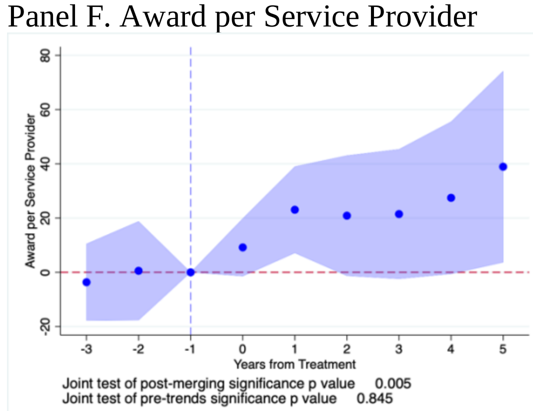
Panel D. HHI



Panel E. Award

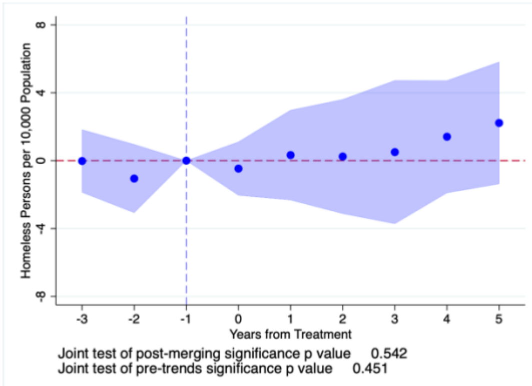


Panel F. Award per Service Provider

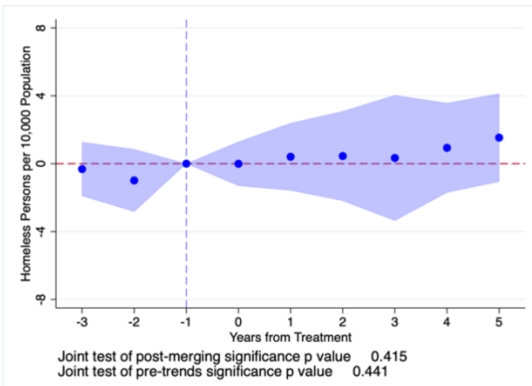


No Control Variables Time-Varying Generalized Difference-in-Difference – Homelessness Measures

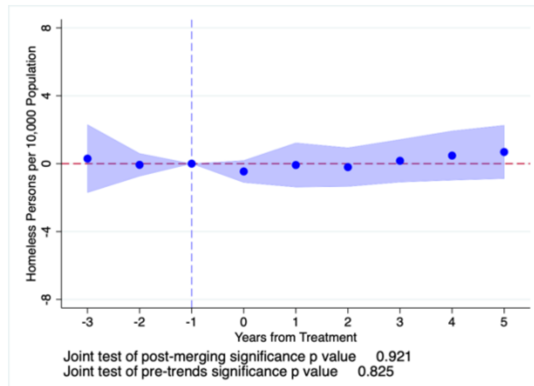
Panel G. Total Homeless



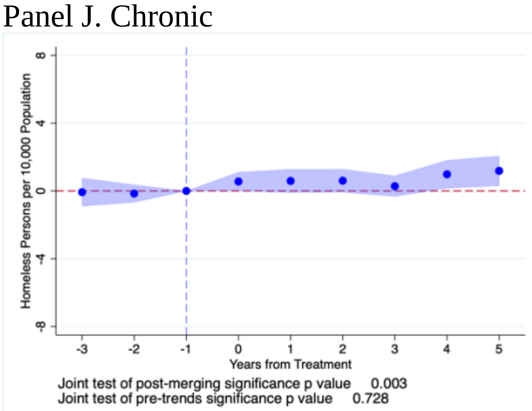
Panel H. Unsheltered



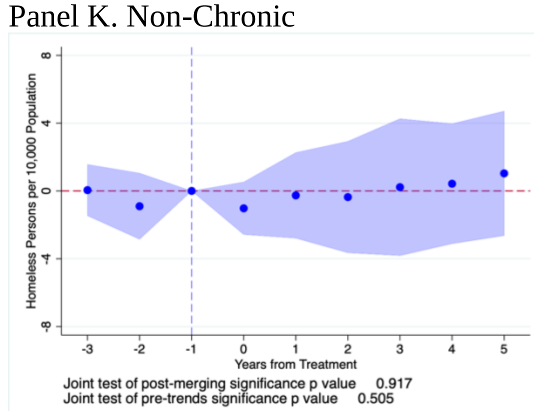
Panel I. Sheltered



Panel J. Chronic



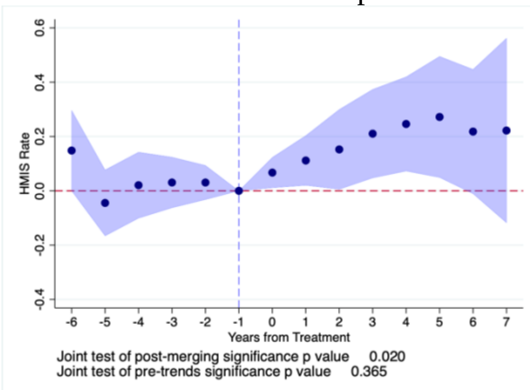
Panel K. Non-Chronic



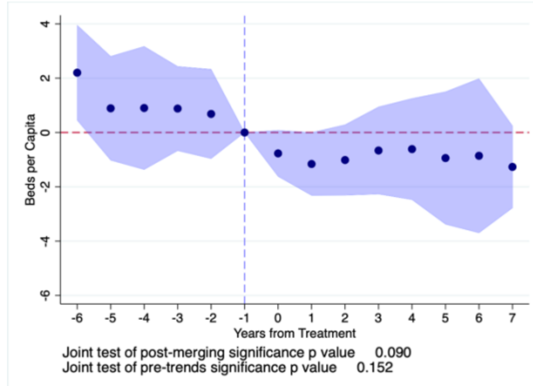
Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. CoC and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years from treatment.

Figure B11. *Unbalanced Panel*, Time-Varying Generalized Difference-in-Difference – Operations Measures

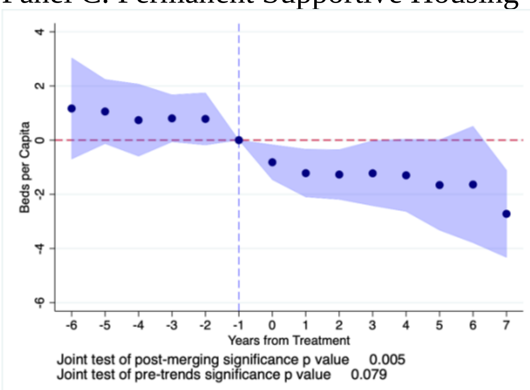
Panel A. HMIS PSH Participation Rate



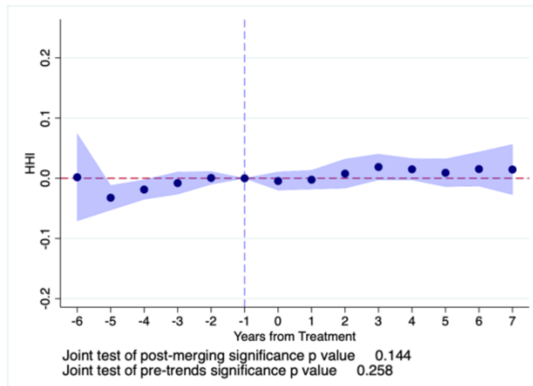
Panel B. Total Beds



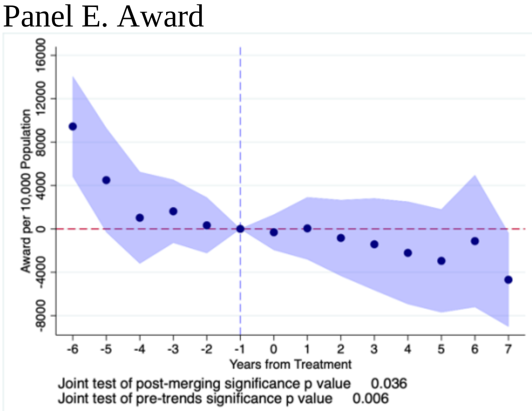
Panel C. Permanent Supportive Housing



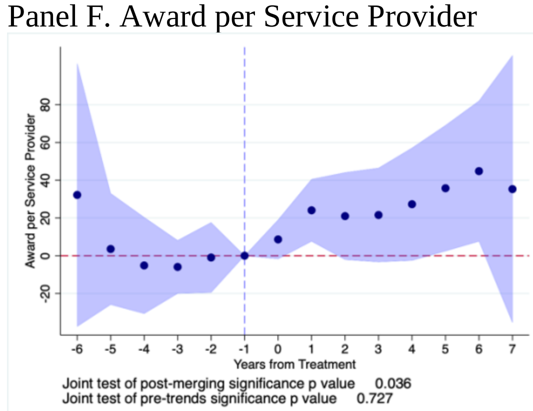
Panel D. HHI



Panel E. Award

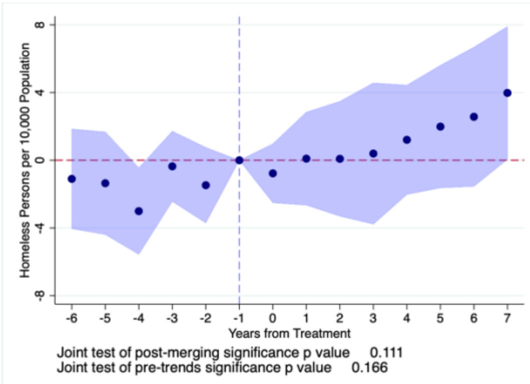


Panel F. Award per Service Provider

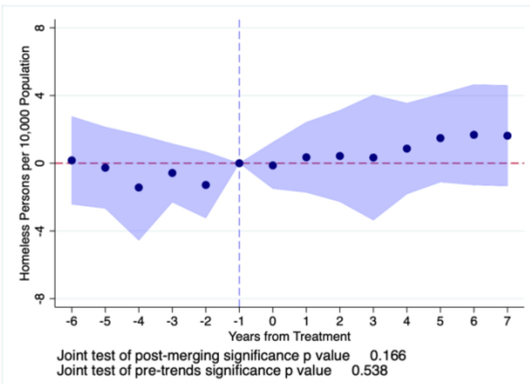


Unbalanced Panel, Time-Varying Generalized Difference-in-Difference – Homelessness Measures

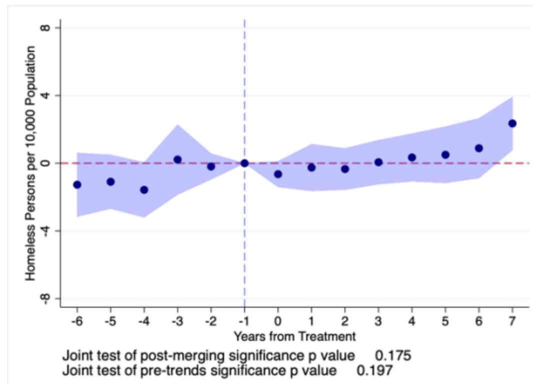
Panel G. Total Homeless



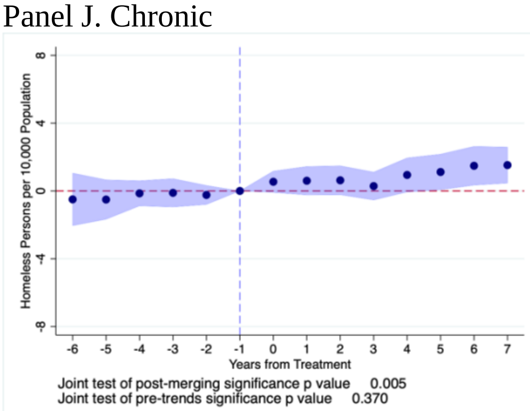
Panel H. Unsheltered



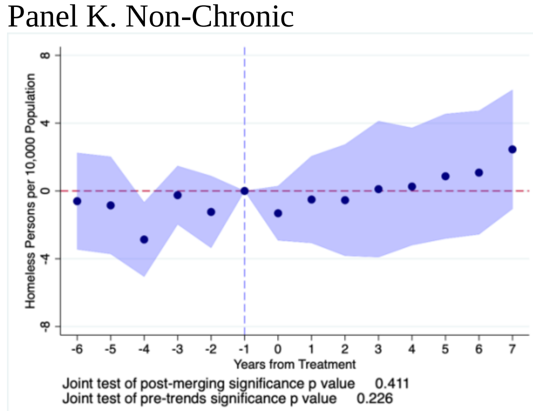
Panel I. Sheltered



Panel J. Chronic



Panel K. Non-Chronic



Notes: Areas in blue show 95% confidence intervals where points are coefficient for each year from treatment with a base year of -1. Model is event-study design with dummy variables for each year from treatment taking the value of one for a CoC if it merged and the observation is *j* years from treatment and zero otherwise. Control variables and CoC and year fixed effects are included in all models. Treatment occurs in period 0. Data are for years 2007-2017. CoCs that merged in 2007, 2008, and 2015-2017 are dropped from the sample to create a balanced sample relative to years in the sample.

C1. Definitions from the 2017 Annual Homeless Assessment Report to Congress

Chronically Homeless refers to an individual with a disability who has been continuously homeless for one year or more or has experienced at least four episodes of homelessness in the last three years where the combined length of time homeless in those occasions is at least 12 months.

Continuums of Care (CoC) are local planning bodies responsible for coordinating the full range of homelessness services in a geographic area, which may cover a city, county, metropolitan area, or an entire state

Emergency Shelter is a facility with the primary purpose of providing temporary shelter for homeless people.

Homeless describes a person who lacks a fixed, regular, and adequate nighttime residence.

Homeless Management Information System (HMIS) is a software application designed to record and store client-level information on the characteristics and service needs of homeless people. Each CoC maintains its HMIS, which can be tailored to meet local needs but must also conform to Federal HMIS Data and Technical Standards.

Housing Inventory Count (HIC) is produced by each CoC and provides an annual inventory of beds that assists people in the CoC who are experiencing homelessness or leaving homelessness.

An individual refers to a person who is not part of a family with children during an episode of homelessness. Individuals may be homeless as single adults, unaccompanied youth, or in multiple-adult or multiple-child households.

People in Families with children are people who are homeless as part of a household that has at least one adult (age 18 and older) and one child (under age 18).

Permanent Supportive Housing (PSH) is a housing model designed to provide housing assistance (project- and tenant-based) and supportive services on a long-term basis to formerly homeless people. HUD's Continuum of Care program, authorized by the McKinney-Vento Act, funds PSH and requires that the client have a disability for eligibility.

Point-in-Time (PIT) Count is an unduplicated 1-night estimate of both sheltered and unsheltered homeless populations. The 1-night count is conducted according to HUD standards by CoCs nationwide and occurs during the last 10 days in January of each year.

Sheltered Homelessness refers to people who are staying in emergency shelters, transitional housing programs, or safe havens.

Transitional Housing Programs provide people experiencing homelessness a place to stay combined with supportive services for up to 24 months.

Unsheltered Homelessness refers to people whose primary nighttime location is a public or private place not designated for, or ordinarily used as, regular sleeping accommodation for people (for example, the streets, vehicles, or parks).

Table C2. CoCs Smaller than Counties		
Original CoC	*Coded CoC*	*State*
Long Beach	Los Angeles County	CA
Pasadena		
Glendale		
Oxnard	Ventura County	
Lakeland	Polk County	FL
Atlanta	DeKalb County	GA
Evanston	Cook County	IL
Chicago		
Cambridge	Middlesex County	MA
Lowell		
Malden/Medford		
Framingham/Waltham		
Somerville		
Brookline/Newton		
Lawrence	Essex	
Lynn		
Fall City	Bristol	
New Bedford		
Detroit	Wayne County	MI
Amarillo	Potter County	TX
City of Spokane	Spokane County	WA

Notes: CoCs in table are CoCs smaller than counties. The first column shows the true CoC while the second shows which CoC I aggregated data to.

Old ID	Old Name	Consolidated into ID	Consolidated into Name	Year
AR-502	Conway/Arkansas River Valley	AR-503	Arkansas BoS	2010
AR-506	Johnson, Pope, Yell Counties	AR-503	Arkansas BoS	2010
AR-507	Eastern Arkansas	AR-503	Arkansas BoS	2012
AR-509	Hot Springs/Southwest Arkansas	AR-503	Arkansas BoS	2010
AR-510	Hempstead, Sevier, Howard, Little River Counties	AR-503	Arkansas BoS	2010
AR-511	Jonesboro/Northeast Arkansas	AR-503	Arkansas BoS	2010
CA-527	Nevada County	CA-515	Roseville, Rocklin/Placer, Nevada Counties	2010
CA-610	San Diego County	CA-601	San Diego City and County	2011
CA-605	San Buena Ventura/Ventura County	CA-611	Oxnard, San Buenaventura/Ventura County	2013
CT-500	Danbury	CT-505	Connecticut BoS	2011
CT-501	New Haven	CT-505	Connecticut BoS	2013
CT-504	Middletown/Middlesex County	CT-505	Connecticut BoS	2010
CT-507	Norwich/New London City & County	CT-505	Connecticut BoS	2010
CT-509	New Britain	CT-505	Connecticut BoS	2011
CT-510	Bristol	CT-505	Connecticut BoS	2011
IL-505	Evanston	IL-511	Cook County	2011
MA-512	Lawrence	MA-516	Massachusetts BoS	2013
ME-501	Bangor/Penobscot County	ME-500	Maine BoS	2012
MI-522	Alpena, Iosca, Presque Isle/NE Michigan	MI-500	Michigan BoS	2010
MN-510	Scott, Carver Counties	MN-503	Dakota, Anoka, Washington, Scott, Carver Counties	2011
NE-503	Southwest Nebraska	NE-500	Nebraska BoS	2011
NE-504	Southeast Nebraska	NE-500	Nebraska BoS	2011
NE-505	Panhandle of Nebraska	NE-500	Nebraska BoS	2011

NE-506	Northeast Nebraska	NE-500	Nebraska BoS	2011
NJ-505	Gloucester County	NJ-503	Camden City & County/Gloucester, Cape May, Cumberland Counties	2013
NJ-520	Cumberland County	NJ-503	Camden City & County/Gloucester, Cape May, Cumberland Counties	2013
NJ-519	Sussex County	NJ-516	Warren, Sussex, Hunterdon Counties	2011
NY-524	Niagara Falls/Niagara County	NY-508	Buffalo, Niagara Falls/Erie, Niagara, Orleans, Genesee, Wyoming Counties	2013
NY-605	Nassau County	NY-603	Nassau, Suffolk Counties	2012
OR-504	Salem/Marion, Polk Counties	OR-505	Oregon BoS	2011
SC-504	Florence City & County/Pee Dee	SC-503	Myrtle Beach, Sumter City & County	2010
TX-501	Corpus Christi/Nueces County	TX-607	Texas BoS	2013
TX-504	Victoria/Dewitt, Lavaca,Conzales Counties	TX-607	Texas BoS	2013
TX-610	Denton City & County	TX-607	Texas BoS	2012
TX-613	Longview/Marshall Area	TX-607	Texas BoS	2010
TX-702	Montgomery County	TX-607	Texas BoS	2012
TX-704	Galveston/Gulf Coast	TX-607	Texas BoS	2011
VA-512	Chesapeake	VA-501	Norfolk, Chesapeake, Suffolk/Isle of Wight, Southampton Counties	2011
VA-519	Suffolk	VA-501	Norfolk, Chesapeake, Suffolk/Isle of Wight, Southampton Counties	2011
VA-518	Harrisonburg/ Rockingham County	VA-513	Harrisonburg, Winchester/Western Virginia	2012
VA-509	Petersburg	VA-521	Virginia BoS	2013

VA-510	Staunton/Waynesboro/Augusta, Highland Counties	VA-521	Virginia BoS	2013
VA-517	Danville/Martinsville	VA-521	Virginia BoS	2013

Notes: List of mergers comes from U.S. Department of Housing Point-in-Time Counts report.

Table C3. Merging CoCs included in analytical sample

APPENDIX 3. Appendix for Chapter 3

	Share Sheltered		*Share Doubled Up*	
	Without Controls	With Controls	Without Controls	With Controls
BW: 5 percentiles				
MV Grant Recipient	0.03 (0.16)	0.13 (0.15)	0.14 (0.43)	0.31 (0.64)
Observations	4,218	3,312	4,218	3,312
BW: 10 percentiles				
MV Grant Recipient	-0.02 (0.10)	0.04 (0.11)	-0.18 (0.29)	-0.07 (0.35)
Observations	8,824	7,095	8,824	7,095
BW: 5 percentiles, Quadratic Distance				
MV Grant Recipient	-0.16 (0.18)	-0.15 (0.18)	0.56 (0.82)	0.50 (1.07)
Observations	4,179	3,404	4,179	3,404

Notes: Each cell is a different model, with six models for each outcome. Each outcome has a model with a bandwidth of 5 percentiles, 10 percentiles, and 5 percentiles with quadratic distance instead of linear. Each then also is estimate without and then with controls. MV Grant Recipient rows show estimated effect of a district's receiving a McKinney-Vento Homeless Assistance grant. Standard errors clustered at LEA level in parentheses. Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. All models include state fixed effects, year fixed effects, and state-by-year fixed effects. Models with control variables include enrollment, share of students that are Black and share Hispanic, estimated share in poverty, and whether urban, suburban, or a town relative to being a rural district. Observations are school district by year. *p<0.05 **p<0.01 ***p<0.001

Table A1. Homelessness

	Share Proficient		*Share Proficient – 3rd Grade*		*Share Proficient – High School*	
	Without Controls	With Controls	Without Controls	With Controls	Without Controls	With Controls
BW: 5 percentiles						
MV Grant Recipient	-6.86*** (1.704)	-4.96* (2.00)	-6.67* (2.76)	-3.74 (3.13)	-2.75 (2.58)	-0.27 (3.02)
Observations	2,885	2,621	1,354	1,299	1,002	929
BW: 10 percentiles						
MV Grant Recipient	-4.79** (1.57)	-3.02 (1.78)	-4.17 (2.44)	-1.71 (2.81)	-3.93 (2.22)	-1.62 (2.61)
Observations	5,945	5,379	2,580	2,478	1,888	1,736
BW: 5 percentiles, Quadratic Distance						
MV Grant Recipient	-4.46 (2.72)	-2.43 (2.96)	-0.98 (3.75)	3.32 (4.05)	-1.46 (3.74)	-0.61 (4.31)
Observations	2,883	2,622	1,300	1,261	951	888

Notes: Each cell is a different model, with six models for each outcome. Each outcome has a model with a bandwidth of 5 percentiles, 10 percentiles, and 5 percentiles with quadratic distance instead of linear. Each then also is estimate without and then with controls. MV Grant Recipient rows show estimated effect of a district's receiving a McKinney-Vento Homeless Assistance grant. Standard errors clustered at LEA level in parentheses. Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. All models include state fixed effects, year fixed effects, and state-by-year fixed effects. Models with control variables include enrollment, share of students that are Black and share Hispanic, estimated share in poverty, and whether urban, suburban, or a town relative to being a rural district. Observations are school district by year. $^{*}p<0.05$ $^{**}p<0.01$ $^{***}p<0.001$

Table A2. Math Tests

	Share Proficient		*Share Proficient – 3rd Grade*		*Share Proficient – High School*	
	Without Controls	With Controls	Without Controls	With Controls	Without Controls	With Controls
BW: 5 percentiles						
MV Grant Recipient	-2.45 (1.91)	-0.51 (2.23)	-4.17 (2.71)	-0.89 (3.06)	-3.52 (3.41)	-3.55 (4.00)
Observations	2,886	2,620	1,355	1,297	1,038	957
BW: 10 percentiles						
MV Grant Recipient	-3.27 (1.73)	-1.50 (1.97)	-3.26 (2.35)	-1.06 (2.83)	-5.41 (2.91)	-5.27 (3.52)
Observations	5,955	5,389	2,570	2,467	1,951	1,795
BW: 5 percentiles, Quadratic Distance						
MV Grant Recipient	-8.24*** (3.13)	-5.47 (3.23)	-0.98 (3.57)	2.74 (4.96)	-5.14 (5.43)	-5.45 (5.84)
Observations	2,896	2,636	1,296	1,257	1,005	939

Notes: Each cell is a different model, with six models for each outcome. Each outcome has a model with a bandwidth of 5 percentiles, 10 percentiles, and 5 percentiles with quadratic distance instead of linear. Each then also is estimate without and then with controls. MV Grant Recipient rows show estimated effect of a district's receiving a McKinney-Vento Homeless Assistance grant. Standard errors clustered at LEA level in parentheses. Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. All models include state fixed effects, year fixed effects, and state-by-year fixed effects. Models with control variables include enrollment, share of students that are Black and share Hispanic, estimated share in poverty, and whether urban, suburban, or a town relative to being a rural district. Observations are school district by year. *p<0.05 **p<0.01 ***p<0.001

Table A3. Reading/ELA Tests

	Revenue from City/County per student		*Title I Revenue per student*		*Student Support Expenditures per student*		*Transportation Expenditures per student*	
	Without Controls	With Controls	Without Controls	With Controls	Without Controls	With Controls	Without Controls	With Controls
BW: 5 percentiles								
MV Grant Recipient	-524 (272)	-520 (296)	-114 (145)	-32 (34)	218 (820)	-101 (230)	-148 (213)	-75 (66)
Observations	2,961	2,499	2,961	2,499	2,961	2,499	2,961	2,499
BW: 10 percentiles								
MV Grant Recipient	-318 (188)	-261** (94)	-113 (210)	105 (90)	8,233 (6,375)	4,916 (4,325)	647 (760)	1,323 (1,086)
Observations	6,445	5,458	6,445	5,458	6,445	5,458	6,445	5,458
BW: 5 percentiles, Quadratic Distance								
MV Grant Recipient	-192 (351)	-134 (313)	-142 (274)	37 (50)	-556 (1,910)	433 (428)	-670 (484)	57 (115)
Observations	2,939	2,542	2,939	2,542	2,939	2,542	2,939	2,542

Notes: Each cell is a different model, with six models for each outcome. Each outcome has a model with a bandwidth of 5 percentiles, 10 percentiles, and 5 percentiles with quadratic distance instead of linear. Each then also is estimate without and then with controls. MV Grant Recipient rows show estimated effect of a district's receiving a McKinney-Vento Homeless Assistance grant. Standard errors clustered at LEA level in parentheses. Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. All models include state fixed effects, year fixed effects, and state-by-year fixed effects. Models with control variables include enrollment, share of students that are Black and share Hispanic, estimated share in poverty, and whether urban, suburban, or a town relative to being a rural district. Observations are school district by year. *p<0.05 **p<0.01 ***p<0.001

Table A4. Finances

	Disability Allegations per 1,000 students		*Race Allegations per 1,000 students*		*In School Suspensions per 1,000 students*		*Out of School Suspensions per 1,000 students*	
	Without Controls	With Controls	Without Controls	With Controls	Without Controls	With Controls	Without Controls	With Controls
BW: 5 percentiles								
MV Grant Recipient	0.10 (0.28)	0.01 (0.41)	0.88 (1.12)	0.10 (1.11)	7.66 (15.56)	10.21 (16.94)	-26.30 (16.46)	-4.71 (10.96)
Observations	1,032	842	1,032	842	1,035	844	1,035	844
BW: 10 percentiles								
MV Grant Recipient	0.76 (0.47)	0.39 (0.60)	2.44 (1.25)	3.19* (1.32)	13.46 (18.83)	-4.26 (17.80)	15.80 (15.98)	-8.78 (10.07)
Observations	2,188	1,830	2,188	1,830	2,193	1,833	2,193	1,833
BW: 5 percentiles, Quadratic Distance								
MV Grant Recipient	0.49 (0.59)	0.11 (0.61)	0.27 (1.05)	-0.94 (1.00)	-11.18 (32.70)	-29.79 (30.76)	-35.63 (28.51)	-10.59 (17.66)
Observations	1,019	866	1,019	866	1,020	866	1,020	866

Notes: Each cell is a different model, with six models for each outcome. Each outcome has a model with a bandwidth of 5 percentiles, 10 percentiles, and 5 percentiles with quadratic distance instead of linear. Each then also is estimate without and then with controls. MV Grant Recipient rows show estimated effect of a district's receiving a McKinney-Vento Homeless Assistance grant. Standard errors clustered at LEA level in parentheses. Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year, which is then used to estimate effects. All models include state fixed effects, year fixed effects, and state-by-year fixed effects. Models with control variables include enrollment, share of students that are Black and share Hispanic, estimated share in poverty, and whether urban, suburban, or a town relative to being a rural district. Observations are school district by year. *p<0.05 **p<0.01 ***p<0.001

Table A5. Discipline

	BW: 5 percentiles	*BW: 5 percentiles*	*BW: 10 percentiles*	*BW: 10 percentiles*	*BW: 5 percentiles Quadratic*	*BW: 5 percentiles Quadratic*
Above Threshold	0.49***	0.43***	0.36***	0.33***	0.43***	0.40***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Distance	-0.05***	-0.04***	-0.02***	-0.01***	-0.06**	-0.06**
	(0.00)	(0.01)	(0.00)	(0.00)	(0.02)	(0.02)
Distance x Above	-0.03***	-0.04***	-0.01***	-0.01***	-0.04	-0.02
	(0.01)	(0.01)	(0.00)	(0.00)	(0.03)	(0.03)
Distance^2					0.01*	0.01
					(0.01)	(0.01)
Distance^2 x Above					-0.01	-0.01
					(0.00)	(0.00)
Controls	No	Yes	No	Yes	No	Yes
Kleibergen-Paap Wald rk F stat	435	291	475	346	154	129
Observations	4,218	3,312	8,824	7,094	4,179	3,404

Notes: Each column is a different model. The Above Threshold row show estimated relationship between being above the estimated threshold and of district's receiving a McKinney-Vento Homeless Assistance grant. Standard errors clustered at LEA level in parentheses. Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. The first stage is predicting the likelihood of receiving a grant the following year. All models include state fixed effects, year fixed effects, and state-by-year fixed effects. Models with control variables include enrollment, share of students that are Black and share Hispanic, estimated share in poverty, and whether urban, suburban, or a town relative to being a rural district. Observations are school district by year. *p<0.05 **p<0.01 ***p<0.001

Table A6. First-Stage

	Share Sheltered	*Share Doubled Up*	*Revenue from City/County per student*	*Title I Revenue per student*	*Student Support Expenditures per student*	*Transportation Expenditures per student*
MV Grant Recipient	0.18*** (0.02)	2.30*** (0.12)	-60.49** (19.92)	4.16 (4.62)	230.45 (142.63)	52.23* (22.93)
Observations	46,880	46,879	38,544	38,544	38,544	38,544

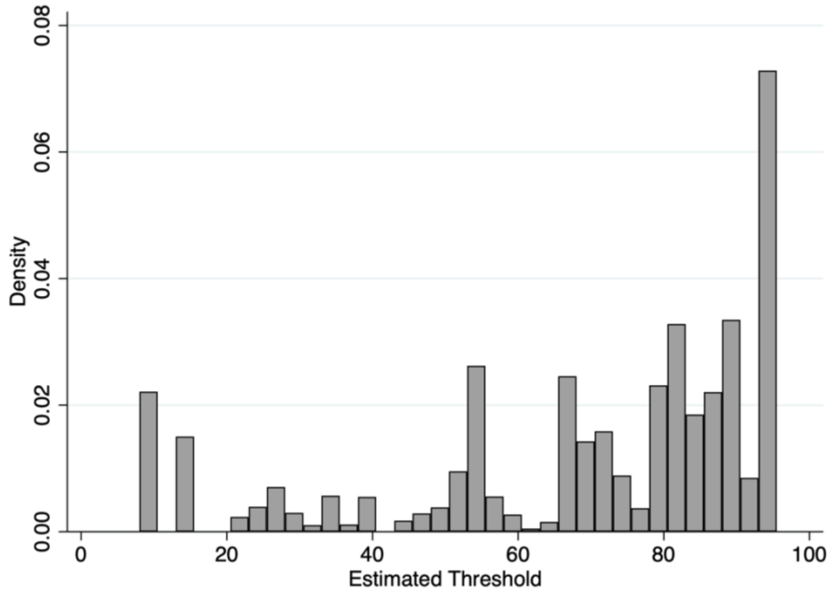
	Share Proficient, Math	*Share Proficient – 3rd Grade, Math*	*Share Proficient – High School, Math*	*Share Proficient, ELA*	*Share Proficient – 3rd Grade, ELA*	*Share Proficient – High School, ELA*
MV Grant Recipient	-1.46*** (0.28)	-1.22* (0.48)	-2.39*** (0.47)	-0.93** (0.28)	-1.98*** (0.45)	-1.90*** (0.52)
Observations	26,429	10,350	7,404	26,535	10,289	7,659

	Disability Allegations per student	*Race Allegations per student*	*In School Suspensions per student*	*Out of School Suspensions per student*
MV Grant Recipient	-0.12 (0.06)	-0.07 (0.09)	2.27 (1.72)	2.41* (1.20)
Observations	9,347	9,347	9,357	9,357

Notes: Each cell is a different outcome. MV Grant Recipient rows show estimated effect of a district's receiving a McKinney-Vento Homeless Assistance grant with standard errors clustered at LEA level in parentheses. Data on homelessness and enrollment come from EDFacts. Models include controls variables, state fixed effects, year fixed effects, and state-by-year fixed effects. Control variables include enrollment, share of students that are Black and share Hispanic, estimated share in poverty, and whether urban, suburban, or a town relative to being a rural district. Observations are school district by year. $*p<0.05$ $**p<0.01$ $***p<0.001$

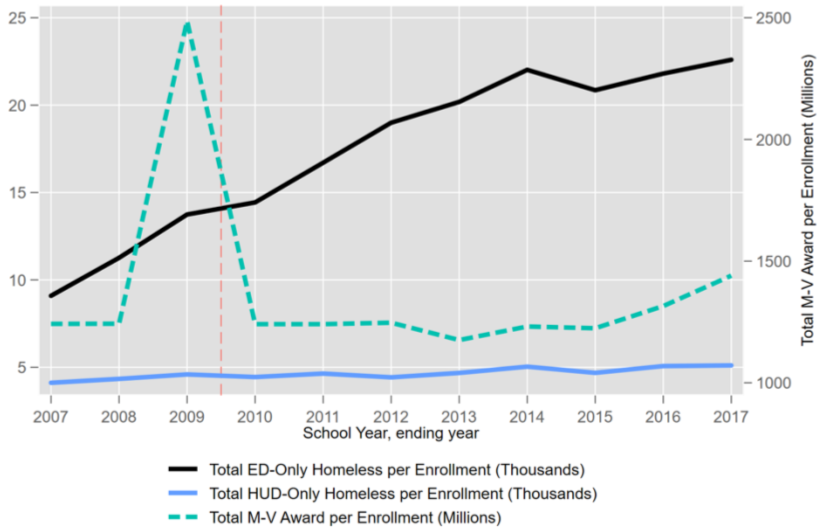
Table B1. Naïve Results of Receiving Grant

Figure B1. Distribution of Estimated Thresholds



Notes: Data on homelessness and enrollment come from EDFacts. Threshold is estimated by finding the state-by-year percentile of homeless students most increasing a district's discontinuous probability of receiving a McKinney-Vento grant within the bandwidth of five. The estimated threshold is then used to find each district's state-by-year distance to the threshold to be used as the running variable for the fuzzy regression discontinuity. This graph shows distribution of estimated percentiles. The x-axis shows districts' state-by-year percentile of homelessness estimated threshold.

Figure B2. Levels of Student Homelessness and McKinney-Vento Funding



Notes: Notes: Data come from Section 1.9 of Consolidated State Performance Reports. Vertical line is year marks when districts received the increase in McKinney-Vento funding.

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