

**RENT CONTROL IN NEW JERSEY:
DO MUNICIPALITY-SPECIFIC RENT CONTROL ORDINANCES
KEEP RENT PRICES LOW?**

BY
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A Thesis

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RENT CONTROL IN NEW JERSEY:

**Do municipality-specific rent control ordinances keep rent
prices low?**

Anais Goubert

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Abstract

Rental housing affordability, an integral component of overall wellbeing, remains out of reach for a majority of Americans. Second generation rent controls, a policy first introduced in the early 1970s which limits the amount landlords can raise rents yearly, may help keep rent prices affordable while allowing landlords a modest profit. Though traditional analyses of rent control have led economists to firmly oppose such policies, findings from recent empirical research on the effects of rent control on rent prices are varied, and often contradictory. This thesis seeks to evaluate the effects of moderate, local rent control policies on rent prices in New Jersey. I begin by replicating and extending Ambrosius et al.'s 2015 study, which utilizes OLS regressions. Though my findings indicate that rent control in NJ, regardless of the policy's strength, has little to no statistically significant effect on rents and rents per room, I argue, using the minimum wage revolution as a guiding framework, that spatial heterogeneity requires the use of models utilizing matching to better understand the real effects of rent control. I present a difference-in-differences model, though data limitations prevent me from running this model. This thesis emphasizes the need for more research, particularly using better methodological design such as a difference-in-differences approach, on rent control's effects to gain a deeper understanding of the role of rent control in addressing the US's rental housing affordability crisis.

Dr. Mark Paul, Assistant Professor of Economics
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Chapter 1

Literature Review

Recently, there has been a spate of interest in the importance of housing affordability and the potential for rent control to help combat the state of unaffordable rental housing in the United States. Knowledge of the effects of rent control on rent prices and neighborhood stability has great importance for tenant organizations, policymakers, local politicians, and economists as they seek to address the negative effects of unaffordable rent prices. A consensus among these individuals and organizations has yet to be established, even as the continued negative effects of unaffordable housing and the recent resurgence in the empirical evaluations of the effects of rent control highlight the importance of developing a response to the US rental housing affordability crisis.

Housing affordability is a concern of municipal, state, and federal governments, all of which have sought to promote stable and affordable housing (Daniel 2018; American Planning Association 2021; National Council of State Housing Agencies 2021). I will briefly explain why governments seek to play a role in the rental housing market. First and foremost, affordable housing is an integral component of wellbeing, decreasing the risk of homelessness among vulnerable populations, increasing a household's ability to manage financial obligations and crises, providing stable learning environments thereby bettering educational outcomes, and improving health outcomes as households can utilize more of their resources for preventative care (more healthful food or access to quality services) and other health care interventions (Brisson 2014;

Schwartz 2016; Baqutayan, Ariffin, and Raji 2015; Baqutayan, Ariffin, and Raji 2016). The effects of unaffordable housing are particularly salient when considering the rental housing market. Rent-burdened households—households which spend upwards of 30% of their income on rent—face “higher eviction rates, increased financial fragility, and wider use of social safety net programs, compared with other renters and homeowners” (Currier et al. 2018, 4). Rent-burdened families similarly tend to have lower savings, thus making them increasingly subject to severe material hardships in the long run and to difficulties in dealing with unexpected income shocks and volatility; similarly, rent-burdened households are significantly less likely than homeowners and non-rent burdened households to have an account with a financial institution (Currier et al. 2018).

Though, as evidenced above, housing, and specifically rental, affordability is an important component of wellbeing, affordable rental units remain out of reach for a large proportion of US residents. With approximately 36 percent of households in the United States renting (Joint Center for Housing Studies 2019) and full-time minimum wage workers unable to rent a one-bedroom apartment in 95 percent of US counties (National Low Income Housing Coalition 2020), affordable housing, specifically affordable rent, is unattainable for many. Over the past couple years, rental prices have outpaced inflation in the economy, as evidenced by the 3.6 percent increase in rent prices in 2018 according to the Consumer Price Index and by the year-to-year growth of rents exceeding double the rate of inflation in April of 2019 (JCHS 2019). In 2017, approximately 48 percent of renters were rent-burdened, paying more than 30 percent of their income on their rent (GAO 2020).

Housing unaffordability and the resulting housing instability has been centered in prior political discourse, as illustrated by the adoption of a nationwide solution to unaffordable rent, commonly referred to as first generation rent control. First imposed in the United States as a complete freeze on nominal rents following the country’s entry into the Second World War¹, this first generation of rent controls aimed to keep

1. Some local rent control laws were considered during WW1 and some cities saw gradual rent control policies enacted during the 1930s, but rent control laws were enacted more broadly during WW2 (Rajasekaran, Treskon, and Greene 2019; Willis 1947; Willis 1950).

housing affordable as significant numbers of women and Black men moved throughout the country to accommodate for the shortage of labor following the exodus of male soldiers to the European front (Arnott 1995). These major migrations destabilized local housing markets, caused rising rent prices due to “dynamic labor market growth” (Rajasekaran, Treskon, and Greene 2019, 2), and generated opportunities for profiteering. The implementation of first-generation rent controls not only addressed these three primary concerns, but the appropriateness war-time policies is also largely undisputed, as they generate little to no efficiency loss (Arnott 1995; Lett 1976). Though these controls continued following the end of the war, primarily as a precaution given the return of soldiers stationed overseas, “a housing boom in the late 1940s and early 1950s [...] lowered market-clearing rents and permitted almost painless decontrol” (Arnott 1995, 100). Consensus among economists notes that a complete freeze on nominal rents is an extreme and ineffective method for increasing the affordability of rent (Arnott 1995; Jenkins 2009), thus leading policymakers to consider a more modest, market-oriented version of rent control, commonly referred to as rent-stabilization or second-generation rent control. Today’s proposed versions of rent control policies vary significantly from first generation rent freezes², typically controlling the percentage increase landlords can apply to their rents by fixing the permitted rates of increase to inflation. Because modern rent control is more flexible—allowing modest profits for landlords, vacancy decontrols, and, frequently, provisions to favor housing production (Arnott 1995)—second-generation rent controls have been revisited as a solution to unaffordable rental housing, both in the academic literature and in various jurisdictions throughout the United States (Gilderbloom and Ye 2007; Ambrosius et al. 2015; Pastor, Carter, and Abood 2018).

Second-generation rent controls were implemented primarily throughout the coastal United States, notably within the Northeast and California (Rajasekaran, Treskon, and Greene 2019) and a majority of the localities with rent control are located in New York, New Jersey, California, Oregon, and Maryland. The National Apartment

2. For example, the rent control ordinance in Atlantic County (NJ) allows landlords to increase rent prices yearly to maintain pace with inflation, to pass along a surcharge to tenants for significant improvements to the unit, and does not apply to units built after 1987.

Association (NAA), alongside the Urban Institute’s 2019 report “Rent Control,” have noted that only 182 cities and municipalities, of approximately 89,000 local governments (US Census Bureau 2012), across the United States have rent control, with 36 states preempting the adoption of rent control policies by local governments (NAA 2018). This preemption by a majority of US states, which restricts lower levels of government from implementing rent control, is of particular importance, since all rent control policies but one, Oregon’s recently passed statewide rent control act, have been implemented by local governments.

Despite new and improved varieties of rent control and a persistent rent affordability crisis, governments have remained reluctant to enact some version of rent stabilization. A variety of potential explanations exist, such as the argument by some states—Arizona, Colorado, Kentucky, Louisiana, Massachusetts, New Hampshire, Oregon, and Washington (iProperty Management 2020)—that rent control is an issue of statewide importance and thus cannot be addressed by local municipalities. Another explanation, perhaps of greater importance considering the resurgence in academic literature on the effects of rent stabilization policies, is founded largely upon economists’ traditional analyses of rent control. Such analyses argue that the free market is better equipped to handle issues regarding housing affordability, with rent control potentially worsening the housing crisis by limiting new construction, creating demand-supply mismatches and deadweight loss, and leading to the deterioration of existing units (iProperty Management 2020; California Association of Realtors; Wiltz 2018).

Understanding the effect of traditional economic examinations of price controls on the state of rent stabilization policies nationwide requires an examination of such analyses. Perhaps most notable is Friedman and Stigler 1946 study, *Roofs or Ceilings: The Current Housing Problem*, which was one of the first pieces of work to outline and evaluate the effects of rent control (in English). Such foundational work claimed that price rationing is preferable to rent control and politically infeasible public rationing. Friedman and Stigler, specifically, argued that “rent ceilings, therefore, cause haphazard and arbitrary allocation of space, inefficient use of space, retardation of

new construction and indefinite continuance of rent ceilings, or subsidization of new construction and a future depression in residential building” (1946). Basu and Emerson (2000) further such work, by exploring how inflation impacts rent under tenancy rent control, arguing that the “removal of rent control laws can not only increase efficiency in the rental market but can also lead to a general lowering of rents, making all tenants better off” (Basu and Emerson 2000, 958). The effects of this foundational work are longstanding, as evidenced by the over 80% of surveyed economists (author’s calculations using IGM Poll 2012) who disagreed or strongly disagreed that “local ordinances that limit rent increases for some rental housing units [...] have had a positive impact over the past three decades on the amount and quality of broadly affordable rental housing in cities that have used them” (IGM 2012). Some of these well-respected economists who have disagreed have gone so far as to explain their answers by stating “Next questions: does the sun revolve around the earth” (Thaler, in IGM Poll 2012) or “Unless all the textbooks are wrong, this is wrong” (Schmalensee, in IGM Poll 2012). Though economists seem clear in their assertions that rent control is inefficient and that this policy’s costs outweigh its potential benefits, we have seen the abstractions used by economists in textbook analyses of price controls inaccurately reflect real world outcomes, notably in the minimum wage debate. I review the textbook models for both rent control and the minimum wage, the assumptions necessary for their findings to hold, and their potential drawbacks in chapter three.

Similarly to rent control, minimum wage policies seek to address crises primarily impacting lower-income individuals through a price control mechanism. Though minimum wage acts as a price floor rather than ceiling, neoclassical analyses of minimum wage asserted that such a price control would harm low-skilled workers as firms’ quantity of labor demanded fell and unemployment rose. A 1992 poll of American Economic Association (AEA) members noted that 79% of respondents believed that an increase in the minimum wage would result in greater unemployment for young and low-skilled workers (The Economist 2020), whereas recent polls by IGM (in 2013 and 2015) highlight a more even split belief among economists on the effects of the minimum wage. This evolution in economic understanding of the benefits and pre-

viously overestimated cost of the minimum wage is the result of a shift in empirical findings, a pattern similarly occurring in the research regarding rent control policies.

Minimum wage research prior to the 1990s focused heavily on the neoclassical theory of price controls and time-series analyses, concluding that there existed significant negative employment effects to minimum wage increases, particularly for young and low-skilled groups (Brown, Gilroy, and Kohen 1982; Bernstein and Schmitt 1998). Though these findings appeared conclusive, research about the minimum wage generated in the 1990s, beginning largely with Card's use of differences-in-differences methodology rather than time-series analysis (1992), found that increases in the minimum wage had no statistically or substantively significant disemployment effects (Card 1992; Katz and Kreuger 1992; Card and Kreuger 1994). Since the emergence of a new minimum wage research, economists such as Bernstein and Schmitt (1998) have untangled the misleading findings of prior decades, noting, for example, that competitive models do not properly account for the complexities of the labor market and that early time-series analyses were fraught with econometric concerns (notably inadequate adjustments for seasonality and stationarity). The shift in the minimum wage literature and subsequently robust understanding of the employment and wage effects of minimum wage increases is pushing economists to rethink price controls generally and the validity of the standard supply and demand model³. This resurgence in the minimum wage literature may be of particular importance here given the recent resurgence in empirical and theoretical work highlighting the benefits of rent control and the failures of prior research to account for the complexities of the rental housing market.

The theoretical evaluation of rent control upon which the aforementioned IGM poll and a majority of economists' opposition to rent stabilization policies are founded depend on a perfectly competitive model of the rental housing market. Such perfectly competitive models operate largely off seven assumptions: 1) numerous tenants and landlords; 2) inability of a single sale to significantly affect the market at large; 3) no collusion or coordination among tenants or among landlords; 4) free entry or exit into

3. A standard supply and demand model is presented in chapter three.

the market; 5) perfect knowledge among both tenants and landlords; 6) homogeneous supply of rental units; and 7) a lack of artificial restrictions on demand or supply (Olsen 1969). Under these restrictive assumptions, rent control policies generate inefficiencies, notably: misallocation of housing units (Glaeser and Luttmer 2003; Thies 1993), decreased maintenance (Kutty 1996; Jenkins 2009), decreased housing availability (Frankena 1975; Mengle 1985; Jenkins 2009), and lower costs of non-controlled units through spillover effects (Arnott 1995). In short, “if the economy were perfectly competitive, any binding form of rent control would reduce social surplus” (Arnott 1995, 106).

The rental housing market, however, is not perfectly competitive (Arnott 1995; Gilderbloom and Appelbaum 1983, 1987). First and foremost, empirical work by Cronin (1983) and Mollenkopf and Pynoos (1973) suggests that rental housing markets are subject to the concentration of rental property in the hands of very few landlords and that these landlords may collude or gain monopoly power (Basu and Emerson 2000), thereby increasing their ability to set rent prices higher than in a perfectly competitive market. Other examples of potential imperfections in the rental housing market are “the non-contractibility of tenant maintenance, separation costs, matching frictions, and asymmetric information regarding tenant type” (Arnott and Shevyakhova 2008, 22). Even considering only homogeneity of housing units— in other words, the assumption that “most scholars would probably find to be the least plausible” (Olsen 1969, 229)— to be untrue, monopolistically competitive models of the rental housing market would thus have to be considered. Most analyses of rent control utilizing monopolistically competitive models of the rental housing market—which assume heterogeneity of rental units thereby providing landlords some market power, the presence of search costs (as prospective tenants seek out units which match their idiosyncratic tastes), and asymmetric information resulting from landlord’s knowledge of their market power—find that “moderate rent controls are beneficial, although stringent rent controls are harmful” (Arnott 1995, 107) as rent control policies may limit landlord market power and can help keep rents more affordable (Diamond 1984; Arnott and Igarashi 2000).

The theoretical findings resulting from a more holistic analysis of the rental housing market— notably the finding that rent control does indeed make rents more affordable (Arnott 1995)— are supported by much of the recent empirical literature on rent control. Pastor, Carter, and Abood (2018) conclude that affordability is improved in rent regulated units, especially for long-term tenants, and highlight that most studies— particularly work by Heskin, Levine, and Garrett (2000), Sims (2007), Autor, Palmer, and Pathak (2014)— have found that “rent regulations either slightly increase rental affordability in non-controlled units or have a modest effect on rents when controlling for other factors” (Pastor, Carter, & Abood 2018, 11).

Not only do rent control policies appear to render housing more affordable, they also contribute to greater neighborhood stability. Much of the prior research—particularly the studies by Clark and Heskin (1982), Gyourko and Linneman (1989), Ault, Jackson, and Saba (1994), and Diamond, McQuade, and Qian (2019)— concludes that second generation rent control policies reduce tenant mobility, thereby increasing “housing stability for rent-stabilized residents” (Pastor, Carter, and Abood 2018, 17). Traditionally, economists have portrayed such stability through the reduction in tenant mobility as an adverse effect of rent control, often claiming it results in misallocation of extant housing units (Glaeser and Luttmer 2003), but these analyses fail to consider the effects of forced displacement and evictions on individuals’ wellbeing. Evictions, the involuntary removal of a tenant by a landlord, have become somewhat commonplace in the United States, with 898,479 evictions in 2016⁴, and are intimately related to the rising rental housing costs (Desmond 2012). The impacts of these evictions and forced displacements on tenants are severe and long-lasting. Because low-income urban mothers are one of the groups most vulnerable to eviction in the United States, Desmond and Kimbro (2015) explored the impacts of eviction on these women. Most notably, “eviction results in multiple and multidimensional negative consequences for mothers” (2015, 22), with mothers who had been evicted in the past year experiencing higher rates of material hardships and parental stress,

4. This represents an eviction rate of 2.3% (i.e. approximately 2.3 households in every 100 are evicted).

as well as increased likelihood of depression and poor health. This analysis of the impacts of eviction is furthered by Pastor, Carter, and Abood in their 2018 report, as they highlight how these outcomes can spiral into further worsening consequences. For example, “research has shown a correlation between forced moves and stress (e.g., anxiety, depression, etc.) and well-being (e.g., substance abuse, premature mortality, etc.)” (Pastor, Carter, and Abood 2018, 18).

It is important to note that the impacts of eviction on wellbeing are just one of the pathways through which evictions can harm residents. The effects of displacement on educational attainment are particularly salient given that they will produce long-lasting effects, potentially disrupting children’s “learning and social support systems, which can dampen learning and exacerbate behavioral problems” (Pastor, Carter, and Abood 2018, 18), as evidenced by lower rates of high school graduation in individuals who moved frequently and the potential for impaired memory functioning as a result of chronic stress in childhood. Clearly, the downsides of eviction are substantive enough to merit consideration when discussing the overall net balance of the effects of increasing neighborhood stability.

Some economists may hold the traditional view that increased neighborhood stability through the lowering of tenant mobility reduces welfare, especially for newcomers to an area. However, considering the precedent set by some explicit US public policy goals (Pastor, Carter, and Abood 2018) and the deleterious effects of eviction and forced displacement, it is arguable that housing stability is preferable and actually increases social welfare.

The case presented above argues that rent control generally renders rental housing more affordable and increases desirable neighborhood stability. The literature remains divided, noting that while rent control may stabilize rents and neighborhoods, its efficacy and potential drawbacks are largely dependent on the assumptions made about the rental housing market. While a new theoretical consensus remains to be established, the empirical literature on rent control, particularly in the United States, is limited and thus has yet to provide much support to those in favor of rent control or to those who oppose such policies. Much of the prior empirical work evaluating rent

control's effects on rent prices has focused on the period of decontrol in Massachusetts (1994) or on larger Californian cities such as San Francisco, which have been subject to high levels of growth and gentrification, thus making it difficult to determine the effects of rent control itself. New Jersey may be a particularly interesting state to focus on as it allows municipalities to enact local rent control ordinances and, largely thanks to advocacy by the New Jersey Tenant's Organization, has a relatively large number of cities with some form of modest rent control.

Indeed, much of the more recent empirical evaluations have focused on New Jersey (Ambrosius et al. 2015; Gilderbloom and Markham 1996; Gilderbloom and Ye 2007). While some prior work focusing on New Jersey has found "a modest lowering of the rate of increase in rents" (Gilderbloom and Markham 1996, 425), most recently, Ambrosius et al.'s 2015 study, "Forty years of Rent Control: Reexamining New Jersey's Moderate Local Policies after the Great Recession," found that rent control policies did not have any statistically or substantively significant effects on median rents in New Jersey. Since, as in our more general view of rent control, a consensus remains to be determined regarding the effects of rent control on rent prices in New Jersey, studying the impacts of moderate rent control policies on rents in NJ municipalities may be of particular interest and is the focus of this thesis. I will begin with an extension of Ambrosius et al.'s work prior to evaluating methodologies better suited to studying price controls under imperfect conditions.

Chapter 2

Replication and Extension of Ambrosius et al.’s 2015 paper, “Forty Years of Rent Control: Reexamining New Jersey’s Moderate Local After the Great Recession”

Because much of the recent empirical work on rent control has focused on New Jersey, I have decided to focus on NJ’s municipality-specific ordinances. Ambrosius et al.’s 2015 paper sought to examine the effects of rent control policies, enacted in the 1970s by municipalities throughout New Jersey, on rent prices. Unlike a breadth of other empirical work evaluating the effects of rent control on rental costs, Ambrosius and colleagues found “a minimal—if any—impact of rent control on median rents in New Jersey cities” (2015, 129), though they noted “rent control communities—which were larger to begin with—have seen greater growths in population and lower vacancy rates over the past 10 years” (130). In other words, this study does not appear to support

the idea that rent control plays a significant role in maintaining manageable rent prices. Given these somewhat abnormal findings, this paper replicates and extends this 2015 study. There are several differences between Ambrosius et al.'s study and the following chapter¹. Most notably, I use an updated dataset rather than the 2003 New Jersey Tenants Organization's Rent Control Survey, do not include foreclosure rates and property value change as dependent variables, and define municipality limits using county-level subdivisions rather than Census Designated Places (CDP). I hypothesize that rent control regimes in New Jersey will contribute to significantly lower median monthly contract rents and rents per room².

2.1 Data Sources and Methods

2.1.1 Data Sources

All data was obtained from three primary sources: a dataset constructed in the style of the New Jersey Tenants Organization's 2003 Rent Control Survey, using city and municipality codes available online primarily at ecode.com, as well as the 2019 and 2010 American Community Surveys (ACS). The constructed dataset is henceforth referred to as the Rent Control Survey. This survey collected data on all cities in New Jersey with rent control ordinances, notably ordinance-specific measures such as which buildings are controlled, whether specific surcharges or vacancy decontrols are allowed, and the method for determining allowable increases. The determination of my sample differed slightly from Ambrosius et al.'s, largely because of the choice to use the Census County-level subdivisions rather than Census Designated Places (CDPs) to determine municipality limits; in keeping with Ambrosius et al., however, municipalities were only considered if they had a population of 10,000 or more individuals in 2019. Due primarily to the use of county-level subdivisions rather than CDPs, my sample includes 241 municipalities (as compared to Ambrosius et al.'s

1. Some of these differences are due to a lack of data. Though I reached out to Ambrosius, I did not hear back. The NJTO did however provide me their 2003 survey.

2. Rooms here refers to all rooms in the unit, rather than just bedrooms.

161).

2.1.2 Independent Variables

This study utilized two independent variables: a nominal-level variable and an ordinal-level variable measuring rent control. These variables were constructed using the Rent Control Survey. Any city with a form of rent control, regardless of its strength, scored a one on the nominal level rent control variable; any city not included in the survey, since it collected data on all cities in NJ with rent control ordinances, or for which no rent control was reported, was assigned a zero on the nominal level rent control variable. Cities without rent control similarly scored a zero on the ordinal-level rent control variable. This ordinal variable measures the strength of a given ordinance, with the strictest possible ordinance scoring a seven and a city without rent control a zero. Cities reporting a rent control policy were assigned scores for a variety of ordinance-specific measures, such as whether increases in rent were allowed due to capital improvements, tax surcharges, and/or demonstrated hardship. The preemption by a city of tax surcharges, increases in rent prices due to capital improvement, demonstrated hardship/fair return clauses, decontrol following vacancy, fuel pass-along clauses, the exclusion of new tenants, and increases due to a tenant's return on tax appeals each increased a city's ordinal level rent control score by one point. Unlike in the Ambrosius et al. study, this ordinal variable does not separate hardship surcharge allowances and fair return clauses, as these are often interchangeable, and does not take into account differences in exempted units due to owner-occupation or dwelling size. Cities for which rent control only applies to mobile homes³ are considered as not having rent control for both the ordinal-level and nominal-level variable.

In the sample I constructed 90 out of 241 municipalities are coded as having some form of rent control; a majority of these municipalities had a moderate form of rent

3. The municipalities with rent control which applies only to mobile homes parks are Shamong, Pittsgrove, Middle, Freehold, Marlboro, Wall, Howell, Barnegat, and Hampton Townships, and Franklin Borough.

control, with nearly 57% of these ordinances scoring a three or below on our seven-point scale⁴. Only one city, Woodbridge Township, scored a one; similarly, only two cities scored a six and a seven, Ewing Township and Lawrence Township respectively. The following maps (Figure 2.1) highlight the location of municipalities with rent control ordinances and their relative strengths.

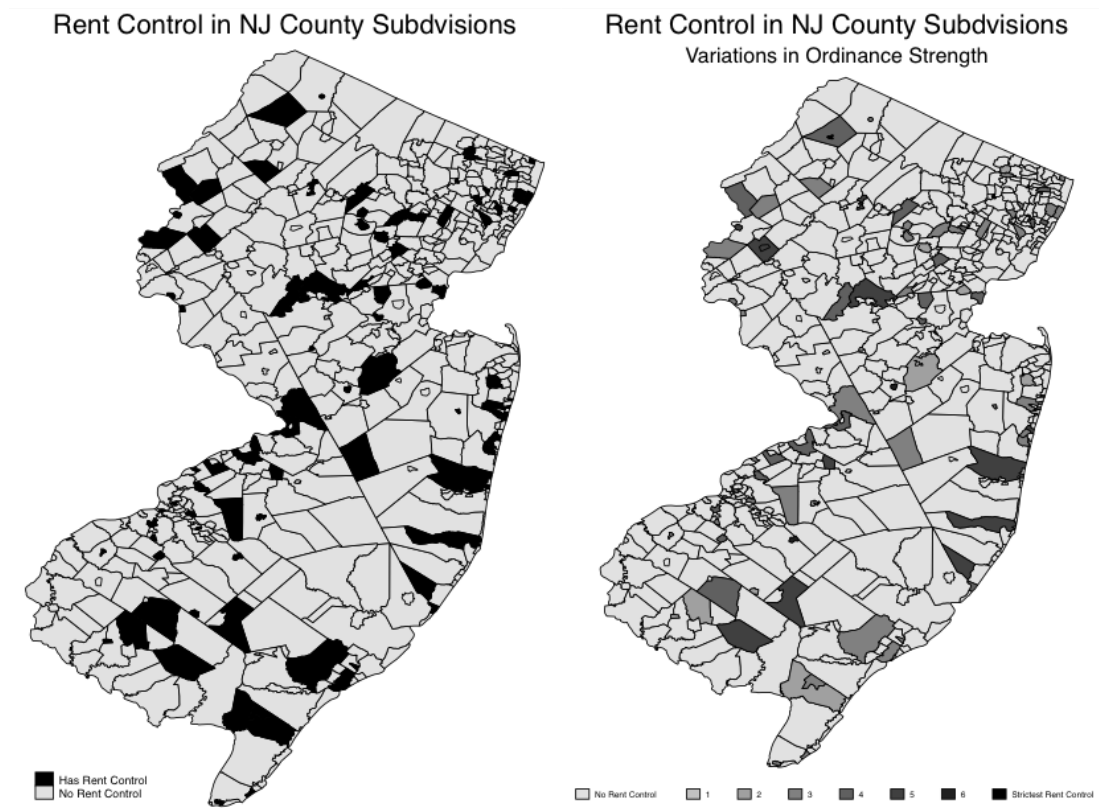


Figure 2.1: Distribution of Rent Control Ordinances in NJ

2.1.3 Dependent Variables

All five dependent variables—median monthly contract rent, number of rooms in renter-occupied housing, median monthly contract rent per room, percent of rental units with plumbing deficiencies, and the share of units built since 2010 (henceforth referred to as “new construction”)—were sourced or constructed from the 2019

4. 40% of the ordinances with some rent control policy score exactly a three on this scale.

American Community Survey (ACS). Though the Ambrosius et al. (2015) study also examined two additional dependent variables, property value change and foreclosure rate as a proxy for abandonment, these have been omitted from my study because of the limited availability of this data and the inapplicability of foreclosure rates for serving as proxy for neighborhood instability.

2.1.4 Control Variables

To ensure I am controlling for other, potentially explanatory variables, the model accounts for seven additional variables: municipal population, as increased city size is positively related with amenities and thus higher rents; population growth from 2010 to 2019, to control for changes in rents as a result of demand-side shifts; race⁵, to control for racial discrimination within the market; the median household income (in inflation-adjusted dollars), as rents are largely set in response to resident incomes; the proportion of total housing units which are renter-occupied, to account for the share of the housing market being affected by rent control; rental vacancy rates, to control for supply-side shortages and/or surpluses; and lastly, share of rental units constructed prior to 1940, as older housing stock is likely to have more maintenance issues and lower rents (Ambrosius et al. 2015). All seven variables were constructed primarily using data from the 2019 ACS, though the 2010 ACS was used to determine population growth.

2.1.5 Regression Modeling

The following OLS regression is the basis for the analysis:

$$y = \beta_0 + \beta_1x_1 + \beta_2x_2 + \beta_3x_3 + \beta_4x_4 + \beta_5x_5 + \beta_6x_6 + \beta_7x_7 + \beta_8x_8 + \epsilon \quad (2.1)$$

where y = Dependent Variable

x_1 = Rent Control

5. The race variable is coded as the percentage of the municipal population identifying as Black or African-American alone (ACS 2019).

$x_2 =$ Total Population

$x_3 =$ Population Growth

$x_4 =$ Percent of Black Renters

$x_5 =$ Median Household Income

$x_6 =$ Proportion of Units which are Renter Occupied

$x_7 =$ Rental Vacancy Rate

$x_8 =$ Proportion of Units Built Prior to 1940

$\epsilon =$ Error Term

The independent variables (nominal and ordinal rent control variables) are rotated into these equations.

2.2 Descriptive Statistics

Descriptive statistics and differences of means between rent control and non-rent control cities are presented in Table 2.1, highlighting several statistically significant differences between these municipalities.

Table 2.1: Descriptive Statistics and Differences of Means Test for Rent Control and Non Rent Control Cities, 2019

Variables	Non Rent Control Cities		Rent Control Cities		T-Test (2-Tailed)
	Mean	Std. Dev.	Mean	Std. Dev.	
Median Monthly Rent	1,375.07	339.91	1,311.04	252.11	1.55
Number of Rooms	4.39	0.62	3.98	0.34	5.81***
Median Rent Per Room	317.01	81.10	333.59	79.04	-1.55
New Construction (%)	5.08	6.41	4.62	4.59	0.59
Units w/out Plumbing (%)	0.41	0.87	0.41	0.58	-0.06
Total Population (2019)	22,764.75	13,241.38	44,449.61	44,452.11	-5.60***
Population Growth (%)	1.86	6.02	2.90	4.24	-1.45
Median Household Income	105,782.20	38,154.42	84,310.74	27,567.20	4.66***
Black (%)	8.16	10.86	14.65	17.16	-3.59***
Renter-occupied Units (%)	24.75	15.48	44.33	19.44	-8.61***
Units built prior to 1940 (%)	13.14	12.21	17.75	11.57	-2.89***
Vacancy Rate (%)	1.33	1.51	2.04	1.47	-3.57***

Source: Author's Calculations using 2019 and 2010 ACS

While median monthly contract rent is approximately \$64 greater in non-rent

control cities, this difference is not statistically significant. Median rent per room is approximately \$17 greater in cities with rent control; this difference is statistically insignificant and may be attributable to the statistically significant difference in the mean number of rooms between cities with and without rent control.

Rent control and non-rent control cities are comparable in their shares of units built after 2010, their proportions of units lacking plumbing⁶, and population growth. Notably, however, rent control cities exhibit statistically and substantively greater population sizes, shares of Black residents, shares of renter-occupied units, percent of units built prior to 1940, vacancy rates, and lower median household income than non-rent control cities. As noted by Ambrosius and colleagues, “in sum, rent control communities exhibit more ‘urban’ characteristics than the non-rent control communities in the sample” (2015, 125), highlighting the need to control for the aforementioned characteristics. Similarly, since cities with rent control and those without are not comparable across most metrics, it may be difficult, if not unattainable, to obtain conclusive findings with an OLS regression.

2.3 Main Findings

Regression results are presented in Tables 2.2 and 2.3 below. Overall, the models predicting median monthly contract rent and median rent per room explain nearly 60% of the variation in our dependent variables. The models predicting number of rooms and new construction are weaker predictors, explaining approximately a quarter of the variation in these variables. Lastly, the model predicting plumbing deficiency fares much worse, failing to explain even 1% of the variation in this variable.

Though rent control does not have a statistically significant impact on median rents, both the nominal and the ordinal level rent control variables exhibit a statistically significant, positive relationship with median rent per room and a statistically significant, negative relationship with the number of rooms. As hypothesized before,

6. The shares of units lacking plumbing is included to ensure maintenance is upheld in rental units. Though negligible shares of units lack plumbing and this measure is largely outdated, plumbing condition is standardly used to measure housing quality.

Table 2.2: Impact of rent control (nominal) on rental housing characteristics in 2019 (n = 241)

	Median Rent	Number of Rooms	Rent per Room	Plumbing deficiency (%)	New Construction (%)
Rent Control, Nominal	18.09 (30.95)	-0.21*** (0.08)	21.93*** (8.29)	-0.12 (0.12)	-0.12 (0.78)
Total Population (2019)	-0.000272 (0.000457)	0.00000127 (0.00000113)	-0.0002* (0.0001)	0.000000183 (0.00000179)	-0.00000915 (0.0000115)
Population Growth (%)	6.39*** (2.45)	0.01 (0.01)	1.06 (0.66)	-0.01 (0.01)	0.56*** (0.06)
Median Household Income	0.007*** (0.0005)	-0.000003** (0.000001)	0.002*** (0.0001)	-0.000001 (0.000002)	-0.00001 (0.00001)
Black (%)	-1.67 (1.07)	0.003 (0.003)	-0.66** (0.29)	0.01 (0.004)	0.035 (0.03)
Renter-occupied Units (%)	5.79*** (1.22)	-0.02*** (0.003)	2.60*** (0.33)	0.004 (0.005)	-0.085*** (0.03)
Units built prior to 1940 (%)	-4.71*** (1.18)	0.01*** (0.003)	-1.64*** (0.32)	0.002 (0.005)	-0.03 (0.03)
Vacancy Rate (%)	-6.17 (11.03)	-0.03 (0.03)	1.94 (2.95)	-0.025 (0.04)	0.88*** (0.28)
Constant	525.4*** (69.5)	5.01*** (0.17)	71.37*** (18.62)	0.39 (0.27)	6.24*** (1.75)
R-squared	0.6155	0.31	0.59	0.04	0.30
Adjusted R-squared	0.60	0.28	0.57	0.01	0.2747
F	46.42***	12.85***	41.50***	1.27	12.36***

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Source: Author's Calculations using 2019 and 2010 ACS, and Updated Rent Control Survey Dataset

Table 2.3: Impact of rent control (ordinal) on rental housing characteristics in 2019 (n = 241)

	Median Rent	Number of Rooms	Rent per Room	Plumbing deficiency (%)	New Construction (%)
Rent Control, Ordinal	1.2 (8.03)	-0.05** (0.02)	4.22* (2.16)	-0.01 (0.03)	-0.006 (0.20)
Total Population (2019)	-0.000226 (0.000452)	0.000001 (0.000001)	-0.0002 (0.0001)	-0.0000001 (0.000002)	-0.00001 (0.00001)
Population Growth (%)	6.44*** (2.45)	0.01 (0.01)	1.11* (0.66)	-0.01 (0.01)	0.56*** (0.06)
Median Household Income	0.0074*** (0.000489)	-0.000003** (0.000001)	0.002*** (0.0001)	-0.000001 (0.000002)	-0.00001 (0.00001)
Black (%)	-1.67 (1.069)	0.003 (0.003)	-0.68** (0.29)	0.01 (0.004)	0.035 (0.03)
Renter-occupied Units (%)	5.975*** (1.21)	-0.02*** (0.003)	2.69*** (0.33)	0.003 (0.005)	-0.086*** (0.03)
Units built prior to 1940 (%)	-4.72*** (1.18)	0.01*** (0.003)	-1.63*** (0.32)	0.002 (0.005)	-0.03 (0.03)
Vacancy Rate (%)	-6.81 (11.05)	-0.025 (0.03)	1.70 (2.98)	-0.02 (0.04)	0.88*** (0.28)
Constant	523.24*** (69.47)	5.03*** (0.17)	69.74*** (18.72)	0.405 (0.27)	6.25*** (1.745)
R-squared	0.61	0.30	0.58	0.04	0.30
Adjusted R-squared	0.60	0.28	0.57	0.005	0.27
F	46.31***	12.60***	40.56***	1.17	12.36***

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Source: Author's Calculations using 2019 and 2010 ACS, and Updated Rent Control Survey Dataset

we would expect rent control to decrease median rent per room. However, given that rent control cities tend to exhibit more urban characteristics and that the effects presented above are somewhat substantively insignificant, these findings are not particularly surprising.

Consider first the effect of simply having a rent control ordinance. The presence of such an ordinance may decrease the number of rooms per unit by less than a single room and increase monthly rent per room by approximately \$22. Since rents per room range from as low as approximately \$169 to as high as approximately \$690, this effect is largely substantively insignificant. When considering the effect of increasing strength of a given ordinance, a shift from having no rent control (i.e. where the 10th percentile of cities are) to having a moderately strong rent control policy increases rents per room by only approximately \$12 and may decrease the number of rooms per unit by less than a single room. It is possible that the potentially large variations in unit size and quality not captured here may be responsible for differences in the rent per room.

Similarly, the statistically significant effects of population growth since 2010 on median monthly contract rents are largely substantively insignificant, capable of generating only a \$63 shift in rents across its lower and upper percentiles. Comparably, race, as proxied by the percent of renters identifying as Black or African-American⁷, and municipal population in 2019 have statistically significant though largely substantively unimportant effects on rents per room.

More substantively important for median monthly contract rents, regardless of the independent variable considered, are the statistically significant effects of median household income, the percent of units that are renter-occupied, and the share of units built prior to 1940.

In both regressions, a \$1 increase in median household income is predicted to increase median rents by approximately \$0.007 and median rents per room by approximately \$0.002. More importantly, however, holding all other factors constant, a

7. Though these terms are not exactly synonymous, the 2019 ACS combines African-American and Black into one racial category.

city with a median household income of \$57,918—a city whose median income was at the 10th percentile—is predicted to have a median rent approximately \$638 lower and a rent per room approximately \$182 lower than that of a city with a median household income of \$149,129—i.e. a city whose median income was at the 90th percentile.⁸ These effects are clearly very substantively significant.

In both regressions, the percentage of total units in a given city that are occupied by renters has a substantively and statistically significant effect on median rents and median rent per room. A one percent increase in the proportion of total units occupied by renters is predicted to increase median rents by approximately \$6 and rents per room by approximately \$2.65, regardless of which independent variable is used. These effects are quite substantively significant, as evidenced by the fact that, holding all other factors constant, a city with 10.15% of total units being renter occupied—i.e. a city whose percentage of total units occupied by renters was at the 10th percentile—is predicted to have a median rent approximately \$306 lower and a rent per room approximately \$135 lower than that of a city with 61.11% of total units being renter occupied—i.e. a city whose percentage of total units occupied by renters was at the 90th percentile.

Lastly, we must consider that a one percent increase in the share of units built prior to 1940 is predicted to decrease median rents by approximately \$4.7 and rent per room by approximately \$1.64, regardless of which independent variable is used. Moving from this variable’s lower to upper percentile may generate a nearly \$155 decrease in median rents and an approximately \$47 decrease in median rent per room, indicating its substantive significance.

In sum, median household income has the most substantively significant effect on both median monthly contract rent and rent per room, followed by share of units that are renter-occupied and, lastly, percent of units built prior to 1940. Rent control has no statistically significant impact on median rents, the share of units with a

8. The fact that rents are greater in higher income cities may be attributable to differences in the quality of the units being rented, notably given that recent growth in cities has led to a greater demand for luxury or higher-end rental units (Wharton 2018; JCHS 2019). This rent difference may also be attributable to better amenities in higher-income cities, such as better schools, parks, and air quality.

plumbing deficiency, or the percent of units which are newly constructed; its effects on the number of rooms per unit and rent per room are statistically significant, though substantively negligible. Greater household income increases the median monthly contract rent and rent per room. The percentage of units built prior to 1940, however, drives down median rents and rents per room, though increasing unit size (as measured by number of rooms). Inversely, the percentage of renter-occupied units increases median rents and rents per room, though driving down unit size (as measured by number of rooms). Though substantively insignificant, population growth exhibits a statistically significant, positive relationship with median rents, as does total population with rents per room, while race exhibits a statistically significant, negative relationship with rents per room.

2.4 Discussion

In accordance with Ambrosius et al.'s 2015 findings, rent control does not appear to have any substantively or statistically significant impacts on median monthly contract rents, percent of units with a plumbing deficiency, or share of units built within the last decade. Regressions run using the updated data, however, highlight that rent control—both its presence and its strength—has a statistically significant effect on the number of rooms and rents per room. Though these effects are counter to my hypothesis, they are largely substantively insignificant, as the presence of rent control increases rents per room by only approximately \$21 (or four percent) and decreases the number of rooms by less than one; similarly, holding all other factors constant, a city without rent control is predicted to have a rent per room approximately only \$13 lower than the rent of a city with a rent control score of three (i.e. a city with a rent control policy strength at the 75th percentile).

Most differences in rents between rent control and non-rent control cities appears to be due to other demographic or housing characteristics, notably median household income, percent of units which are renter-occupied, age of the housing stock, and race. Because these variables are likely linked to rent control, as increased pro-

tections for tenants may increase the percentage of renters, the proportion of lower-income households, and the share of Black individuals in a city, the conclusiveness of the aforementioned results is hard to establish and it is unclear whether rent control is helping maintain lower rents given that cities with rent control exhibit more “urban” characteristics (i.e. characteristics which are typically viewed as increasing rent prices).

Recent research on a similarly functioning price control, the minimum wage, has highlighted the incapacity of OLS regressions to accommodate for the aforementioned spatial heterogeneity problem. To better understand the drawbacks and limitations of using OLS regressions for evaluating the effects of rent control on rent prices, the minimum wage debate should be evaluated. Most importantly, since Card and Krueger’s seminal 1994 paper on minimum wage effects, “Minimum Wages and Unemployment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania,” a majority of rigorous minimum wage studies have utilized some form of matching. Though some economists, notably Neumark, Salas, and Wascher (2013), have continued to disagree about the best econometric approach to evaluating the effects of price controls, Kuehn (2014) argues that “the critical advance has been that Neumark, Salas, and Wascher (2013) appear to concede that some sort of modern matching approaches are essential for evaluating the effect of minimum-wage increases in the absence of a randomized experiment.”

Such a shift in the methods used to determine minimum wage effects, especially considering that “the strongest studies that use matching strategies find little or no evidence that such increases have a negative impact on employment” (Kuehn 2014), highlight the need to use matching to determine the effects of rent control on rents. A difference-in-differences model is presented in the following chapter.

Chapter 3

Lessons Learned from the Minimum Wage: Using Difference-in-Differences Methodology to Understand Rent Control's Effects

In the previous chapter, I demonstrated that rent control policies, regardless of their strength, do not exhibit any statistically significant effects on rent prices in the case of New Jersey. There are reasons however, notably the theoretical implications of a monopolistically competitive rental housing market and concerns of spatial heterogeneity, to believe that the methods used (OLS) poorly capture the impacts of rent control on rent prices.

Recently, major shifts in empirical economic research have provided new insights into the effects of traditionally frowned upon price controls. Most importantly, the minimum wage revolution—largely caused by a shift in methodology from OLS to techniques utilizing matching—has led to a deeper understanding of the lack of effects of the minimum wage on unemployment. Because the minimum wage and rent

control are both price controls in imperfect market conditions and work in similar, albeit opposite, ways, the minimum wage debate and its resulting importance for methodological design will be used to highlight the need for a difference-in-differences model to better appreciate rent control's effects.

This chapter will begin by highlighting similarities in the neoclassical understandings of price controls, specifically of the minimum wage and rent control, before delving into the revolution in the minimum wage debate. A model which could be used to better understand the impacts of rent control ordinances on rent prices in New Jersey will then be presented. Data limitations preventing the model from being run will lastly be explored, but an explanation of what data would be required to run the model will be presented.

3.1 Understanding Rent Control Through the Minimum Wage Literature: Lessons Learned

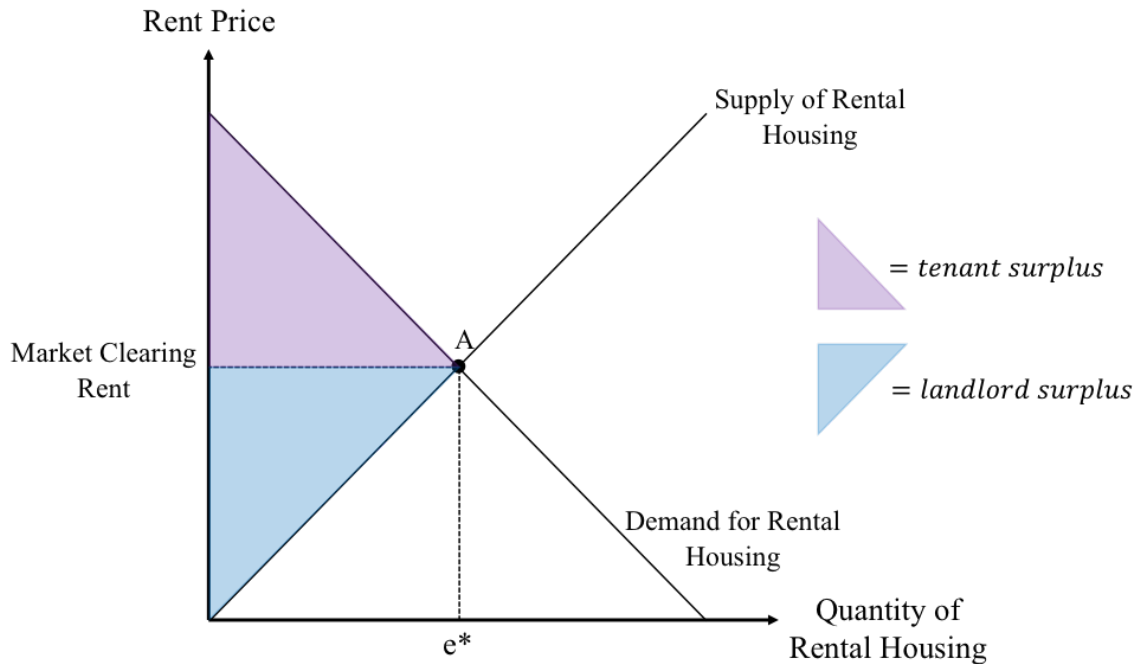
3.1.1 Validity of Comparisons between Minimum Wage and Rent Control Research

Albeit functioning oppositely, both the minimum wage and rent control are often thought of as textbook examples of price controls. Price controls are a policy imposed upon markets, setting a maximum or a minimum price for a specific good. The two major categories of price controls are price floors, which set a minimum price for a given good, and price ceilings, which set a maximum price. Most economists generally oppose price controls, citing the distortion of resource allocation as the major problem with these policies. To better understand what economists really mean by “distortion of resource allocation,” consider the following graphs (Figures 3.1 and 3.2 respectively).

Assume a perfectly competitive rental housing market. Note that the demand curve is downward sloping due to four major reasons: the law of diminishing marginal returns, the substitution effect, the income effect, and increases in the quantity de-

manded as a result of lower prices. The supply is upward sloping, largely due to land constraints: constructing more rental housing units on a given land stock is characterized by increasing marginal costs, thus requiring that as suppliers generate more, they must also earn more (set their price higher) to overcome rising marginal costs.

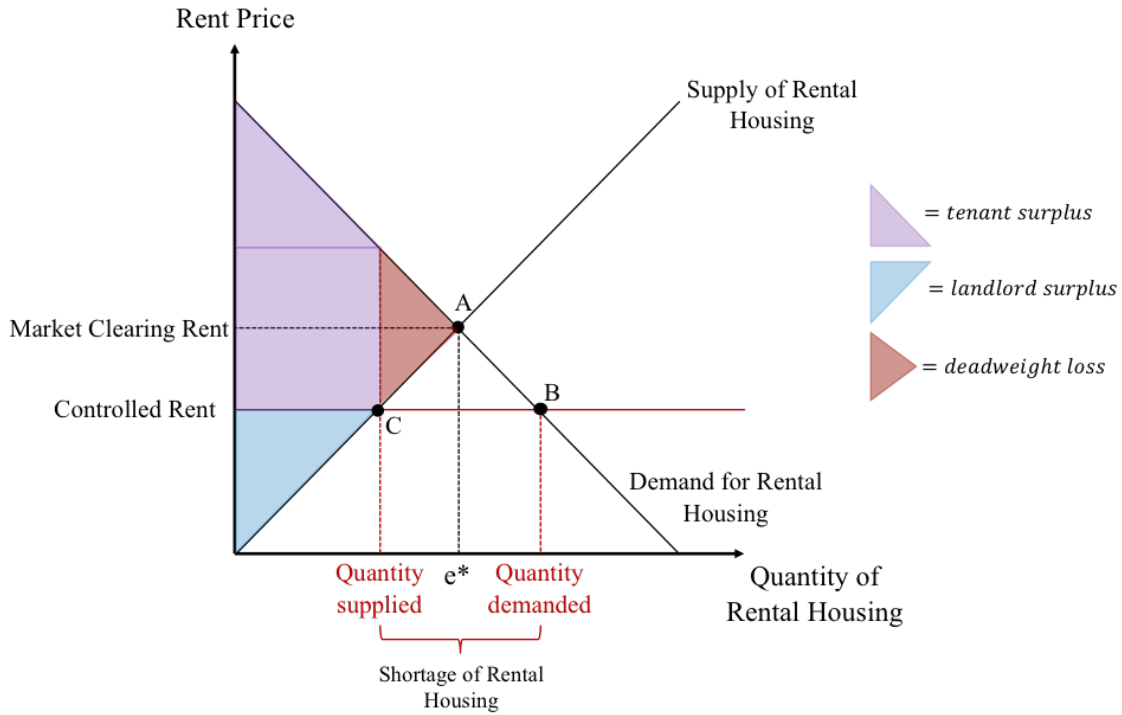
Figure 3.1: Rental Housing Market without Rent Control



As evidenced in Figure 3.1, when rent control is not enacted, i.e. when there is no price ceiling on rents, the market clears at equilibrium point A. The tenant surplus is indicated by the purple shaded triangle, whereas the landlord surplus is indicated by the blue-shaded triangle.

If we assume the price ceiling to be binding (i.e. the controlled rent is lower than the previously established market clearing rent), the market no longer clears. The quantity of rental housing demanded increases, sliding down the demand curve to point B. On the other hand, the quantity supplied of rental housing falls, shifting down the supply curve to point C. This results in a shortage of rental housing, as the quantity demanded outweighs the quantity supplied. We can also see a shift in the total surplus: there is some deadweight loss, indicated by the red-shaded triangle, and the landlord surplus clearly decreases. In short, all other things held equal, the

Figure 3.2: Rental Housing Market with Rent Control

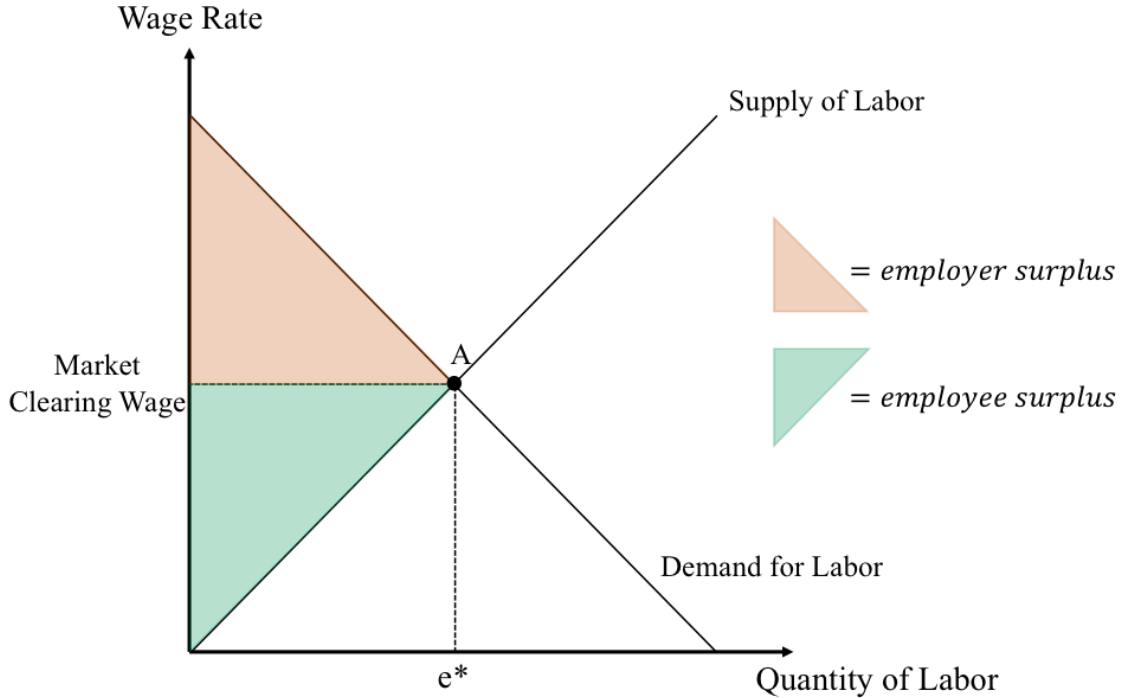


neoclassical understanding of rent control notes that rent control would lead to a mismatch in the quantities of rental units demanded and supplied, thus resulting in shortage of occupiable rental housing. However, individuals who manage to get rental housing would benefit from lower rent prices.

A similar distortion of resource allocation was believed to result from increases in the minimum wage, an often cited “textbook example” of a price floor. Assume a perfectly competitive labor market. As above, the market is characterized by a downward-sloping demand curve and an upward-sloping supply curve.

If we assume the price floor to be binding (i.e. the minimum wage is greater than the previously established market clearing wage rate), the market no longer clears. The quantity supplied of labor increases, shifting up the supply curve to point B. On the other hand, the quantity demanded of labor decreases, shifting up the demand curve to point C. This results in a surplus of labor, as the quantity supplied outweighs the quantity demanded, and a lower level of employment. We can also see a shift in the total surplus: there is some deadweight loss, indicated by the red-shaded

Figure 3.3: Labor Market without a Minimum Wage

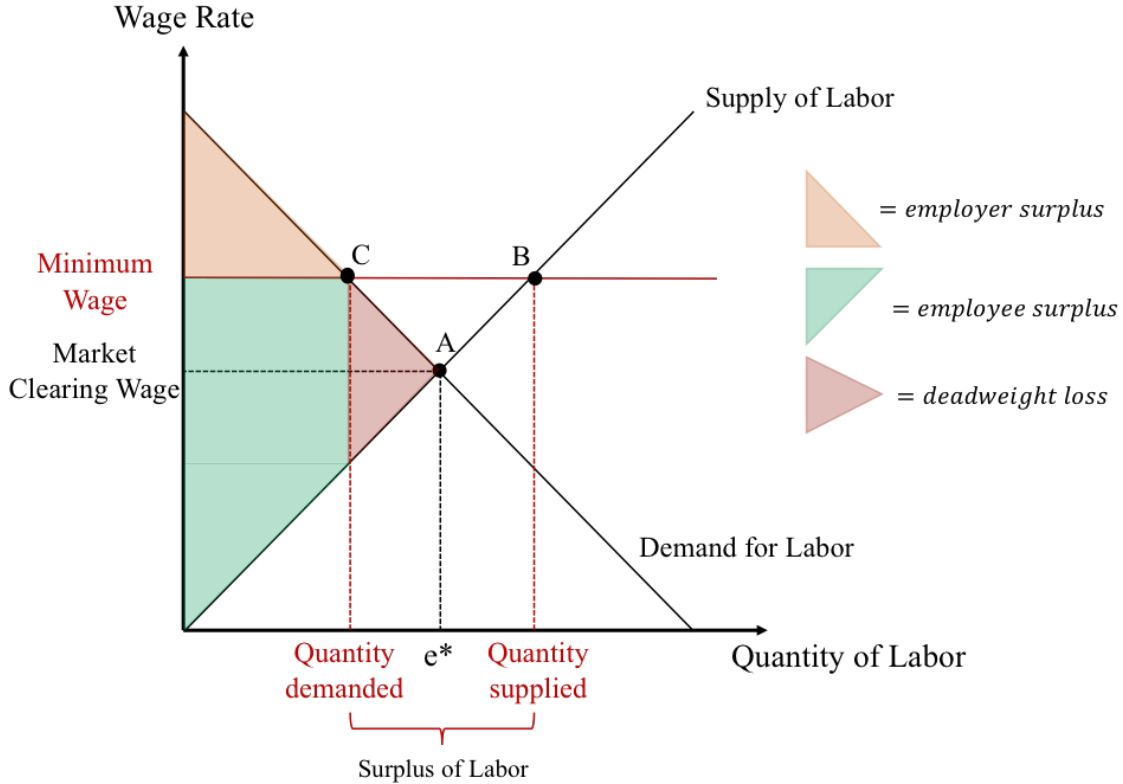


triangle, and the employer surplus clearly decreases. All other things held equal, the neoclassical understanding of the minimum wage notes that a minimum wage would lead to a mismatch in the quantity of labor demanded and supplied, thus resulting in a labor surplus. Though individuals who manage to get or retain their jobs would benefit from higher wages, *ceteris paribus*, unemployment would rise.

Economists have challenged these “textbook analyses” of price controls for both the minimum wage and rent control, largely because of the imperfection conditions in the labor and rental housing markets. The figures and analyses presented above depend on the labor and rental housing markets being perfectly competitive. There exists, however, theoretical and empirical evidence for market imperfections in both of these markets which can affect our understanding, and the real world outcomes, of the minimum wage and rent control.

Consider first the labor market. There are two major theoretical reasons the labor market may be monopsonistic. First, and generally accepted, workers may have relatively inelastic labor supply curves to the firm at which they work, because

Figure 3.4: Labor Market with a Minimum Wage



searching for another job is time-consuming and costly. Thus, employers can pay these workers less than their marginal product without losing them to a firm which would pay the worker’s full marginal product. The second reason to believe the labor market may be monopsonistic is the degree of market concentration. The framework developed by Azar, Marinescu, and Steinbaum argues that “firms pay higher wages if the labor market is unconcentrated and workers can expect abundant job offers from competing employers” (2020, 5). Figure 3.5 depicts the monopsonistic labor market.

Under these market conditions, rather than being at the competitive equilibrium labeled Point B, with wages at W_1 and employment at Q_1 , there are less workers employed (Q_2) and those employed have a lower wage (W_2).

If a minimum wage were implemented in such a market, say a minimum wage at the level of W_1 , both the wage rate and the quantity of labor employed would increase without loss of efficiency or a misallocation of resources. Such a shift is depicted in Figure 3.6.

Figure 3.5: Monopsonistic Labor Market

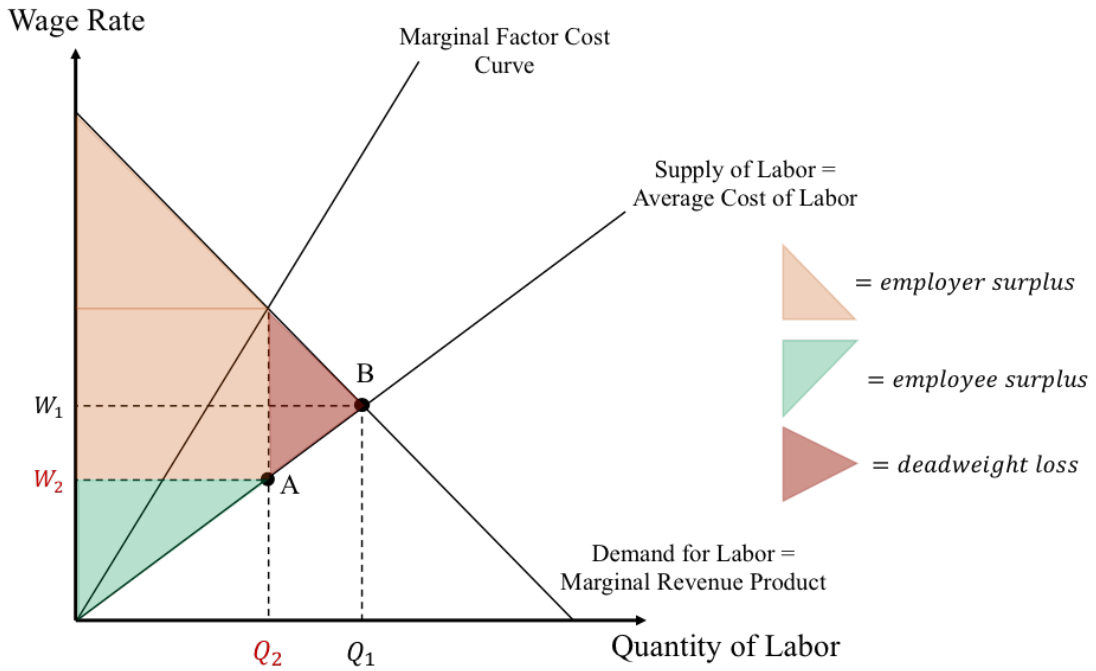
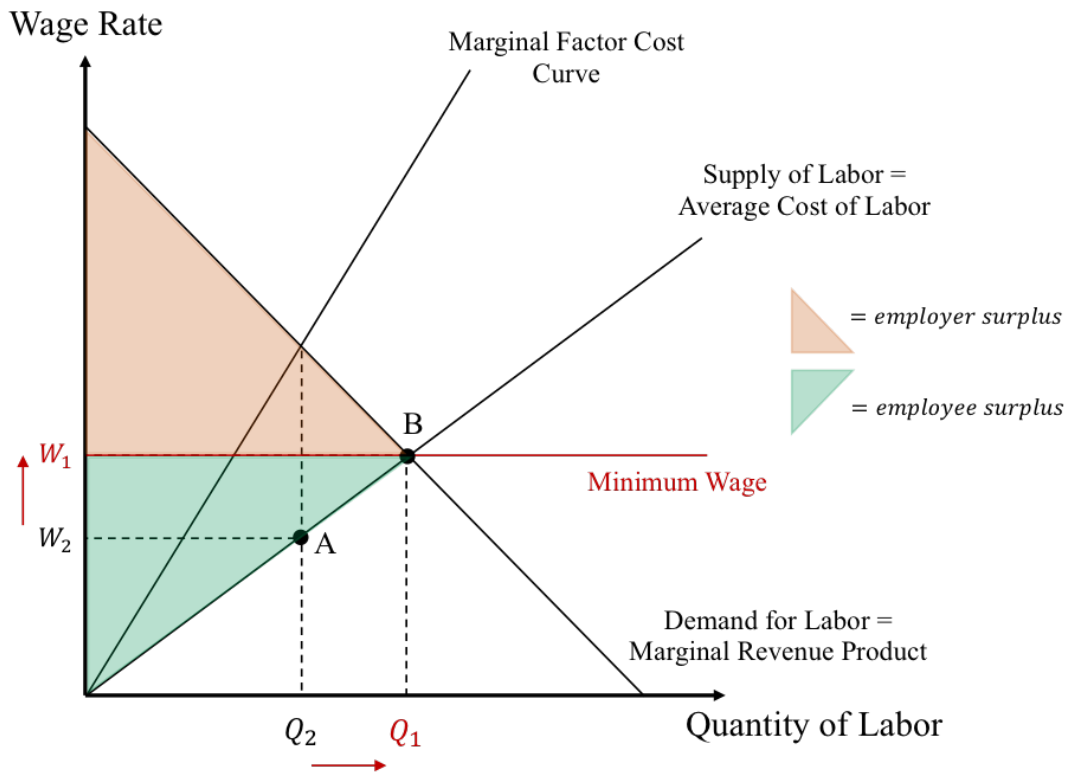


Figure 3.6: Monopsonistic Labor Market with Minimum Wage



Similarly, some economists (notably, Arnott (1995)), have argued that there are two major ways to model an imperfect rental housing market: monopolistically competitive models and contract models. I will focus on monopolistically competitive models, since contract models rely heavily on the assumption that some tenants are “bad,” an inherently flawed assumption.

Igarashi and Arnott (2000) argue that, due to idiosyncratic tastes and differentiated products, tenants must search for a unit which is a good fit. Similarly to the case with labor markets, such searching can be costly and time-consuming, thus providing landlords the ability to price their units above cost. In this model, “the market power effect dominates the other effects [...] so that the natural vacancy rate is higher than the (constrained) socially optimal vacancy rate” (Arnott and Igarashi 2000, 251). The mismatch costs typically associated with rent control, which are in reality a lowering of the vacancy rate, can be beneficial as “the uncontrolled equilibrium vacancy rate is inefficiently high [and] a reduction in rent below its uncontrolled equilibrium level is welfare-improving” (Arnott and Igarashi 2000, 277).

As evidenced by the above, there are theoretical justifications for reconsidering the methodological validity of studies which find disemployment effects as a result of minimum wage increases. Such theoretical justifications were indeed used as the background for a new wave of minimum wage research beginning in 1990. This minimum wage revolution, particularly given the similarities between the labor and rental housing markets and the mechanisms of the minimum wage and of rent control, can be used to orient research about rent control to more appropriate methodology.

The following section explores the minimum wage revolution in more depth, highlighting the switch in empirical findings and the methodology used.

3.1.2 Lessons Learned from the Minimum Wage Revolution

In decades prior to 1990, the minimum wage research seemed to support the neo-classical understanding presented above (Brown, Gilroy, and Kohen 1982; Neumark 2018) but, following a resurgence in research surrounding the minimum wage and the use of different econometric methods, such a consensus among economists was some-

what overturned. As explored by Card and Krueger in their pivotal book, *Myth and Measurement: The New Economics of the Minimum Wage* (1995), several studies beginning with Card's 1992 study of the effects of the 1990 increase in the federal minimum wage on high school workers in New Jersey find little to no disemployment effects from increases in the minimum wage (Card 1992; Card and Krueger 1994; Schmitt 2013). Further noted, and of particular importance for this research however, is the reason behind this shift in the understanding of minimum wage's effects on employment: changes to the econometric models used to study these effects.

First and foremost, a majority of studies finding disemployment effects as a result of increases in the minimum wage are based on aggregate time-series analyses, but such approaches lack a clear counterfactual and can suffer from an endogeneity problem as they fail to account for the potential that governments may choose to change the minimum wage as a result of economic patterns (Card and Krueger 1994). Further, and similarly concerning for the consensus on rent control's effects, "a 'meta-analysis' suggests that the published time-series studies have been affected by "publication bias" or "specification searching," leading to a tendency toward finding statistically significant effects of the minimum wage" (Card and Krueger 1995, 177). The above findings highlight the need for careful decision-making regarding the methodology used to study the effects of price controls, particularly considering that publication bias tends to impact findings which support neoclassical economic theory (Card and Krueger 1995).

The need for careful decision-making regarding econometric modeling is insufficient to argue the inappropriateness of linear regression for evaluating the effects of rent control. The arguments presented by Card and Krueger in "Evaluation of Cross-Section and Panel-Data Evidence" (1995), however, are even more directly in support of the argument that linear regression is insufficient for understanding the effects of rent control ordinances on rent prices. Card and Krueger's primary argument rests upon the importance of controlling for spatial heterogeneity resulting from minimum wage policies, potentially confounding variables, and likely unobservable confounds clustering locally. In short, there exist differences between low and high minimum

wage states which cannot be teased out from these wage policies. As noted in the previous chapter, New Jersey cities with rent control have, on average, a greater percentage of black renters, a higher share of units which are renter-occupied, and a lower median household income. It is unclear how these variables are affected by rent control, thus generating a similar spatial heterogeneity problem when evaluating the effects of rent control policies on median rents. The most effective methods for controlling for such heterogeneity, unless spillover effects are of great importance, are with some form of matching: border discontinuity control and/or synthetic control approaches (Card and Krueger 1994).

As evidenced by the above, and as stated by Kuehn (2014), “a matching design is strongly preferred by economists [...] because it is often the closest study design to randomized experiments available”. A majority of the minimum wage studies utilizing matching, i.e. the preferred methodological design, find little to no disemployment effects as a result of increases in the minimum wage. The contradiction of these increasingly valid findings with prior research pushes me to believe that prior studies evaluating the effects of rent control, a majority of which do not use matching, are insufficient for economists to conclude that rent control has no statistically significant effect on rent prices. To best develop a more credible rent control study design, the seminal minimum wage papers utilizing matching, particularly difference-in-differences approaches, should be evaluated.

3.1.3 Difference-in-Differences in the Minimum Wage Literature

Due to the minimum wage’s ambiguous theoretical effect on employment, depending on the assumption of a perfectly competitive or a monopsonistic labor market, the ideal study to evaluate the direction and size of the employment effects of the minimum wage would randomly assign various labor markets to two main, identical groups. The control group would experience no change in the minimum wage, whereas the treatment group would see an increase in the minimum wage. Because such an

experiment cannot be conducted, economists turn to the next best option: research design utilizing matching. Thankfully, differences in policy design across states has generated natural experiments, though treatments have not been randomly assigned. Card and Krueger’s seminal 1994 paper, “Minimum Wages and Employment: A Case Study of the Fast Food Industry in New Jersey and Pennsylvania,” revolutionized the minimum wage literature using such a natural experiment and a difference-in-differences approach. In April of 1992, New Jersey raised its minimum wage from \$4.25 to \$5.05, whereas Pennsylvania’s minimum wage remained at \$4.25. Card and Krueger surveyed 473 fast food restaurants across New Jersey and Pennsylvania, once prior to the increase (February 1992) and once following the implementation of the policy (November 1992). They then ran a difference-in-differences to evaluate whether full-time employment (FTE) had been significantly impacted, as summarized by the following equation:

$$\widehat{\delta_{NJ,PA}^{2X2}} = (\mathbf{E}[Y_{NJ}^1|POST] - E[Y_{NJ}^0|POST]) + ((E[Y_{NJ}^0|POST] - E[Y_{NJ}^0|PRE]) - (E[Y_{PA}^0|POST] - E[Y_{PA}^0|PRE])) \quad (3.1)$$

The first equation term, bolded above, is the average treatment effect on the treated group (ATT)—here the real change in employment in New Jersey resulting from the minimum wage increase. The above equation appropriately estimates the ATT so long as the parallel trends assumption holds, such that the following equation terms go to zero. The steps utilized to estimate the above equation—full-time employment is first differenced between NJ and PA and these differences are then differenced again—are best represented in Table 3.1 below.

Most importantly, however, is the validity of Card and Krueger’s then-shocking finding that the ATT was actually positive, or in other words, that increasing the minimum wage resulted in increased employment. By utilizing a comparable control group, which exhibited a parallel trend prior to the implementation of the minimum wage increase, Card and Krueger overcome the spatial heterogeneity problem. Other minimum wage studies, most notably work by Dube, Lester, and Reich (2010), have

Table 3.1: Difference-in-Differences using sample averages on full-time employment (FTE)

Dependent Variables	Stores By State		
	NJ	PA	NJ-PA
FTE Prior	20.44 (0.55)	23.3 (1.35)	-2.89 (1.44)
FTE After	21.03 (0.52)	21.147 (0.94)	-0.14 (1.07)
Changes in Mean FTE	0.59 (0.54)	-2.16 (1.25)	2.76 (1.36)

Source: Card and Krueger (1994); Cunningham (2021)

since supported the use of difference-in-differences methodology and similarly found little to no employment effects as a result of changes in the minimum wage.

3.2 Moving Forward: Methodological Design for a Better Rent Control Study

As with the minimum wage studies' resurgence following the 1990s, the evaluation of rent control's effects on rent prices and on neighborhood stability would be better addressed by a difference-in-differences approach. The following section outlines how such a study would be constructed, before explaining the data limitations preventing such work.

3.2.1 Difference-in-Differences: Rent Control in New Jersey

A majority of New Jersey's municipalities with rent control first enacted these policies in the early 1970s, though the strength of these policies has changed significantly over time. The simplest, and likely most feasible approach, would be to assume the pre-treatment period to be anytime prior to 1971 and the post-treatment period to be

1980, by which point all affected New Jersey municipalities had enacted their rent control regimes. For ease of data use, “pre-treatment” period data would be obtained in the 1970 census and “post-treatment” data in the 1980 census.

Rather than using non-rent-controlled cities in New Jersey as a control group, as these cities likely fail the parallel assumptions trend as a result of their statistically different demographics (presented in the above chapter), a control group would be constructed using cities in Pennsylvania, or in another nearby state, with similar housing and demographic characteristics to our sample of rent-controlled cities in New Jersey. To ensure the parallel trends assumption—i.e. the assumption that “that there is no time-variant [municipality] specific unobservables” (Cunningham 2021, 412)—trends in the median monthly contract rent from 1960 to 1970 for both our groups should be graphed and exhibit parallel (or nearly parallel) slopes.

The following equation (equation 3.2) would be used to estimate the ATT:

$$Y_{its} = \alpha + \gamma NJ_s + \lambda D_t + \delta(NJ \times D)_{st} + \epsilon_{its} \quad (3.2)$$

$$\text{where } NJ = \begin{cases} 0 & \text{if observation is from Pennsylvania} \\ 1 & \text{if observation is from New Jersey} \end{cases}$$

$$D = \begin{cases} 0 & \text{if observation is pre-treatment, i.e. from 1970} \\ 1 & \text{if observation is post-treatment, i.e. from 1980} \end{cases}$$

and ϵ = error term

This equation would take the following values:

Pennsylvania Pre-Treatment Period : α

Pennsylvania Post-Treatment Period : $\alpha + \lambda$

New Jersey Pre-Treatment Period : $\alpha + \gamma + \lambda$

New Jersey Post-Treatment Period : $\alpha + \gamma + \lambda + \delta$

The estimation of the above regression would thus provide an estimation of the causal effect of rent control on median monthly contract rent, Y .

3.2.2 Data Limitations

The methodology detailed above cannot be undertaken given the current availability of data on median monthly contract rents (or rent prices more generally) in years prior to 1990. No measure of such rent prices is available in the 1960 census and the variables generated beginning with the 1970 census are inconsistent. The median rent price variable used throughout this paper was sourced from the ACS, but the ACS was not established until 2005. The census would thus be the only large scale source capable of providing data on median rent prices at the municipal level statewide. The analysis could likely be run without the 1960 data, but we would not be able to confirm the parallel assumptions trend, making it hard to ensure the validity of our results.

Given more time, a slightly more complex difference-in-differences approach could be applied here. Instead of treating 1970 as the pre-treatment period, the effect of changes in the strength of rent control policies in municipalities could be considered the treatment, thus changing the post-treatment period. Because a majority of these large-scale changes happened following 2003, we could use more recent data, such as the 2000, 2010, and/or 2020 census. Determining our sample, particularly narrowing down our treatment and control groups given that cities changed rent control policies at different times, is the major hurdle associated with this approach.

Chapter 4

Conclusion

The literature on rent control is largely mixed. Advocates for rent control argue that such policies can help keep rent prices affordable, while others, notably a majority of economists, believe that such policies, at best, have little to no effect on the rental housing market and on rent prices, and at worst, cause mismatches of housing units and tenants, and a shortage of available units.

The first portion of my analysis supports the theory that rent control has no real effect on the rental housing market and rent prices, as I find that rent control, regardless of its strength, has no statistically significant impacts on rent prices in New Jersey. It is important to note that my findings are likely New Jersey specific, as the rental housing market varies significantly across state borders (JCHS 2013). New Jersey is also unique as it has a well-established tenant's organization (the NJTO), which has advocated for rental affordability and tenants' rights since 1969, and has a long history of municipality-specific rent control ordinances.

Further, the methodology used to evaluate the effects of rent control on rent prices in the first portion of my analysis may be ill-suited for studying a price control. In New Jersey, cities with rent control exhibit more urban characteristics than non-rent control cities, notably a greater proportion of renters, greater population sizes, a greater share of Black individuals, and a lower median household income. Though these control variables have a significant impact on rent prices, it is important to consider that there may also be a spatial heterogeneity problem, as it is unclear how

these urban characteristics and rent control interact.

There also exists some theoretical backing for re-evaluating the effects of rent control using methodology developed in the minimum wage literature. Both the labor and the rental housing markets are fraught with market imperfections, notably some market power to employers and landlords respectively. The findings predicted by the monopsonistic labor market— increases in the minimum wage would not lead to disemployment effects— were supported by the empirical literature since the 1990s and since the revolution in the methodology used to evaluate the minimum wage. The shift towards using matching, especially difference-in-differences, and the resulting findings lead me to conclude that rent control should be evaluated using a difference-in-differences approach.

This approach should be relatively straightforward, though I was unable to undertake such an analysis due to a lack of appropriate data. Further research on the effects of rent control is particularly warranted given the continuing housing affordability crisis facing a majority of municipalities in the United States (JCHS 2020).

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