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The effects of residential landlord–tenant laws: New evidence from Canadian reforms using census data[☆]

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ABSTRACT

We study the consequences of landlord–tenant laws on quality and prices in the rental housing market. We use the staggered introduction of Canadian *Residential Tenancy Acts* to study the consequences of a landlord–tenant reform that reduced tenants' litigation costs and improved their bargaining power through mandatory contractual terms. To do so, we employ the difference-in-differences approach to estimate the average treatment effect on a repeated-cross section of households, controlling for income and family structure in five cities. The estimates imply that the reform led to a decline of 2.2 percentage points in the probability of a major defect, with no measurable effect on rent prices or homeownership rates. The average treatment effects are concentrated within families with children, who face greater costs to moving in response to property damage. The results are consistent with a stylized model in which a reduction in litigation costs allows the tenant to more cheaply recover on damages when moving costs are high, with second-generation rent controls limiting increases in rent prices charged by the landlord.

> Patrice went two months without a working sink. When Patrice discovered a large hole in one of the walls, Sherrena gave her a pamphlet about how to keep her children safe from lead paint […] Things came to a head. ***"I'm gonna get an attorney and sue you!"*** **Patrice shouted.** ***"Go ahead."*** **Sherrena laughed.** ***"But my money is longer than yours."*** The next month she tried a different approach. If Sherrena wouldn't respond when the rent was paid, maybe she would respond when it wasn't […] Patrice's plan backfired. Sherrena refused to work on Patrice's place unless she delivered her rent in full. To Patrice, it felt like a catch-22.
>
> —Matthew Desmond, *Evicted: Poverty and Profit in the American City* (2016, p. 73)

> Take the case of a poor man who is homeless. He agrees to pay a high rent to a landlord just to get a roof over his head. The common law will not interfere. It is left to Parliament.
>
> Lord Denning, *Lloyd's Bank Limited v. Bundy*, [1975] 1 QB 326.

1. Introduction

Property owners in the U.S. and Canada have long borne a legal responsibility to make timely repairs as necessary to ensure that their property is habitable for tenants. In most jurisdictions and for much of this history, this responsibility was (or remains) enforced through private litigation. As a result of this costly enforcement, many leases include unenforceable terms, and many tenants fail to receive

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housing services to which they are legally entitled.[1] Although tenants are entitled to withhold rent if their landlord fails to make legally mandated repairs in most of the United States, the amount and cases in which tenants are allowed to withhold vary by state, and tenants who rightfully withhold rent still risk illegal eviction due to poor enforcement. One such failure of the prevailing regime is illustrated in the opening vignette: Wisconsin's habitability laws and protection of tenants who withhold rent failed to ensure that Patrice's landlord supplied the housing services to which Patrice was legally entitled. Patrice was also a mother of three.

This paper studies the effect of a set of reforms in Canada, the *Residential Tenancy Acts* (*RTA*s), that are intended to close this gap in the legal environment by lowering the cost of litigation for tenants and making statutory several pro-tenant rules surrounding habitability and eviction. We use a difference-in-differences framework to estimate the causal effect of these reforms on housing quality. In order to interpret our empirical results, we develop a stylized model in which tenants face both litigation costs and moving costs as a means of responding to property damage which the landlord invests to mitigate. Our results indicate that the law had a greater effect on households with children, where moving costs may be high relative to litigation costs. In the model, shifting liability onto the landlord through the legal system should increase the cost of supplying marginal units, resulting in rent increases on those units. Thus, we also estimate the effect of the *RTA*s on rental rates, though, to be sure, the *RTA*s contained substantial controls on rent increases. We also test whether the laws caused any observable changes to the housing quality of homeowners or to homeownership rates, as one would expect if the reforms caused substantial exit of dilapidated housing stock from the rental market (Sweeney, 1974; Ohls, 1975). We find that the *RTA*s led to a 2.2 percentage-point reduction in the share of the rental stock in need of major repair, which is a greater than 24% reduction in the pre-policy rate of major repair need among rental homes. Across a range of specifications, we find little evidence of an increase in city-level average two bedroom rent prices, likely due to rent control measures included in the *RTA* reforms of many provinces. We additionally find no evidence that the reforms induced a substitution of housing from the rental market into owner occupation.

This study contributes to several literatures. First, we contribute to the literature on habitability laws and tenant welfare that was pioneered primarily by Werner Z. Hirsch, who, in a series of studies, finds that habitability laws are associated with decreases in the stock of substandard housing (Hirsch et al., 1975; Hirsch and Margolis, 1977; Hirsch and Law, 1979; Hirsch, 1981).[2] Only two recent papers could be found that study the effect of habitability laws on housing market outcomes in the last 30 years. Coulson et al. (2023) use a state-level index measuring the landlord–tenant legal relationship to estimate that a one-unit increase in the index (in favor of tenants) reduces eviction rates by 32 basis points and increases rental rates by 1.8%. The recent study that is most similar to ours is by Vigdor and Williams (2022), who use a difference-in-differences approach to estimate the effect of state-level habitability laws and federal lead abatement laws on housing affordability. They find that habitability laws reduce the spread of rental rates across units of different ages, but their estimates for the aggregate rent effect are mixed. Though the authors find evidence of confounding trends, their results suggest that contract rents of older units increased by as much as 25% following the introduction of habitability laws. Their results for lead paint regulations are clearer, suggesting that rent premiums for older units of two or more bedrooms increased by 8.8% following the lead abatement policy. There are multiple possible explanations as to why our results differ from those of Vigdor and Williams (2022). The simplest and most likely explanation is that the *RTA*s typically included second-generation rent control provisions, which limited rent increases on existing tenants and thus also limited the treatment effect on rents.[3]

While we are not the first to study the effects of landlord–tenant laws on housing quality, we do make several advancements. First, we take advantage of census microdata that combine a host of household characteristics with information on the physical state of the household's property. This allows us to examine the heterogeneous effects of the law across income and family structure. New to our study, we find that the landlord–tenant laws we study had a much smaller effect on non-parents than on households with children. We interpret the results through a stylized model adapted from Clarke (2024) in which tenants face both moving and litigation costs. Under rent control, households with high moving costs occupy housing of worse condition — an empirical regularity which we observe in the data. In support of our evidence of the causal effects of the law across family structure, we find that the quality of housing occupied by households with children is substantially less sensitive to the vacancy rate, suggesting that such households are less likely to "fly to quality" in the event of a property damage, consistent with our theory and findings.

However, rather than study rental prices or eviction, we study another equilibrium variable which is of particular importance to the law: the quality of housing. There is a large literature in urban economics on what is known as "filtering", the process by which housing depreciates and is occupied by poorer households. Sweeney (1974) and Ohls (1975) each model the hierarchical nature of the housing market, in which new, high-quality durable units deliver the highest level of housing services and filter to the poorest renters over time. Both models imply that, absent an improvement in pre-transfer income for the poorest renters, income-targeted voucher programs would improve the quality of housing for the bottom of the market rather than subsidized construction programs, which would increase prices for the unsubsidized substitutes. Rosenthal (2014) empirically observes the filtering process posited by these models, suggesting that the rental stock filtered at a rate of around 2.5% per year from 1975 to 2011. Arnott et al. (1983) model housing quality as an endogenous outcome of the housing market.[4] Under this model, habitability laws (or their enforcement) do not affect any units for which the equilibrium level of maintenance is above the legal standard. Units with equilibrium levels below the legal standard would be prematurely demolished or abandoned, depending on the cost of demolition and new home sale prices.

Our paper shows that the legal environment is a first-order feature of models that treat housing as a durable good. Indeed, the dispute over maintenance and repair is the second most frequent dispute between landlords and tenants, next to only eviction for non-payment of rent. Our census data on the quality of housing allow us to establish facts about who occupies housing of poor quality, the central prediction borne out of filtering models. Our causal evidence shows that what

[1] A study of residential lease agreements in Massachusetts finds that 40% of leases contain unenforceable or misleading clauses (Furth-Matzkin, 2017). Leases include terms such as "*[the] tenant agrees to be responsible to pay, in addition to rent, for all damage above wear and tear or unavoidable casualty.*" or "*[the] tenant will, at its sole expense, keep and maintain the premises and appurtenance in good and sanitary condition and repair.*".

[2] Hirsch studies separately the effect of receivership laws, repair and deduct, rent withholding, and retaliatory eviction laws. The studies done by Hirsch and co-authors also show increases in rent paid by low-income tenants, with varying degrees of statistical significance. The net effect of such laws on tenant welfare is inconclusive, though Hirsch (1981) finds more evidence for a negative effect than a positive one.

[3] Further explanations could include differences in the U.S. and Canadian rental markets and/or habitability laws and differences in specifications: namely, that data availability in the Vigdor and Williams study enables estimation of treatment effects by age of the housing stock, which is not possible under our data constraints for the rent specification.

[4] Arnott et al. (1983) model housing quality as being dependent first on the level chosen upon construction, then endogenously determined throughout the lifecycle of the unit, based on construction costs, competition, and demand for quality.

this class of filtering models treats as maintenance expenditures are a function of the legal environment governing the relationship between landlords and their tenants.

More generally, this paper contributes to the literature on the equilibrium effects of supply regulation in the market for housing services, most of which has focused on price controls.[5] Molloy (2020) details the empirical research on the relationship between supply restrictions and housing affordability, including quality regulations. Nearly all such papers study building safety codes and conclude that they have a positive effect on prices (Listokin and Hattis, 2005; Noam, 1982; Dumm et al., 2011, 2012). We find little effect of the *RTA*s on average two-bedroom rent prices and no effect on housing supply as measured by the homeownership rate. The results suggest that, in our context, second-generation rent controls were largely effective at reducing pass-through from shifting liability onto landlords through the legal system. However, the coarseness of our data prevent us from examining whether units on the policy's margin saw rent increases that fail to generate an observable aggregate treatment effect. Future research should continue to take a rich view of reforms to the legal relationship between landlords and tenants and the policies that regulate their behavior.

The paper proceeds in five sections. Section 2 describes the *Residential Tenancy Acts*. Section 3 describes and summarizes the data. Section 4 outlines the empirical strategy for estimating the causal effects of the law on sub-standard housing, average two bedroom rent prices, and homeownership. Section 5 describes the results. Section 6 concludes.

2. The *Residential Tenancy Acts* in Canada

The *Residential Tenancy Acts* (*RTA*s) and variants thereof were important landlord–tenant legislation. As the developing world underwent the process of urbanization and the residential lease outgrew the farm lease, landlord–tenant law evolved to shift liability from the tenant, *caveat emptor*, to the landlord, *caveat venditor* (Hirsch, 1981). The introduction of the *Residential Tenancies Acts* in Canadian provinces began in the 1990s and continued throughout the 2000s (See Appendix A). This legislation emerged from an evolving rental housing market, clogged lower courts, and a growing interest in the use of administrative tribunals for simple litigation. Ontario had statutory regulation of the residential tenancy since the enactment of the *Landlord and Tenant Act*[6] in 1970 and the *Tenant Protection Act*[7] in 1997. However, prior to the *RTA*s, in many provinces, landlord–tenant disputes were sent to Superior Court with little to guide the conduct of their relationship beyond common law rules.[8]

The *Residential Tenancy Act* can be understood best by examining the first section of the legislation, in this case, Ontario's *RTA*,[9] which reads as follows:

> **1.** The purposes of this Act are to provide protection for residential tenants from unlawful rent increases and unlawful evictions, to establish a framework for the regulation of residential rents, to balance the rights and responsibilities of residential landlords and tenants, and to provide for the adjudication of disputes and for other processes to informally resolve disputes.

The *Residential Tenancy Acts* (and their variants) had some differences, but the central aspects are consistent across all provinces, including dispute resolution and mandatory contractual terms.[10] The statutory reforms for residential tenancies have three main features:

1. the transformation of the residential lease into a regulated contract that attempts to rebalance the inequality of bargaining power between lessors and lessees by imposing new duties on lessors, and limiting the ability to evict tenants except for cause;
2. the creation of new tribunals aimed at providing speedier and less-costly resolution of residential tenancy disputes that were previously litigated in the courts; and
3. the adoption of some form of rent control, although the details vary widely across Canada (Mossman, 2019; p. 436).

The policies can be understood as creating a standardized lease form, establishing the Landlord and Tenant Board (LTB), and statutorily mandating certain common law landlord duties such as the duty to mitigate losses (s. 16), covenants interdependent (s. 17), and the duty to repair (s. 20), also known as the implied warranty of habitability in U.S. law (see, e.g., *Pines v. Persion*, 14 N.W. 2d 590 (Wis. 1961)). The rent control provisions of the *RTA*s specify that rental increases within leases will be capped as dictated by regulation under the statute. This is, in essence, a second-generation rent control, also known as vacancy decontrol, as opposed to a strict rent ceiling.

The policies are all clearly intended to shift bargaining power from landlords to tenants in the rental housing market. This interpretation is invoked by language from the courts. In *Northwest Territories Housing Corp. v. Yellowknife Syndicate*,[11] de Weerdt J. offers an interpretation of the purpose of the *Residential Tenancy Act* in the Northwest Territories (which is similar to those in the provinces) as follows:

> "Bearing in mind the usual disparity of bargaining power and financial resources between such tenants and their landlords, the *Act* is evidently intended to restore the balance of power through the public employment of a rental officer to try and mediate and, if necessary, to adjudicate disputes between them". (para. 19).

The *RTA*s addressed two well-understood legal barriers faced by tenants in the private rental housing market:

1. prohibitive litigation costs relative to the stakes of disputes, leaving tenants with little or no access to courts to enforce their rights (Super, 2011); and
2. poor tenant bargaining power over contractual obligations at the time the contract is written or signed, leading to unenforceable and misleading clauses (Furth-Matzkin, 2017).

[5] Arnott (1995) and Turner and Malpezzi (2003) detail the debate in economics over the costs and benefits of rent control. Glaeser and Luttmer (2003) focus specifically on the efficiency losses of rent control that are due to misallocation. More recently, rent control policies in Europe have been studied by Skak and Bloze (2013) and Breidenbach et al. (2022), while U.S. rent control policies in Boston and San Francisco were studied by Autor et al. (2014) and Diamond et al. (2019), respectively. Most relevant to our study, Arnott and Shevyakhova (2014) show that a second-generation rent control in which increases on the rates charged to existing tenants are controlled but rates charged to new tenants are not results in landlords failing to commit to a maintenance path, delaying optimal repairs until the unit turns over, at which time they invest heavily in order to maximize the rate that they are able to charge to a new tenant.

[6] R.S.O 1970, c 236 [*LTA*].

[7] S.O. 1997, c 24 [*TPA*].

[8] The Ontario legislation underwent many reforms that made substantial changes to the law. For instance, the 2006 reform introduced the ability to raise maintenance issues at an eviction for non-payment hearing under s. 82, which was understood to be a major flaw of the Ontario Rental Housing Tribunal that preceded the Landlord-Tenant Board established under the *Residential Tenancy Act*, S.O. 2006, c 17 (see Ontario, Standing Committee on General Government, 38th Parl, 2nd Sess (5 June 2006) at 602).

[9] S.O. 2006, c 17 [*RTA*].

[10] For instance, some mandatory contractual terms are that landlords are required to maintain the premises in a reasonable state of repair, tenants' relief in paying rent in the event of breaches of covenant by the landlord or if the lease is frustrated, and the landlord's duty to mitigate any loss caused by the tenant's breach (Smyth et al., 2015; p. 560).

[11] 1990 CanLII 12545 (NWT SC)

Most importantly for this paper, which examines the law's consequences for rental market outcomes, the administrative tribunals offered a substantial reduction in trial and filing fees. The filing fee in Small Claims Court is typically over $200 but varies by province. However, the cost of filing at Ontario's Landlord-Tenant Boards (LTBs) is $50 for tenants, but $190 for landlords. After receiving cheaper and easier access to courts, tenants with legal claims may seek legal remedy, or at least have more credibility when threatening to take a dispute to adjudication, enabling out-of-court private settlements. However, while the policy is claimed to be pro-tenant, the effects of such a policy are not immediately obvious. With costly higher courts as the alternative, the motivations behind the *RTAs* may not have been entirely benevolent, but instead may have been chosen to reduce litigation costs paid by the State.[12]

There is substantial uncertainty as to whether new laws and procedures are to any effect at all. One argument for the law having no effect is that tenants can simply move to new housing stock — fly to quality — in the event of a damaged rental. However, the data in Ontario on the duration of trials make clear that Ontario's LTB was producing faster processing times than the broader civil litigation system. Based on an exponential fit, the mean time from case initiation to final disposition in Superior Court in the mid-2000s was approximately 130 days, while the LTB processed L1/L2 (landlord eviction) and T2/T6 (tenant maintenance) claims in 30 and 60 days on average in 2016, respectively. This suggests the *RTAs* substantially reduced the time and hassle costs associated with disputing a claim over maintenance. At what margin and for whom this matters most is the topic of our exploration.

Theory. To interpret the *Residential Tenancy Acts*, we borrow from the optimal contracting model between a landlord and tenant with litigation in Clarke (2024). We extend the model to include moving costs to make predictions that are testable in the data used in our empirical application. Homes experience a random damage $x \sim F(x)$ with probability $p(i)$, where i is the landlord's investment which lowers the probability of damage. That is, p is decreasing ($p'(i) < 0$) and convex ($p''(i) > 0$) in landlord investment. The landlord faces a cost of investment $c(i)$, which is increasing ($c'(i) > 0$) and weakly convex ($c''(i) \geq 0$). Tenant liability is capped at minimum of litigation cost ℓ and moving costs m, with the probability that the tenant moves or sues given by $\theta \equiv \mathbf{P}(x > (\ell, m)^-)$ where $(\ell, m)^- = \min\{\ell, m\}$. The landlord chooses a rent price r and investment level i to maximize expected profit subject to individual rationality (IR) and rent ceiling (RC) constraints, solving the problem

$$\begin{aligned} &\underset{(r,i)}{\text{maximize}} && r - c(i) - p(i)\int_{(\ell,m)^-}^{\infty} x f(x)\mathrm{d}x && \\ &\text{subject to} && r + p(i)\left[\theta(\ell,m)^- + \int_0^{(\ell,m)^-} x f(x)\mathrm{d}x\right] \leq \bar{u} && \text{(IR)} \\ &\text{and} && r \leq \bar{r} && \text{(RC)} \end{aligned} \tag{1}$$

where $\bar{u}$ is the cost of the tenant's outside housing bundle and $\bar{r}$ is the rent ceiling. It clearly follows that the landlord chooses i according to whichever of the two constraints is more binding, with $\lambda + \mu = 1$, where λ and μ are the Lagrange multipliers on (IR) and (RC), respectively. Moving costs, such as having children enrolled in school, make a tenant more likely to use the legal system as a means of bargaining over housing quality.

Assuming that (RC) binds, the landlord invests according to the first order condition for investment i^*,

$$c'(i^*) = -p'(i^*)\int_{(\ell,m)^-}^{\infty} x f(x)\mathrm{d}x \tag{2}$$

where the landlord's optimal choice of investment i^* is decreasing in the litigation cost faced by the tenant ℓ if and only if $m > \ell$. Consider a reduction in litigation fees ℓ, as we would expect from the *RTAs*. This model suggests that changes to the legal system improves the housing quality of those for whom moving costs exceed litigation costs ($m > \ell$) and have no effect on those for whom moving costs are below even the new, reduced cost of litigation ($m < \ell$). The theory also predicts that those with higher moving costs occupy housing of worse condition under rent control (or an equally binding wealth constraint $r \leq W$). Finally, under rent control or a wealth constraint, recipients of legal aid (i.e. no litigation costs $\ell = 0$) would occupy housing of better condition than middle-income families facing high moving costs with no subsidy for litigation costs $\ell \in [0, m]$.

3. Data

3.1. Microdata sample

The public use microdata file (PUMF) for each Canadian census of the population is available at the household level and identifies households by their census metropolitan area (CMA) of residence.[13] Five CMAs across four provinces are represented in each year of the sample, which runs every five years from 1991 to 2016: Montreal, Toronto, Calgary, Edmonton, and Vancouver.[14]

Using this data set, we study the effect of the landlord–tenant reforms on the quality of the rental stock and the share of households that rent. Housing quality is measured as the share of households indicating the need for a major repair in their home.[15] Throughout the paper, we refer to a home in need of a major repair as "substandard housing" or a "damaged home" and the need for a major repair as a "defect", which is consistent with its definition: defective plumbing, structural repairs, or issues with electrical wiring. We also refer to whether a home has defects as "housing quality". Table 1 displays the summary statistics for this household-level sample.

From the household-level data, we construct 17 cells based on household income and family structure to control for non-linear relationships between family structure, income, and housing quality.[16] The six family structures are: adult with no kids, adult with 1–2 kids, adult with 3+ kids, couple with no kids, couple with 1–2 kids, and couple with 3+ kids. Income brackets differ for each family structure to ensure

[12] The Macfarlane (1994) Report, a major study motivating this policy change, indicates that the policy attempted to clean out Small Claims Court from relatively simple claims, not simply to improve tenants' rights. For instance, the report claims that many of the cases involve straightforward rent arrears, for which it may seem there is no need for the formal court system.

[13] A CMA is an area consisting of one or more neighboring municipalities situated around a core (of which there can be more than one). A census metropolitan area must have a total population of at least 100,000, of which at least 50,000 must live in the core. An extensive description can be found at `http://www12.statcan.gc.ca/census-recensement/2011/ref/dict/geo009-eng.cfm`.

[14] Due to identification requirements described in Section 4, we omit 2011 and 2016 from our sample.

[15] Major repairs include defective plumbing or electrical wiring, or structural repairs to walls, floors or ceilings. More precisely, the Census of Population in Canada asks respondents "*Is this dwelling in need of any repairs? (not including desirable remodelling or additions)*" to which respondents can answer in three ways: 1. *No, only regular maintenance is needed (painting, furnace cleaning, etc.)*; 2. *Yes, minor repairs are needed (missing or loose floor tiles, bricks or shingles, defective steps, railings or sidings, etc.)*; 3. *Yes, major repairs are needed (defective plumbing or electrical wiring, structural repairs to walls, floors or ceilings, etc.)*. The dependent variable we use is whether the household answered the survey with (3).

[16] This approach to controlling for household income also reduces the impact of extreme income outliers, which are a clear concern, as is shown in the fourth row of Table 1.

Table 1
Mean and standard deviation of sub-standard housing share and homeownership rate by family structure and tenure status.

	mean	sd	min	p25	p50	p75	max	N
1(Repair)	7.03	25.56	0	0	0	0	100	409,571
1(Couple)	57.83	49.38	0	0	100	100	100	409,571
1(Homeowner)	57.63	49.41	0	0	100	100	100	409,571
Income ('000)	57.13	51.08	0	24	47	76	1,000	409,571
No. dependents	1.00	1.21	0	0	1	2	7	409,566
No. rooms	5.84	2.25	1	4	6	8	11	409,571
1(Condo)	8.66	28.12	0	0	0	0	100	409,571

Notes: Household income is in constant 2022 Canadian dollars and censored at $1 million. 16,859 households (3.3% of the total) have incomes above this censor. Censoring the data does not influence the empirical results, as incomes are included only non-parametrically, via income bins. Observations are unweighted, so these statistics do not align with population-level statistics using the same sample of CMAs.

that each cell has mass in every CMA in every 5-year PUMF sample for both renters and homeowners, and that no cell is much larger than any other. We use "tenure status" to refer to whether a household rents or owns their home. Table B1 in Appendix B displays the number of household-year observations by cell, CMA, and tenure status.

Table 2 displays the mean and standard deviation of repair need and the homeownership rate by family structure before and after (i.e., pre and post) the introduction of the *RTA*s were enacted in the households' respective provinces. The change in homeownership rate is positive and statistically significant across all family structures, and the increase is larger for households without children than for households with children, holding the number of adults constant. Repair need generally decreases post-policy, with the exception of renting couples without children, though the difference is not statistically significant. The only differences in repair need that are statistically significant are those for renting households with children and for home-owning couples with children. Improvements in housing quality following the policy are greater for households with children than for households without children, holding the number of adults constant. Across all household structures, rates of substandard housing are higher among renters than among homeowners.[17] See Appendix B for additional summary statistics by CMA and cell.

3.2. Annual CMA-level data

To study the effect of the law on rent prices, we use annual housing market data from the Rental Market Survey provided by the Canada Mortgage and Housing Corporation (CMHC). The CMHC data include annual average two-bedroom rental rates, vacancy rates, and housing starts for 34 CMAs from 1990 to 2019.[18] Three provinces, New Brunswick, Nova Scotia, and Manitoba, are treated prior to 1991 and are dropped from the sample, leaving 30 CMAs across 6 provinces. Relevant variables are summarized in Table 3 alongside relevant variables from the CMA-level 5-year census. As with the PUMF data, repair need is meaningfully higher for renters than for homeowners. In the PUMF data, the average share of tenant-occupied buildings in need of major repair is three percentage points higher than that of owner-occupied buildings (8.8% compared to 5.4%). In the macro data, the difference is nearly identical (8.5% vs. 5.4%).[19]

Additionally, to study the heterogeneous effect of the law on housing quality by the age of the housing stock, we obtain data on housing quality by tenure status and building age from the CMHC at the CMA level for 2001 and 2006. Relevant variables are summarized in Table B4, in Appendix B.

4. Empirical strategy

To assess the effect of the reform on housing market outcomes, we construct differences-in-differences estimates from the public use microdata file (PUMF) and annual CMA panel using the staggered introduction of the *RTAs* across provinces. The three main outcomes of interest are housing quality and average two bedroom rent prices. Since we use homeowners in a triple difference design in the housing quality regression, we also test the effect of the law on homeownership to ensure that homeownership is exogenous to the law.

We can reasonably assume that any treatment effect of the law on any of these variables is not constant over time. The arbitration process is not immediate, and even if a tenant receives a favorable ruling, the landlord may not immediately make the necessary repairs. Furthermore, tenants may not be immediately aware of the law, and public awareness campaigns could take years to improve knowledge of the law among the most affected tenants. Many recent papers have shown that difference-in-difference designs with staggered treatment effects can result in biased two-way fixed effects (TWFE) estimates when the treatment effect is not constant over time or varies by treatment group (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Borusyak et al., 2023; de Chaisemartin and D'Haultfœuille, 2022, 2020). For each of the outcomes we study, we estimate the dynamic treatment effect using the cited heterogeneity-robust estimators with the exception of Goodman-Bacon (2021), who does not propose an event-study estimator, and de Chaisemartin and D'Haultfœuille (2022), whose estimator is not recommended given our setting and data constraints.[20]

We further address dynamic treatment effects by estimating our TWFE models by dropping years for which no untreated observations are available, i.e., census years 2011 and 2016.[21] We also provide estimated treatment effects for the separate difference-in-difference specifications from the Callaway and Sant'Anna (2021) estimator.[22]

Housing Quality. To estimate the effect of the law on a household's probability of defect we construct a two-way fixed effects (TWFE)

[17] When controlling for income, rates of defective housing are higher among renters than among homeowners across all family structures other than single adults with no children, as shown in Table B2 in Appendix B.

[18] As with the census data, we omit all observations beginning in 2010, when the final province, Quebec, is treated (see Section 4 and Appendix A).

[19] The difference is also nearly identical when sample breadth is held constant. When restricted to the five CMAs from the PUMF sample, mean repair need is 8.4% for renters and 5.2% for homeowners. The remaining difference is likely due to the lack of weighting and sample restrictions imposed on the PUMF that are necessarily absent from the macro data. Namely, we drop Aboriginal households and those with missing data on income and tenure status.

[20] In particular, the de Chaisemartin and D'Haultfœuille (2022) estimator assumes a balanced panel with observation periods that map to treatment periods. British Columbia was treated in 2002 and Alberta was treated in 2004. Neither of these years map to the 5-year intervals of our data, which begin in 1991. In unreported analyses, we mapped all treatment dates to the first census year following their treatment and found that the de Chaisemartin and D'Haultfœuille (2022) estimates do not substantively differ from the others.

[21] In a dynamic model, this means that our TWFE estimation is identical to that of Sun and Abraham (2021), who interact event dates with relative treatment dates to extract group-level treatment effects. In our setting, each relative treatment period (e.g., 2 years pre-treatment) includes only one treatment timing group due to our data constraint of 5-year census intervals. For example, the 2001 census is one year prior the Vancouver treatment date, three years prior to the Calgary and Edmonton treatment date, and five years prior to Toronto's treatment date. Thus, interacting a treatment dummy with a relative treatment period variable is equivalent to interacting a relative treatment period variable with dummies indicating each observation's treatment year.

[22] In our setting, which includes treatment effects observed in multiple post-treatment periods but does not include observed periods in which some groups are treated while others are not, the Borusyak et al. (2023) estimator is identical to the TWFE estimator. Were we to include the 2011 and 2016 census years, the Borusyak et al. (2023) estimator would drop those years to

Table 2
Mean and standard deviation of sub-standard housing share and homeownership rate by family structure and tenure status.

	% in need of major repair						% Homeowners		
	Renters			Homeowners					
	Pre	Post	Total	Pre	Post	Total	Pre	Post	Total
Single Adult	7.40 (26.09)	6.65 (25.62)	7.32 (26.04)	7.46 (26.58)	6.37 (22.61)	7.28 (25.98)	32.18 (46.72)	43.43*** (49.57)	33.62 (47.24)
Single Parent	11.80 (32.05)	9.17* (31.22)	11.55 (31.97)	8.34 (27.51)	6.82 (26.34)	8.15 (27.37)	39.94 (48.98)	47.57*** (49.94)	40.76 (49.14)
Couple	7.88 (27.14)	9.28 (27.15)	8.01 (27.14)	4.80 (21.54)	4.24 (19.03)	4.73 (21.22)	66.38 (47.24)	74.29*** (43.71)	67.34 (46.90)
Couple w/ Children	10.59 (30.63)	8.12* (29.03)	10.38 (30.50)	5.01 (21.58)	3.41*** (20.77)	4.86 (21.50)	76.32 (42.51)	78.77*** (40.89)	76.55 (42.37)
All observations	9.09 (28.71)	8.22 (27.80)	9.01 (28.63)	5.70 (23.12)	4.58*** (21.48)	5.57 (22.94)	57.07 (49.50)	62.16*** (48.50)	57.63 (49.41)

Notes: Standard deviations in parentheses. Means are calculated in a regression that controls for year fixed effects to account for survey-level compositional changes. Asterisk indicates that the pre-post difference is statistically significant at the 5% level. Pre and post refer to periods before and after the province-level landlord–tenant reforms. The reforms occurred in 2006 for Toronto, in 2004 for Calgary and Edmonton, in 2002 for Vancouver, and in 2010 for Montreal.

Table 3
Summary statistics for annual CMA-level data and 5-year CMA-level census data.

Variable	Mean	Std. Err.	Min	p25	p50	p75	Max	Count
% major repair (owners)	5.57	0.10	3.39	4.71	5.45	6.29	8.50	120
% major repair (renters)	8.64	0.15	4.55	7.42	8.80	9.96	12.00	120
Homeownership rate (%)	65.04	0.63	46.70	60.95	65.95	70.65	80.70	120
Avg. 2br rent	681.26	6.62	395.00	584.00	659.00	775.50	1,169.00	540
Housing starts	4,087.43	304.90	154.00	684.00	1,418.00	3,190.00	45,475.00	540
Vacancy rate (%)	3.37	0.10	0.00	1.60	2.80	4.40	16.60	540

Notes: Rents, housing starts, and vacancy rates are from annual data from 1992 to 2009 for 29 CMAs across 5 provinces: Abbotsford-Mission, Barrie, Brantford, Calgary, Edmonton, Gatineau, Greater Sudbury/Grand Sudbury, Guelph, Hamilton, Kelowna, Kingston, Kitchener–Cambridge–Waterloo, London, Montréal, Oshawa, Ottawa, Peterborough, Québec, Regina, Saguenay, Saskatoon, Sherbrooke, St. Catharines-Niagara, St. John's, Thunder Bay, Toronto, Trois-Rivières, Vancouver, Victoria, and Windsor. Homeownership and major repair are from the 5-year census for over the same CMAs from 1991 to 2006.

differences-in-differences (DD) specification using CMA-cell and year fixed effects. The estimated equation is

$$\mathbb{1}\left\{\text{Repair}_{ict}\right\} = \alpha_{h(i),c} + \alpha_t + \delta^{DD}\mathbb{1}\{\text{Law}_{ct}\} + \epsilon_{ict} \tag{3}$$

where $\mathbb{1}\left\{\text{Repair}_{ict}\right\}$ equals one if individual i occupies a home with a major defect in city c at time t; $\alpha_{h(i),c}$ and α_t are CMA-cell and year fixed effects, respectively, where $h(i)$ is individual i's income-family structure cell; $\mathbb{1}\{\text{Law}_{ct}\}$ is an indicator at the province level (which nests the CMA level) that equals one following the introduction of the *RTAs* and is zero otherwise; and ϵ_{ict} is a mean zero disturbance term clustered at the province-cell level.

The parameter of interest is δ^{DD}, the difference-in-difference estimator, which, under the assumptions of parallel trends and homogeneous treatment effects, recovers the causal effect of the introduction of the law on the outcome variable by using not-yet-treated provinces as a control group. Four out of five cities in our sample (Toronto, Vancouver, Calgary, and Edmonton) are treated between the 2001 and 2006 censuses, while the remaining city (Montreal) is treated in 2010. Therefore, we only have one never-treated province (Quebec) to use as a control group. Our treatment effect is thus identified by the variation in the probability of a defect for individuals in the pre-2006 census waves relative to those in the 2006 census wave and how that variation differs between individuals in treated cities and those in the Montreal CMA.

It is possible that unobserved trends in each CMA's housing stock could bias our estimates. For example, provinces could enact the *RTAs* in response to a deterioration in housing quality. To address this concern, we also estimate the model separately for homeowners and renters, then conduct a difference-in-difference-in-differences (DDD) test (also referred to as triple differences). The "fully interacted" DDD model is

$$\mathbb{1}\left\{\text{Repair}_{ict}\right\} = \alpha_{r(i),h(i),c} + \alpha_{r(i),t} + \delta^{DD}_{Own}\mathbb{1}\{\text{Law}_{ct}\} + \delta^{DDD}\mathbb{1}\{\text{Law}_{ct}\} \times \mathbb{1}\{r(i) = \text{Rent}\} + \epsilon_{ict} \tag{4}$$

Where $r(i) \in \{\text{Rent, Own}\}$ indexes household i's tenure status, δ^{DD}_{Own} is the difference-in-differences estimate of the treatment effect of the law on homeowners, and δ^{DDD} is the triple differences estimate of the treatment effect of the law on renters. Assuming that the law is exogenous to homeownership status and that homes occupied by owners and renters share a common trend, δ^{DDD} is the causal effect of the law on renters.

We repeat all of the analyses using the CMA-year-level data from the 5-year census of the population and not-yet-treated CMAs as a control group, since the CMA-level data are available for a larger set of cities than the micro data sample.[23] We report the results of the CMA-level analyses in Appendix C.

Rent Prices. Given that we have annual data on rent prices at the CMA level, we employ a standard difference-in-difference estimator using a CMA-year panel rather than cell-year. Thus, the model we estimate is

$$\ln(\text{Avg. 2br. Rent}_{ct}) = \alpha_c + \alpha_t + \delta^{DD}\mathbb{1}\{\text{Law}_{ct}\} + +X'\beta + \epsilon_{ct} \tag{5}$$

Where X is a vector of covariates, which includes contemporaneous and one-period-lagged vacancy rates and housing starts for each CMA.

avoid "forbidden" comparisons between newly-treated and previously-treated groups. However, we choose to end our sample in 2006 in order to prevent such comparisons in the TWFE estimation.

[23] The CMA-level data include a province, Newfoundland and Labrador, that is treated in 2000, which is able to use multiple provinces and dates as controls. However, only the St. John's CMA is included, leading to only one treatment observation in each year for relative treatment years that occur in 5-year intervals relative to 2000. Thus, we exclude Newfoundland and Labrador from the CMA-level analyses.

Table 4
TWFE and triple-difference estimates of landlord–tenant reforms on housing quality.

	Owners (1)	Renters (2)	Both (3)
1(Law)	0.033	−2.208***	
	(0.499)	(0.657)	
DDD			−2.241***
			(0.725)
R-squared	0.008	0.007	0.012
Clusters	68	68	68
N	226,994	166,224	393,218

Notes: Jackknife standard errors clustered by cell-province in parentheses . Columns (1) and (2) include cell-CMA and year FE, while column (3) includes cell-CMA-tenure and year-tenure FE.

The rent data include a wider array of CMAs. Thus, the treatment effect is identified using not-yet-treated CMAs as controls for newly-treated CMAs. We drop CMAs from any province that was treated prior to 1991 and observations for the remaining CMAs beginning in 2010, when Quebec became the last-treated province. We also drop the St. John's, NL, CMA, which is treated in 2000, as it is the only treated CMA in the province and is thus not sufficient to identify its treatment effect, with only one observation per year. In the TWFE estimation, the treatment effect is estimated using the post-treatment observations for CMAs in British Columbia and Alberta as a control for CMAs in provinces that were treated after them. If treatment effects increase (in absolute terms) over time, the TWFE estimate will be biased toward zero. To derive consistent estimates of the treatment effect, we report TWFE estimates alongside those of Borusyak et al. (2023) and Callaway and Sant'Anna (2021), which do not use already-treated units as control groups for newly-treated units.

Homeownership. When testing for the effect of the law on homeownership, we include both homeowners and renters and estimate

$$\mathbb{1}\{r(i) = \text{Own}\} = \alpha_{h(i),c} + \alpha_t + \delta^{DD}\mathbb{1}\{\text{Law}_{ct}\} + \varepsilon_{ict}, \tag{6}$$

Where $\mathbb{1}\{r(i) = \text{Own}\}$ equals one if household i in CMA c owns their home in year t and equals zero if the household rents. As in the repair specification, we use households in the Montreal CMA in 2006 as the control group for households in Vancouver, Calgary, Edmonton, and Toronto. We estimate the treatment effect using both TWFE and the Callaway and Sant'Anna (2021) estimator.

5. Results

5.1. Aggregate effects

Housing Quality.
The results of the housing quality regressions are displayed in Table 4. Column (1) displays estimates from Eq. (3) for homeowners, column (2) displays estimates from Eq. (3) for renters, and column (3) displays estimates from Eq. (4), which conducts a triple-difference estimation using both homeowners and renters. We find that the introduction of the *RTAs* led to a statistically and economically significant decrease of over 2 percentage points in the share of rental homes in need of major repair. The point estimate for owner-occupied homes is positive but negligible and statistically insignificant. We also estimate the treatment effect using the Callaway and Sant'Anna (2021) estimator, which yields statistically significant treatment estimates for both owners and renters, of −0.807 (0.383) and −2.593 (0.636) , respectively. The estimates for the two methods are very similar, which reflects that the TWFE estimator is derived by comparing newly-treated provinces to not-yet-treated provinces, thus eliminating part of the bias that the Callaway and Sant'Anna estimator addresses. However, the two estimates still differ, as Callaway and Sant'Anna further address the bias from dynamic treatment effects by estimating the separate treatment effect of each timing group, then averaging the estimates.

The DDD estimates displayed in the third column is slightly larger in magnitude than the DD estimate, but the difference is not statistically significant, reflecting the small estimated treatment effect on homeowners. As Table 2 shows, the pre-policy rate of major defects was 9% for renters, indicating that the policy caused a nearly 25% decrease in the rate of substandard rental housing.

Both to test for pre-trends and to determine the extent to which the policy's effect changes over time, Figs. 1 and 2 display for homeowners and renters, respectively, the TWFE event study estimates and dynamic treatment effect estimates using the estimators proposed by Callaway and Sant'Anna (2021), Sun and Abraham (2021), and Borusyak et al. (2023). The TWFE coefficients are derived by interacting relative treatment year dummies with the treatment indicator and controlling for cell-CMA and year fixed effects.[24] Both the TWFE and Sun and Abraham (2021) dynamic estimates require an omitted pre-treatment observation in order to identify treatment effects. Due to our 5-year census intervals, we have to omit one pre-treatment observation for each treatment group.

The results for homeowners, displayed in Fig. 1, provide little evidence for a treatment effect. They do, however, suggest that the housing quality of homeowners deteriorated prior to the treatment. There is some evidence that there is a small treatment effect four years after the law was introduced, but the effect is not statistically significant for all estimators, and the variance of the estimate is large. The results for renters, displayed in Fig. 2, are much clearer. There is no evidence of a pre-trend, and the treatment estimates are consistent across estimators. Point estimates are negative for all post-treatment periods, though they are marginally insignificant at the 5% level two years after the treatment. There does not appear to be a trend in the magnitude of the treatment effect within the first four years of the treatment, though data constraints prevent us from estimating the effect on the same CMAs for multiple post-treatment periods.

Section C.1 of appendix C presents a replication of both the DDD estimation and the event study displayed in Figs. 1 and 2 using CMA-year data from the Canadian census of the population. The results hold in both direction and statistical significance but differ in magnitude for both the DDD and event study estimates. In particular, the estimates derived from aggregated data for a larger range of CMAs indicate a treatment effect of a roughly 1 percentage point decrease in repair need as opposed to over 2 percentage points using micro data. This difference could be due to compositional effects (i.e., the treatment effect is weaker in the CMAs that are omitted from the PUMF sample) or due to heterogeneity in repair need by income and/or family structure, which are controlled for in the PUMF sample but not in the CMA-level sample.[25]

Rent Prices. Columns (1), (2), and (3) of Table 5 respectively display the TWFE, Borusyak et al. (2023), and Callaway and Sant'Anna (2021)

[24] See Callaway and Sant'Anna (2021); Sun and Abraham (2021); and Borusyak et al. (2023) for a detailed description of how the other estimates are derived. As mentioned in Section 4, the TWFE point estimates are identical to those of Sun and Abraham (2021), though the standard errors differ because the former are estimated via jackknife, while the latter are estimated via wild bootstrap.

[25] Column (1) of Table C6 repeats the difference-in-difference estimation from column (2) of Table 4 using both TWFE and the Callaway and Sant'Anna (2021) estimator on a CMA-year panel of the five CMAs included in the PUMF sample. The TWFE point estimate is −1.765 and not statistically significant at conventional levels, and the Callaway-Sant'Anna estimate is −2.218 and significant at the 1% level. This estimate is similar to the Callaway-Sant'Anna estimate using the full sample of CMAs, suggesting that the difference between the estimates derived from micro vs macro data are due in part to sample composition.

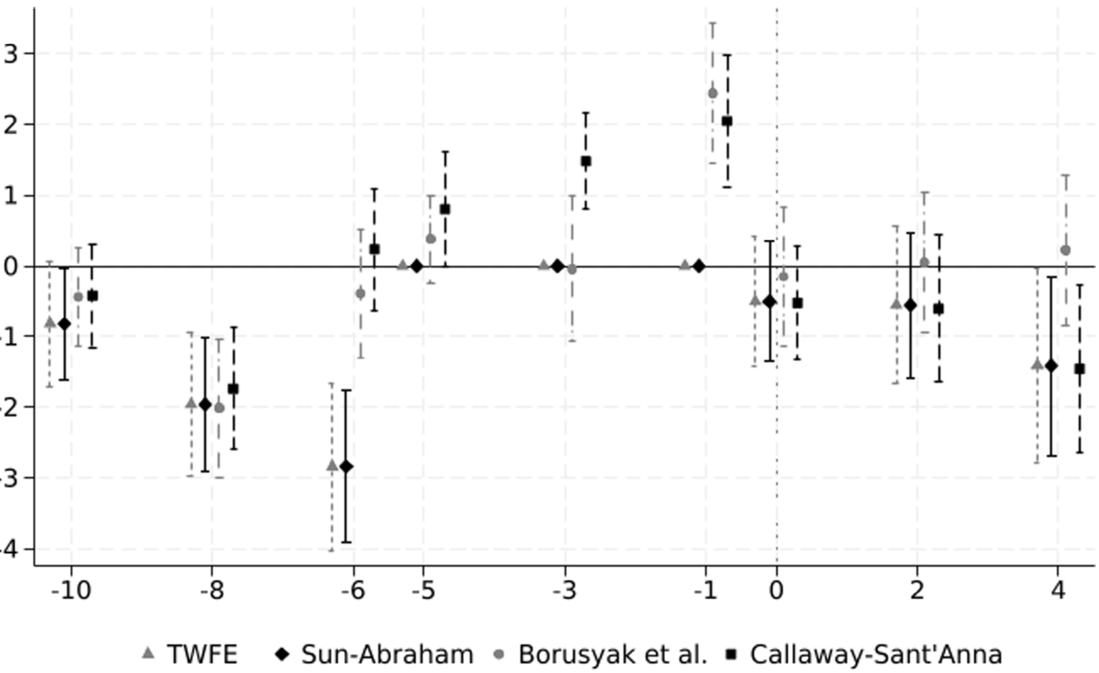

Fig. 1. Dynamic Treatment Effect Estimates for Homeowners.
Notes: Error bars indicate 95% confidence intervals using standard errors clustered by cell-province. TWFE standard errors are estimated via jackknife. All others are estimated via wild bootstrap. The *x*-axis displays the number of years since the introduction of landlord–tenant reforms. The estimators displayed are two-way fixed effects, using gray triangles and short dashed lines; Sun and Abraham (2021), using black diamonds and solid lines; Borusyak et al. (2023), using gray dots and dot-dashed lines; and Callaway and Sant'Anna (2021), using black squares and dashed lines. The treated group is home–owning households in Vancouver, Edmonton, Calgary, and Toronto, which were treated in or before 2006. The control group is home–owning households in Montreal, which was not treated until 2010 (after the sample ends). The dependent variable is an indicator that equals 100 if the household's home is in need of major repair and 0 otherwise.

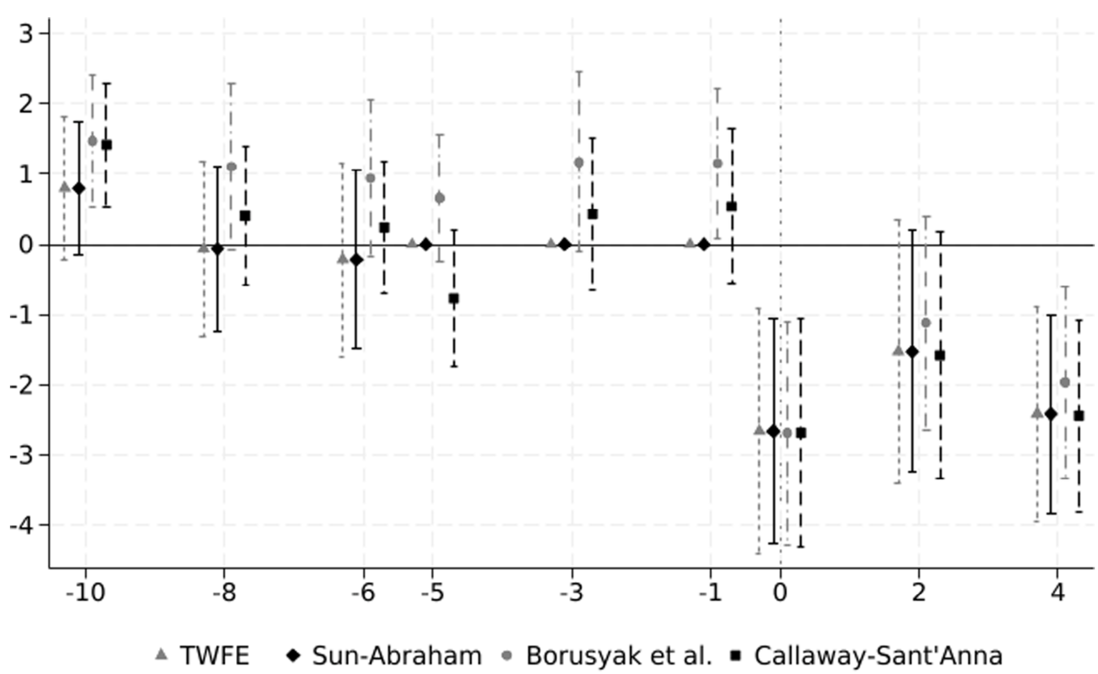

Fig. 2. Dynamic Treatment Effect Estimates for Renters.
Notes: Error bars indicate 95% confidence intervals using standard errors clustered by cell-province. TWFE standard errors are estimated via jackknife. All others are estimated via wild bootstrap. The *x*-axis displays the number of years since the census year immediately following the introduction of landlord–tenant reforms. The estimators displayed are two-way fixed effects, using gray triangles and short dashed lines; Sun and Abraham (2021), using black diamonds and solid lines; Borusyak et al. (2023), using gray dots and dot-dashed lines; and Callaway and Sant'Anna (2021), using black squares and dashed lines. The treated group is renting households in Vancouver, Edmonton, Calgary, and Toronto, which were treated in or before 2006. The control group is renting households in Montreal, which was not treated until 2010 (after the sample ends). The dependent variable is an indicator that equals 100 if the household's home is in need of major repair and 0 otherwise.

Table 5
TWFE and triple-difference estimates of landlord–tenant reforms on housing quality.

ATT estimate	log(Two-Bedroom Rent)			1(Homeowner)	
	TWFE	B-J-S	C-S	TWFE	C-S
	(1)	(2)	(3)	(4)	(5)
	0.032	0.046**	0.023	1.802	0.946
	(0.074)	(0.022)	(0.023)	(1.303)	(0.966)
CMA FE	✓	✓	✓		
CMA-Cell FE				✓	✓
Year FE	✓	✓	✓	✓	✓
R-squared	0.947			0.226	
Clusters	5	5	5	68	68
N	520	520	520	393,218	393,218

Notes: Standard errors in parentheses. TWFE standard errors in the rent regression are estimated using the jackknife method proposed by MacKinnon et al. (2023), which uses a generalized inverse when clusters (such as Quebec) cannot be omitted. TWFE standard errors in the homeownership regression are estimated via standard jackknife. Borusyak et al. (2023) and Callaway and Sant'Anna (2021) standard errors are estimated using the wild bootstrap method. Standard errors are clustered by province-cell in the homeownership regressions and clustered by province in the rent regressions. Rent regressions include controls for contemporaneous and one-period-lagged vacancy rates and housing starts.

estimation results from Eq. (5), which specifies the effect of the law on average two-bedroom rent prices. All three estimates are positive and imply an increase of 2.3% to 4.6% in average rent prices following the introduction of the law. However, only the Borusyak et al. (2023) estimate, displayed in column (2), is statistically significant at conventional levels. The standard errors of the other two estimates are very large. Since a different sample of cities is used for the rent regressions than for the other two dependent variables, Table C6 in Appendix C displays the same three treatment estimates using a sample of the same five cities used in the other two specifications. The point estimates for all three methods are larger, ranging from 7.2% to 9.3%, but only the Borusyak et al. (2023) estimate is statistically significant, at the 10% level.

The absence of observable aggregate rent increases despite costly improvements to the rental stock is consistent with the second-generation rent control associated with most of the *RTAs* (Arnott, 1995). These results do not preclude increases on some units, e.g., newly-rented ones which were excluded in some of the rent control regimes. More detailed micro-data would be needed to investigate the effect that the *RTA*'s had on the distribution of rents.

Homeownership. Columns (4) and (5) of Table 5 respectively display the TWFE and Callaway and Sant'Anna (2021) results from Eq. (6), which specifies the effect of the law on homeownership. Both estimates are modest, indicating a .9 to 1.8 percentage-point increase in the homeownership rate caused by the law. However, the standard errors are large, so neither estimate is close to statistically significant at conventional levels. These results are largely confirmed by estimates derived using CMA-year data from the 5-year census of the population, which are displayed in Table C6 of Appendix C. However, the Callaway and Sant'Anna (2021) estimate is statistically significant at the 10% level, though the point estimate of the treatment effect is smaller, at half of a percentage point. These results suggest that the landlord–tenant reforms induced little, if any, substitution across ownership types, from rental use to owner occupancy.

5.2. Heterogeneous effects on housing quality

It is possible that the point estimates from our pooled specification mask wide variation in the treatment effect across the age of the housing stock and the income or family structure of the tenant as the model in Section 2. Fig. 3 plots the heterogeneous treatment effects separately for owners and tenants for different age groups of the housing stock, using our data on dwelling conditions by tenure and period of construction from the Canadian Mortgage and Housing Corporation (CMHC). The effects are derived using the methodology proposed by Borusyak et al. (2023), in which individual treatment effects are estimated and aggregated by a grouping variable, adjusting standard errors accordingly. The rental stock results are mostly consistent with expectations: The point estimate for every age group before 2000 is negative for renters and increases monotonically from 1946 to 1980, after which the effect flattens.[26] The largest effect is for renter-occupied homes built in the final year bin,1996–2000. This result is surprising and inconsistent with expectations. Given the small number of observations and time periods, it is possible that this result is driven by a few outliers.[27]

The housing quality of owner-occupied homes of all ages appear largely unaffected. The estimated effect generally increases with construction year, though only the estimate for 1961–1970 is statistically significant at the 5% level. Thus, there is some evidence of the reforms leading to a substitution out of the rental market. However, the results for owner-occupied homes are unclear, while the results for the rental stock are consistent with theory and statistically significant across most groups: The oldest homes, which are likely to have the greatest repair need, saw the largest decreases in repair need following the policy.

Fig. 4 plots the heterogeneous treatment effect estimates separately for renters and homeowners using the PUMF sample for each income-household structure cell. The results suggest that renting households with children disproportionately benefited from the reforms: Single-adult households with 1–2 children earning over $75k and two-adult households with three or more children saw the largest decreases in repair need, of between 5 and 6 percentage points. Most other groups who rent experienced a two to three percentage-point decline in repair need, with a few groups seeing no statistically significant effect. Renting households with children also generally experienced the largest difference in treatment effects relative to home-owning households with the same income and household structure.

Two fifths of the homeowner groups experienced a statistically significant change in repair need, most of which were small, and with no consistent sign. Single parents with 1–2 children and $75k-$100k in income and single adults with three or more children experienced roughly 2.5 and 4 percentage-point increases, respectively.

Taken together, these results suggest that the groups with the greatest cost of substandard housing—those with children—benefited the most from the reforms, and there is little, if any, effect on households that own their homes.

The explanation may lie in moving costs, as the theory presented in Section 2 predicts: it is much more costly for households with children to move, so they are more likely to stay in a dilapidated unit and thus be on the margin of the reform. Indeed, Fig. 5 shows that repair need is much higher and more strongly correlated with income for households with children.[28] Furthermore, the rate of substandard housing correlates with the CMA vacancy rate for households without

[26] This flattening or even slight decline between 1971 and 1995 could be due to survivorship bias, as real assets are typically renovated on a 20- to 30-year timeline. Homes constructed between 1971 and 1980 that were not demolished by 2001 and 2006 were more likely to have been recently renovated than those constructed between 1981 and 1995.

[27] Indeed, from 2001 to 2006, two CMAs in Quebec, Gatineau and Sherbrooke, experienced large increases in repair need for homes built between 1996 and 2000, causing an average increase of 0.7 percentage points for Quebec. Homes in that age group in other provinces experienced a decrease or plateau on average, with the exception of Alberta, which experienced a one percentage-point increase. The average rate of repair need for homes built between 1996 and 2000 across the provinces was below two percent in both 2001 and 2006.

[28] The coefficient on income is −0.011 (p-value=0.253) for renters without children and −0.018 (p-value=0.003) for renters with children. Reported p-values are computed using jackknife standard errors clustered by CMA.

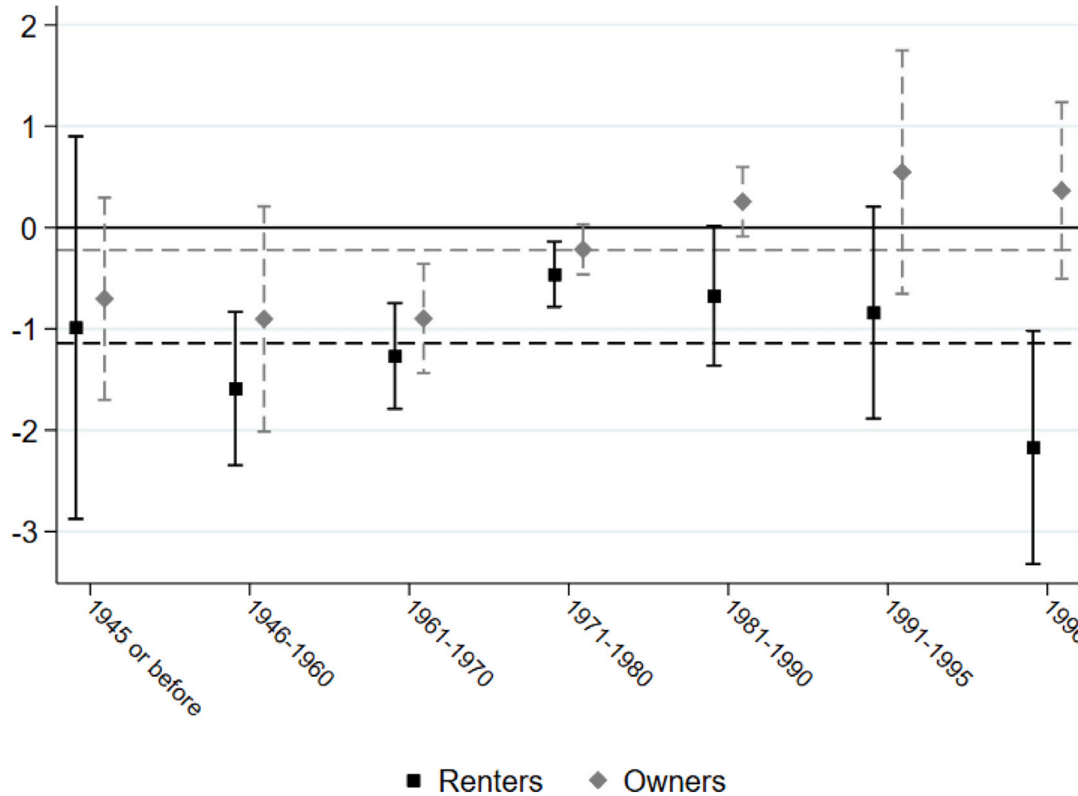

Fig. 3. Treatment Effect Estimates of Landlord-Tenant Reforms on Housing Quality by Construction Year of Housing Stock.
Notes: The dependent variable is the share of homes (in percent) that are in need of major repair. Estimates derived using the heterogeneous effects estimator proposed by Borusyak et al. (2023), using CMA, year, and age bin fixed effects. Error bars indicate 95% confidence intervals using wild bootstrap standard errors clustered by province. The *x*-axis displays the age bins for each set of homes. The gray dashed line displays the estimated ATT for homeowners, and the black dashed line displays the estimated ATT for renters.

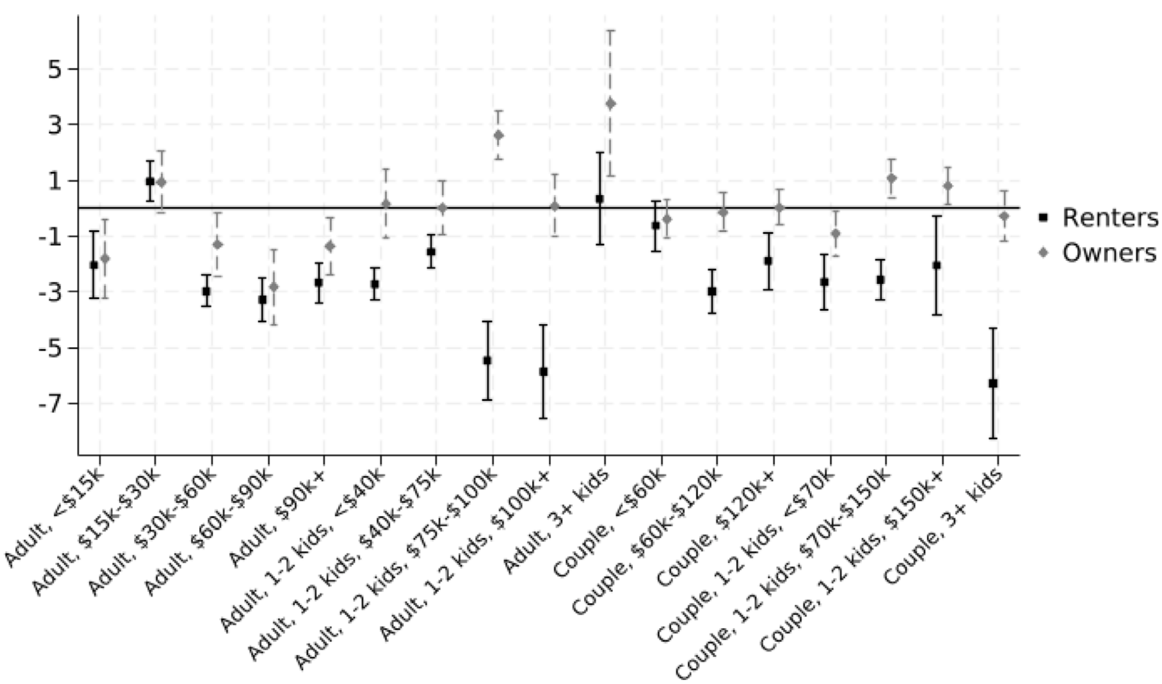

Fig. 4. Treatment Effect Estimates of Landlord-Tenant Reforms on Housing Quality by Income and Family Structure.
Notes: The dependent variable is a dummy variable equal to 100 if the household's home is in need of major repair and 0 otherwise. Estimates derived using the heterogeneous effects estimator proposed by Borusyak et al. (2023), using CMA-cell and year fixed effects. The *x*-axis displays the income-family structure cells that households were grouped into. Error bars indicate 95% confidence intervals using wild bootstrap standard errors clustered by CMA-year.

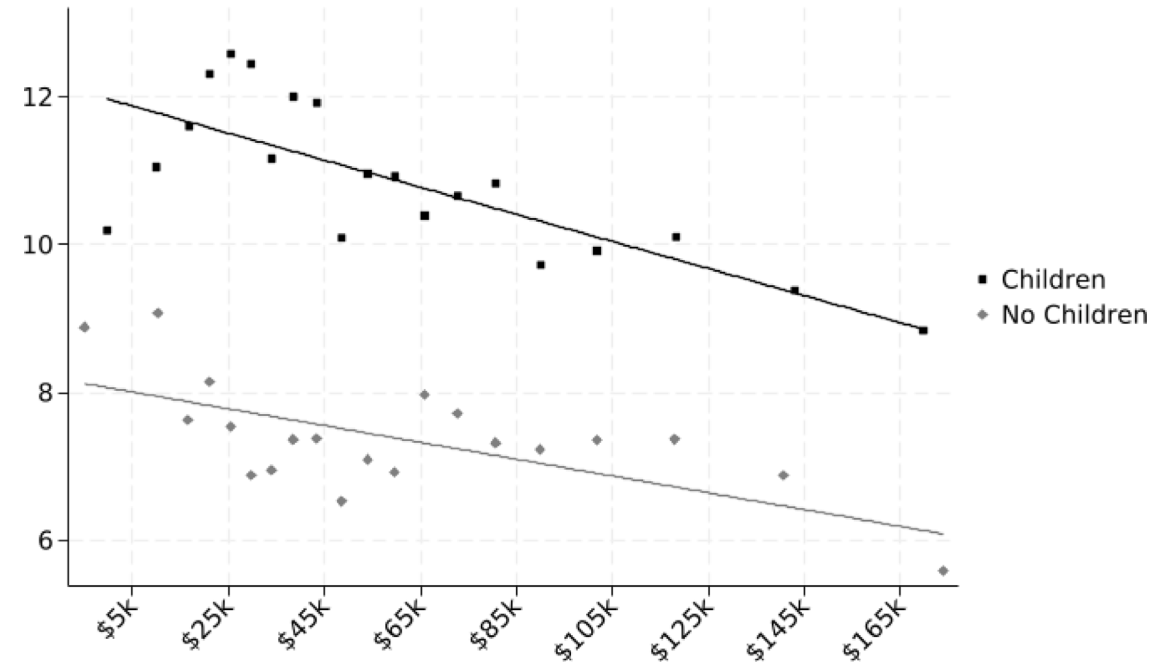

Fig. 5. Binscatter Comparing the Share of Renters' Homes in Need of Major Repair by Presence of Dependents in the Home.
Notes: The dependent variable is an indicator (multiplied by 100) for whether a household's home is in need of major repair, residualized by CMA and year fixed effects. The independent variable is residualized household income. Income is censored at $5k and $180k, which represent the 5th and 95th percentiles of the income distribution. Income is in 2022 Canadian dollars.

children, but not for households with children. Regressing a dummy (scaled by 100) indicating whether the household's home is in need of major repair on the lagged vacancy rate (in percent) interacted with a dummy for whether the household includes children indicates a coefficient of −0.561 (p-value=0.014) for childless households and a coefficient of 0.177 (p-value=0.260) for households with children.[29] This suggests that families without children have a higher propensity to "fly to quality" in slack housing markets, which is consistent with our proposed theoretical mechanism in which children impose higher moving costs.

6. Conclusion

This paper accomplishes several goals which advance the study of the landlord–tenant relationship and the rental housing market. We use the staggered introduction of the Canadian *Residential Tenancy Acts* across provinces, legislation which increased tenants' access to courts through cheaper administrative tribunals and supplanted lease agreements with mandatory contractual terms, to study the effect of landlord–tenant laws on the rental housing market. We find that the law led to a decrease of over two percentage points in the share of tenants occupying a property in need of major repair (a 24% decline) and little evidence of an increase in the average two bedroom rent price. This absence of an increase in aggregate rents despite a comparatively large repair effect is consistent with the second-generation rent control associated with many of the *RTAs* (Arnott, 1995). Applying modern econometric tests for our research design, we find little support for the claim that landlord exit or substitution across ownership type played an important role in the improvement in housing quality.

Using a repeated cross-section of households from the Canadian census microdata sample and controlling for income and family structure, we find that the aggregate improvement in housing quality is disproportionately driven by large improvements for families with children. We also show that the law's effect on the quality of the rental stock increases almost monotonically with the age of the building, while the effect on the owner-occupied housing stock is statistically insignificant for all but one age group.

We employ recent advances in difference-in-differences (DiD) methods to examine the dynamics of the reforms' treatment effect. While point estimates and their variance differ across methods in the pre-treatment period, the estimates from each method show no evidence of a pre-trend for renters, and the treatment estimates for 0-, 2-, and 4-years post-treatment are remarkably similar across methods and statistically significant for all but the second year.[30] We do not find evidence for an increasing treatment effect over time, though our setting and data constraints prevent us from estimating multiple post-treatment periods for the same treatment group.

This paper may have larger implications for the landlord–tenant law and other consumer protection efforts. Decades of legal reforms from courts and legislators to improve the quality of the rental housing stock through an implied warranty fell short of a single piece of legislation.[31] Stories such as Patrice's in the opening vignette and other research on contracts and litigation in residential tenancies (Greiner et al., 2013; Furth-Matzkin, 2017) highlight the central role that mandatory contractual terms and cheaper enforcement play in the rental housing market. Our results offer an example of legislation filling a gap in common law to achieve policy goals (i.e., a reduction in urban blight) through reforms to the legal process.

This paper is surely not the end of the long debate on the effects of various landlord–tenant laws. Shifts in liability through the legal system may have consequences for prices. While we find no evidence of

[29] p-values derived from jackknife standard errors, clustered by CMA. CMA and year fixed effects are included. The difference in coefficients has a p-value of 0.004.

[30] For some methods, the treatment effect is statistically significant in the second year at the 10% level.

[31] For comparison, the effects found in this paper are much larger than those found in Hirsch and Law (1979) when studying the effects of habitability laws on sub-standard housing.

an aggregate increase in rents, perhaps due to complementary second-generation rent control in the *RTAs*, future research should attempt to examine the housing supply implications of various landlord–tenant laws, especially for poor tenants or renters not covered by the reforms' rent control provisions. Data challenges also limit our ability to examine finer price predictions. More disaggregated data may offer a window into other margins of adjustment by landlords, such as flatter rent gradients or unit modifications to escape regulation (Vigdor and Williams, 2022; Diamond et al., 2019). How residential landlords and tenants bargain and share surplus remains a fruitful area for future research. Finally, future studies may examine the role of moving costs in disproportionately exposing children of poor families to blight.

CRediT authorship contribution statement

Dylan R. Clarke: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Project administration, Resources, Software, Validation, Visualization, Writing – original draft, Writing – review & editing. **Daniel E. Gold:** Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Project administration, Resources, Software, Validation, Visualization, Writing – original draft, Writing – review & editing.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jue.2024.103631.

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