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Abstract

We use the introduction of rent stabilisation measures in Ireland after 2016, and their tightening in 2021, to understand the impact of these policies on rental supply. We use a district-level quarterly panel 2010-2023 to estimate whether there were market exits by landlords, in particular an increase in sale listings and a decrease in rental activity, after rent controls were applied. Rental activity is measured using both online listings and official tenancy registrations, with further data on room rentals. We use both two-way fixed effects and, given the nature of treatment, staggered treatment estimators. We also control for housing market and wider economic conditions and restrict our sample to minimize the impact of unobserved variables. Across all specifications, we find evidence of market exit, especially after rent controls were tightened: rent controls are associated with more sale listings and fewer rental listings/registrations. The negative impact of rent controls of room rental listings is, likewise, consistent with market exit rather than simply reduced mobility.

JEL codes: D22; R31.
Keywords: rent control, Ireland, landlord attrition.

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1 Introduction

In response to rapidly rising rental prices internationally, many jurisdictions have in recent years revisited the use of regulatory measures that limit the allowable increase in rents (Kholodilin, 2024). Rent control measures had been widely used internationally in the period following the First and Second World Wars, in the context of high inflation rates due to severe housing shortages. Following the global financial crisis starting in 2007, many economies again faced pressures from housing rent inflation and many cities in countries such as Spain, US, France, Germany and others all began to re-deploy or strengthen rental regulations.

This is also true of Ireland. Responding to rapidly growing inflation in rental housing, in late 2016 the Irish Government introduced a series of rent control policies called "Rent Pressure Zones" (RPZs). The policy provided for a rent increase cap, with the allowable annual increase at 4 per cent. In 2021, the cap was revised to 2 per or the overall inflation rate[1], whichever is lower. Somewhat unusually, these caps are designed to apply both within and between tenancies. RPZs status is designated by criteria around the growth rate in new rents and the average rent level. Over time, more and more areas met these criteria, and by 2023, roughly 80 per cent of tenancies in Ireland were covered by RPZ policy.[2]

However, despite their increasing popularity as a policy tool, the economic costs and benefits of these regulations has long been a source of considerable debate amongst academics and policymakers. While research has shown that the benefits of these regulations can accrue to sitting tenants in the form of lower nominal rents (Sims, 2007; Mense et al., 2018) or slower rent increases (O'Toole et al., 2021), the impact on the supply side of the market has been clearly identified as a major cost. On the one hand, rent controls may simply reduce mobility in the rental sector, but the overall stock in unaffected: rent-controlled dwellings, because of those controls, change hands less often. On the other hand, caps on rents (or rent increases) may actively reduce supply in the rental sector, with landlords exiting in the face of a reduction in the net present value of their property as a rental home. Other impacts, as outlined by Kholodilin (2024) in his comprehensive review of the literature, include reduced construction of new rental supply, and a fall in maintenance and upkeep, leading to depreciation of the existing stock.

[1] As measured by the Harmonised Index of Consumer Prices (HICP).

[2] For more discussion on the policies, please see O'Toole (2023).

A number of papers have demonstrated the various supply channels. However, much of this literature relates to older, strict rent control settings. Diamond et al. (2019) explore the impact of rent controls in San Francisco on landlords and tenants. They find that tenants remained for longer in their rent controlled property and landlords reduced supply by up to 20 per cent through sales, conversion of the building and redevelopment. Sims (2007) explores the ending of rent controls in Massachusetts and finds that while rents did fall, landlords shifted units away from renting and also reduced the quality of the rental units. Similarly, following the introduction of the Berlin Rent Freeze in 2020, Hahn et al. (2022) find impacts on both prices and supply by exploiting variation across controlled and uncontrolled areas. Similar findings are also presented in Sagner & Voigtländer (2022). Kholodilin et al. (2022) find impacts on new supply and prices in Catalonia following the short lived rent control measures in that jurisdiction. Kholodilin & Kohl (2023) takes a long-run cross country perspective in analysing rent controls and also finds evidence of negative supply effects. In addition to the supply externalities arising from these rules have been clearly demonstrated, rent control policies have also been shown to cause other unintended effects. Examples include frictions in the labour market by reducing job mobility levels (Svarer et al., 2005) and changing the composition of communities (Sims, 2011). They also include contributing to homelessness (Early & Olsen, 1998), by lowering the vacancy rate and increasing the rental level in the uncontrolled units.

Given the extensive literature in this area, our contribution stems from the combination of applying modern difference-in-differences methods to a novel dataset that spans both official registrations and unofficial listings. Specifically, we use the introduction of RPZs across Ireland's official Local Electoral Areas (LEAs) after 2016 to understand the impact of these policies on market exits by landlords. RPZs were introduced in Ireland following a period of rapid rental inflation and applied a second-generation rent cap on allowable increases both within and between tenancies O'Toole (2023). The introduction of these policies provides a useful setting to explore the market exit channel, as the private rental sector across the whole jurisdiction moved from a position of no price caps to having specific geographic areas – the RPZs – designated as rent controlled during our sample period.[3] While the RPZ assigment is non-random, the setting is still rich in temporal and

[3] The nature of Ireland's rental regulations means that the sale of a property in the rental sector is almost always done with vacant possession, i.e.a landlord can ask the tenant to vacate the property if they are selling it.

spatial variation across neighbouring local areas, allowing the estimation of causal effects using the appropriate two-way fixed effects (TWFE) approach. The regulations exempted new to market supply and therefore only applied to existing properties which had a rental history in the past 24 months. We can therefore exploit the geographic variation in RPZ status across LEAs over time to identify the impacts of rent controls on property sales as well as rental advertisements and rental registrations with the national regulator.[4] We also tighten our identification strategy by using a contiguous border design strategy which eliminates non-neighbouring areas.

While the empirical setting provides novelties relative to the literature, a major novelty of our research comes from the data. We combine different datasets to explore the impact of rent price caps on landlord market exists, to generate a panel of almost 10,000 observations – 166 LEAs observed for each of 56 quarters. First, we have property-level data on both sales listings and rental listings from Daft.ie, Ireland's largest online property sales and rental advertisement website. Second, we have property-level data on rental registrations from Ireland's rental regulator the Residential Tenancies Board (RTB). We supplement these with lagged sale and rental price inflation and a measure of the local unemployment rate, as controls. Our dataset allows us to explore the heterogeneous effects of the rules across property (second hand versus new) and landlord types (household individual versus company). To the best of our knowledge, this heterogeneity has not been explored in the literature to date.

Five principal results emerge from our analysis. We find that introduction of rent controls, firstly, increases the supply of homes for sale and, secondly, reduces rental listings and registrations. Thirdly, we find that the effects are stronger in the period following the the tightening of the allowable rental price increase in July 2021. Looking at all LEAs, and using the full set of controls, rental listings in the post-2021 period are 13% lower and rent registrations 9% lower after the introduction of rent controls, while sale listings are 14% higher. The negative supply effect is evident, fourthly, even in room rentals, technically exempt from rent controls, rather than full-property rentals, consistent with market exits rather than simply a reduction in mobility. Lastly, we find that the effects are driven by individual, rather than corporate landlords: the effect on registrations is 20% for individuals but positive and not statistically significant for corporate landlords. Corporate

[4] The commencement of any new tenancy must be registered with the Rental Tenancies Board (RTB) in Ireland. This regulatory database has been made available for the purposes of this research.

landlords were more likely (especially after 2021) to register newly-built rental properties, exempt from initial rent caps, and to register properties built under planning codes that required them to remain rental homes. Alternatively, household landlords may face additional financial constraints, making them more responsive and their greater sensitivity to the allowable increase in rents. Regardless, our results suggests a non-uniform impact of these policies that should be considered by policymakers when choosing the appropriate calibration of these measures.

The rest of the paper is structured as follows: section 2 provides a short background to RPZs in Ireland and outlines our empirical setting. Section 3 presents the theoretical framework and outlines our empirical strategy, while Section 4 presents the data. Section 5 documents the empirical results while section 6 concludes.

2 Background and context

Following the onset of the financial crisis in Ireland, the housing market suffered a protracted and sustained downturn. The period from 2002-2007 was characterised by a significant credit boom which led property values to increase unsustainably and left the financial system with huge vulnerabilities (Lyons, 2018; McCarthy & McQuinn, 2017; Kelly et al., 2018; McQuinn, 2014). As the financial system collapsed, property prices and rents declined and many mortgaged households were left in arrears, with major affordability challenges and negative equity (Kelly et al., 2012; O'Toole & Slaymaker, 2021). This led to an increase in household and landlord mortgage arrears as well as the requirement for a major bank recapitalisation programme to stabilise the financial sector.

Rental prices dropped notably following the onset of the crisis while the number of properties available for rent increased. As the economy began to recover in 2013, rental prices began to rebound and supply became more scarce. This continued into 2015 and 2016 when issues around affordability began to come to the fore from a policy and political perspective (Corrigan et al., 2019). Acute signs of rent affordability distress were becoming evident for particular cohorts of the population, and coupled with a near halting in new housing supply following the crisis, a greater proportion of the population were seeking housing in the private rental sector. Historically, this sector in Ireland played a less important role in long term housing given the county's high homeownership rates. The rental sector had therefore

been characterised by low levels of regulation and, in particular, an absence of rent controls on allowable price increases.

In late 2016, in response to rapidly rising rent prices and low supply, a system of allowable inflation caps were introduced which limited the rent increases in any given year to 4 per cent. These caps were to apply in specially designated areas called "Rent Pressure Zones" (RPZs) which were characterised by high and rapidly rising rents. RPZs could be designated at either the local electoral area level (136 LEAs in 2016, but now 166), or at the broader local authority level (32 LAs). The allowable rent increase rate was changed in July 2021 to a maximum of the monthly level of the Harmonised Index of Consumer Prices (HICP) as published by the Irish Central Statistics Office. A further change capped the maximum allowable increase to 2 per cent from November 2021, due to the major spike in inflationary pressures following the war in Ukraine. The impact of these changes to the allowable increases is quite notable as the change post 2021 is a very severe tightening of the potential real return; when inflation was high in 2022 the allowable rent increases were negative in real terms. More generally, the two-part condition after 2021 means that, by law, rents cannot increase in real terms in RPZs. We explore this change as part of our analysis as we test whether the policy tightening was associated with an increase in the supply side effects of the policies by increasing the rate of disinvestment through sale.

At the outset in late December 2016, the Minister for Housing classified Dublin (all four local authorities) and Cork City as RPZs. Following this point, assignment as a RPZ followed a number of criteria which were evaluated against national rent indicators on a quarterly basis.[5] More specifically, if an area experienced rental growth in new rents of more than 7 per cent per annum in 4 of the past 6 quarters and the level of the rent was greater than a reference rate, then the Minister was obliged to designate the areas as a rent pressure zone. The reference rate began as the national standardised average rate but this was adjusted in mid-2019 to provide three reference rates depending on the geographic area.[6]

Figure 1 presents the RPZ status of all local electoral areas in Ireland. The RPZs are presented in colour depending on the quarter in which they were classified. Areas not designated as RPZs are indicated in grey. There were

[5] The process was as follows: the Housing Agency, a state body, would refer areas to the Minister, who in turn would ask the Residential Tenancies Board (RTB) to review the criteria using their Rent Index data. If the areas passed the criteria, they would be referred to the Minister for classification.

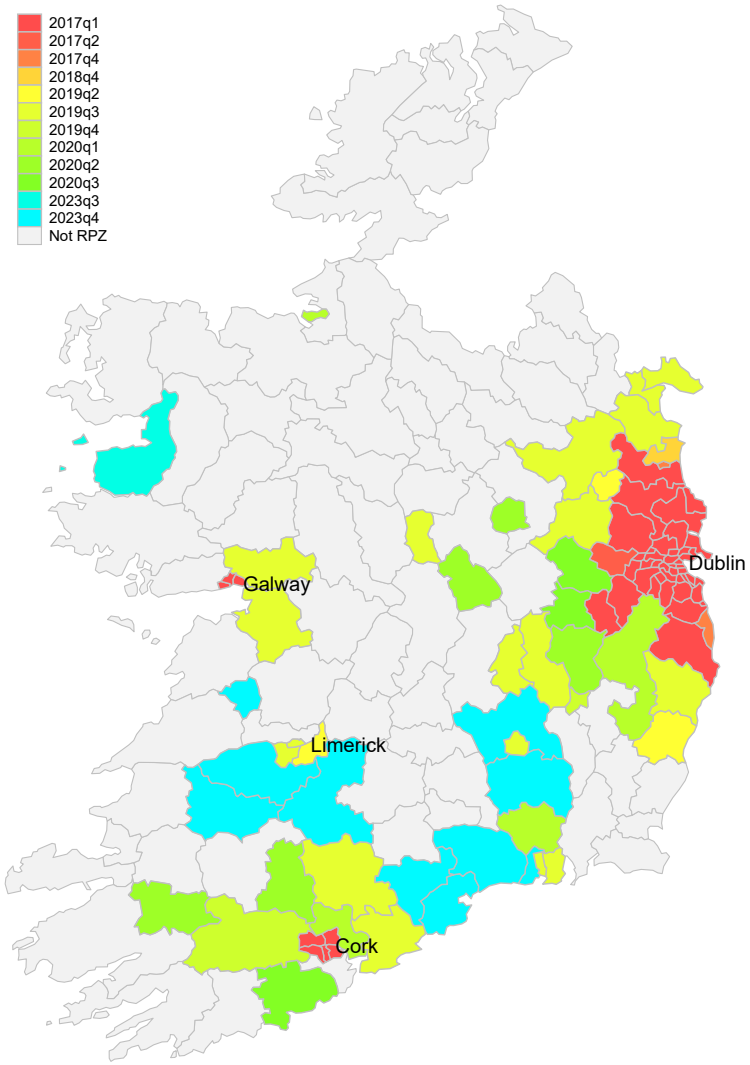
[6] See O'Toole (2023) for details.

three major waves. The first areas to became RPZ were Dublin and Cork (in red) in 2017. Soon, the classification pattern has followed along the major urban areas, other cities and the Greater Dublin Area (which would serve as the commuter belt for Dublin). During the second wave, a large number of areas (noted in green here) were classified as RPZs following the change to the designation criteria in summer 2019.[7] During the Covid-19 pandemic there was a slowdown in rent increase and consequently no new RPZ were designated. However, by 2023 with the third wave, even more rural areas became RPZs.

To explore this expansion of RPZ policy, Figure 2 presents the total share of LEAs that are RPZs, the share of residential addresses in RPZ areas, and the share of addresses that had at least one rental listing as an RPZ. At the end of 2023, over 60 per cent of all LEAs are rent pressure zones, which represent 70 of total housing stock, and around 80 per cent of all rental market. This highlights the fact that the majority of the rental market in Ireland is now covered by the rent stabilisation rules.

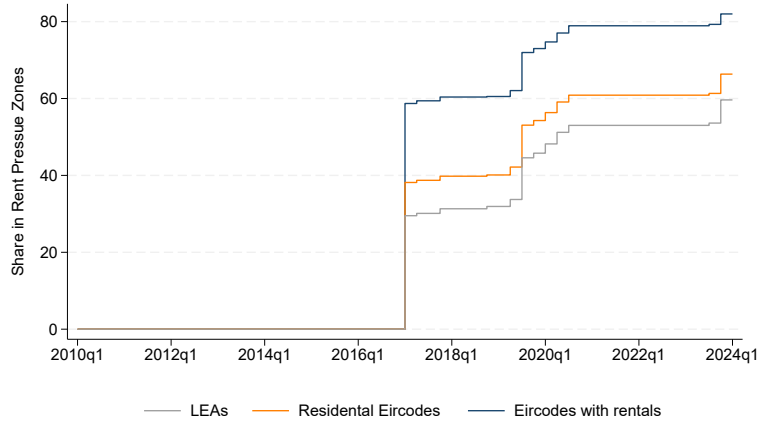
[7] The denominator of the ratio used to classified an area changed from using just a national standardised average to having three different regional comparison groups: national for Dublin, non-Dublin for the Greater Dublin Area (GDA) and the rest of the country for other areas.

Figure 1: Overview of Rent Pressure Zones by Classification Date



Source: Residential Tenancies Board.

Figure 2: Share of Irish housing market in Rent Pressure Zones



Sources: GeoDirectrory database of addresses and Eircodes, Daft.ie rental listings.

3 Theoretical Framework

3.1 Baseline

The aim of this research is to test the impact of the RPZ rules on market supply i.e. the impact on property sales by landlords. The economic mechanism behind this potential hypothesis is that the imposition of rent controls changes the rent-pricing ability of landlords, thus impacting the net present value of their investment relative to the no-rent control setting. If the negative impact on their profitability from the regulations is sufficient, this may lead them to divest the asset and put the property for sale on the market. Thus, the two main related hypotheses that we wish to test are as follows:

- **H1**: *The introduction of RPZs increases the sales of properties by landlords.*
- **H2**: *The introduction of RPZs decreases the number of rental properties.*

There is one particular feature of the Irish regulations that makes it a useful setting to these these hypotheses. Irish rent controls are second-generation, in that rent increases, rather than levels, are the subject of control (Arnott, 1995; Kholodilin & Kohl, 2023). However, unlike in other jurisdictions, those controls apply across tenancies, rather than simply within a tenancy. This feature reflects a concern among policymakers that, given the disaggregated nature of ownership of rental homes in Ireland, landlords may simply try to evict tenants to "reset" to the market rent.[8] However, landlords in Ireland do have the right to evict a tenant if they want to sell the property.

RPZ status in Ireland applies at the Local Electoral Area level, of which there were 166 in 2019. Our identification strategy to explore the impacts of the rent pressure zones is to aggregate the two micro datasets to the local electoral area and to test the impact of the introduction of the regulations on the following variables:

(1) the number of properties listed for sale
(2) the number of properties listed for rent
(3) the number of properties registered as new tenancies with the RTB

To conduct our main analysis, we construct an LEA-quarter i, t panel dataset that covers 166 LEAs for 56 quarters over the period 2010Q1 to 2023Q4. While our treatment is time-varying, affecting our empirical strategy as explained below, our window has roughly seven years before and after the first RPZ designations come into force.

[8] This concern was effectively one of enforcement, as – other than in certain specified circumstances – landlords were not free to evict tenants paying the legal rent.

Further, we use the policy change, recalibrating the allowable maximium increase from 4% to the lower of HICP/2% to test whether this tightening had an impact on the supply-side of the market. Based on market commentary, this significant shift from a relatively loose control on rents to guaranteeing the rents would, in real terms, decline over time would be expected to affect landlord returns and thus be more likely to lead to exits. For that reason, we use the following regression in our baseline, for each LEA i and quarter t:

$$ln(Y_{it}) = \beta_0 + \beta_1 RPZ_{it}^{\text{PreJuly2021}} + \beta_2 RPZ_{it}^{\text{PostJuly2021}} + \gamma_i + \alpha_t + \varepsilon_{it} \quad (1)$$

where Y is the outcome of interest. Our principal outcomes are: (1) the number of second-hand sales listings, (2) the number of rental listings; and (3) the number of tenancies registered; but further outcomes (in relation to landlord type and room rentals) are discussed below.[9] We include a full set of LEA fixed effects (γ_i) and time fixed effects (α_t). Coefficients β_1 and β_2 pick up the impact of the RPZ classification for those LEAs that become designated in the period from classification to July 2021 and after July 2021, respectively. In terms of our main hypotheses, and variable definitions, these are presented in Table 3.1. If the RPZ legislation caused supply-side market exits, then we would expect to find sales listings increased in treated areas and rental listings (and registrations) decreased. Theory suggests, further, that these effects were strong after July 2021, than before.

Table 1: A-priori expectations

Dependent Variable	Pre-July 2021	Post-July 2021
ln(Sales Listings)	$\beta_1 > 0$	$\beta_2 > \beta_1 > 0$
ln(Rental Listings)	$\beta_1 < 0$	$\beta_2 < \beta_1 < 0$
ln(Rental Registrations)	$\beta_1 < 0$	$\beta_2 < \beta_1 < 0$

3.2 Control Variables

A simple identification strategy such as this relies on the assumption that no other confounding factors explain the differential trends between classified RPZs and non-classified RPZs after the introduction of rent controls. Given that the criteria for classification rely on the level and growth rate of rents,

[9] Where no listings or registrations were recorded in any time period per LEA the data were replaced by ln(1) in the analysis.

there are likely to be differences in housing market outcomes across the two areas that could confound the effects in the main specification, leaving it difficult to isolate the effect of the RPZs on our outcome variables.

We control for this in two ways. Firstly, we include a range of additional control variables, capturing the variation in both housing market conditions and economic conditions more broadly, across LEAs over time. Our vector of contorls, $X_{i,t-1}$, includes the level and year-on-year change in sale and rental prices, at LEA level, included with a year's lag to avoid any simultaneity. To capture the difference in economic conditions across LEAs over time, we include a measure of the unemployment rate which should ensure our estimates are purged of any effects of time-varying economic differences across Ireland.[10]

$$ln(Y_{it}) = \beta_0 + \beta_1 RPZ_{it}^{\text{PreJuly2021}} + \beta_2 RPZ_{it}^{\text{PostJuly2021}} + \gamma_i + \alpha_t + \mathbf{\Omega X_{i,t-1}} + \varepsilon_{it} \quad (2)$$

Despite the inclusion of these additional factors, it is also possible that their are other omitted variables that might lead to systematic differences in the group of RPZs with non-RPZs and thus bias our coefficient on the RPZ assignment status. To attempt to further address this particular concern, we use a contiguous border design technique, the "adjacency sample". In this specification, we run our analysis only on LEAs that border RPZ or non-RPZ areas, roughly half the sample of LEAs. The aim of this approach is to limit both the treated and control groups to neighbouring areas which are more likely to share similar economic and housing market contexts. In other words, with this approach, it makes the LEAs more similar in housing and economic conditions, reducing the potential bias from omitted variables.

Figure 3 presents the map of LEAs by RPZ status and RPZ adjacency. The purple areas indicate the control group in the adjacency sample, while the orange areas represent the treatment group. LEAs in grey are excluded, as they are not RPZs and do not border an RPZ, whlie LEAs in black are excluded for the opposite reason. Treatment and control LEAs in this specification are typically hinterlands around larger towns. Table 2 gives the number of LEAs in each category for the period 2020-2022.

[10] These time varying local unemployment rates are were developed using Census-level statistics on LEA unemployment, interpolated between Census years using unemployment insurance statistics at the local office level. Further details on the relevant calculations are available on request.

Table 2: Number of Local Electoral Areas by RPZ status in 2020-2022

	RPZ since approx.				
Status in 2022-2022	2017	2019	2023	Non-RPZ	Total
RPZ & fully surrounded by RPZ	46	4	0	0	50
RPZ & adjacent to non-RPZ	6	32	0	0	38
Non-RPZ & adjacent to RPZ	0	0	8	33	41
Non-RPZ & fully non-RPZ	0	0	3	34	37
Total	52	36	11	67	166

Figure 4 presents, for both the full and "adjacency" samples, trends in average rents across treated and control groups. outcome variables and controls across the different areas, using mix-adjusted hedonic price regressions applied to the daft.ie dataset of market rental listings. In the full sample (right-hand panel), rents in the treated areas, which are dominated by the largest cities, are higher than in the other group and appear to rise earlier (and faster). Most of this difference in levels and trends disappears using only the adjacency sample.

3.3 Heterogeneous effects

A feature in our approach that is new to the literature is testing whether the effects of rent controls are heterogeneous across different subgroups of the landlord population and rental sector. The RTB data enables us to test whether the landlord is a registered company or an individual. Individual landlords may have different time horizons and opportunity costs than corporate landlords: individual landlords may require rental income to supplement labour income, while corporate landlords may enjoy economies of scale and thus lower costs overall. Further, after 2018, a new planning code for purpose-built rental housing meant that sale of individual properties was not possible during the initial fifteen years of the life of the development. As these new rental developments were required, by law, to have a single (corporate) owner, this may also drive differential effects.

Alternatively, it could be the case that corporate landlords react quicker to policy changes, adjust their investment holdings and are less willing to hold the assets under negative real returns while household landlords hold the asset for the overall price appreciation as well as the rental income thus are more patient. In the two alternative hypotheses (H1, H2), the magnitude of the coefficient would differ, as summarized in Table 3. Under

these hypotheses, the change in rental registrations is greater for household landlords (denoted H) under H1 and for corporate landlords (denoted C) under H2. All effects are expected to be less that zero if the RPZs have led to market divestment.

Table 3: A-priori expectations

Dependent Variable	ln(Rental Registrations)
H1: Financial constraints	$\beta_{1,2}(H) < \beta_{1,2}(C) < 0$
H2: Asset Appreciation/Rapid Adjustment	$\beta_{1,2}(C) < \beta_{1,2}(H) < 0$

4 Data

The data for this research is drawn from two principal sources, with additions, in particular for control variables. The first main data source is daft.ie, Ireland's largest property sales and rental listing website, which contains over 90 per cent of all listings, both sale and rental, coming to the market (Lyons, 2015).[11] The dataset contains a range of fields. Of most relevance here is the date, location and type (sale/rental) of listing, so that each listing can be placed in the correct LEA and its RPZ status is known. Other information, including rental or sale price, property type, size, age and energy efficiency, are used to generate mix-adjusted price indices, used to capture trends in prices in the open market, at a local level, that may affect a property owner's decision to sell or rent a home. For sale listings, there is also information on whether the property is second hand or newly built, allowing us to test whether there are any differential effects of rent controls on new versus existing homes. A third dataset, on room rentals (as distinct from full property rentals) is also included, as this may help distinguish between reduced turnover in the rental market and exit of properties from the rental market.

Our second principal data source is the set of registrations of new and renewing tenancies from the Residential Tenancies Board (RTB), Ireland's rent regulator. By law, all new tenancies must be registered with the RTB. Information included in this process includes address, property type and size, number of tenants, and other characteristics of the property and tenancy. An identifier for landlord type is be used to distinguish between

[11] The micro-level information associated with each listing has been collated and used in multiple research papers, such as Carroll et al. (2020) and Gillespie et al. (2024).

individual/household and corporate landlords.[12] These data are used by the RTB to calculate a quarterly Rent Index which is in turn used in the calculation of RPZ status.[13] The underlying tenancy registration microdata have been made available to the researchers for the purposes of this research. These RTB data have been used in a number of previous research papers (O'Toole et al., 2021; Coffey et al., 2022). As with listings, tenancies are geocoded and can be allocated to each local electoral area.

The main aim of this research is to assess the impact of the Rent Pressure Zone legislation on the supply of properties to the market, in particular, whether landlords exited the rental market after these regulations applied to their property. Figure 5 presents the number of properties listed for sale on Daft.ie, Ireland's largest online property sale and rentals listing website. These sales listings cover all seller types, including non-landlords, but are useful to provide a high level trend. The data are presented for second-hand homes as well as new sales. It can clearly be seen the number of listings has increased notably following the recovery from the financial crisis in 2014, 2015, and 2016 as property prices recovered. The trend reversed following the initial onset of the Covid-19 pandemic but recovered somewhat thereafter. At the end of the period (2023IV), there is a further decline in total listings, reflecting the impact of rising interest rates on the second-hand market in Ireland.

The hypothesis outlined earlier would suggest not only an increase in sale listings, after RPZ status locally, but also a corresponding reduction in the number of properties available for rent. Figure 6 presents the total number of rental listings on the daft.ie website over time as well as the number of registrations with the RTB. During our sample period, by law, all tenancies had to be registered with the RTB at the time of commencement and at set renewal periods. Earlier these renewals took place every six years, then four years and, from 2022, all active tenancies needed to be registered annually. It can clearly be seen that the number of properties listed and registered has been declining rapidly over time. It is possible that the rental stock was constant, but turnover fell, reflecting a supply constrained environment. If this were the case, the number of room rentals (not controlled by RPZs)

[12] This distinction is possible as the data can be split out by whether the landlord provides a household social security number (PPSN) or a company registration office number (CRO) when registering. These are required to be included by law and the RTB verifies these fields.

[13] See https://www.rtb.ie/calculator/rpz for details.

would be unaffected; our extended analysis below tests this hypothesis using room rental listings as the outcome variable.

Summary Statistics Table 4 presents summary statistics, for the almost 9,300 LEA-quarter observations. Across the sample, 22 per cent of observations have RPZ status. The average (lagged) growth in rental prices is just under 5 percent, while for sale prices it is 0.5%; the local unemployment rate is on average just over 13 per cent. As noted above, conditions vary considerably across the period, including years of sharp economic contraction (especially 2010-2011) and rapid economic growth (in the late 2010s).

Table 4: Summary statistics of regression variables

Variable	Obs	Mean	Std. dev.	Min	Max
ln(Daft 2nd-hand sales)	9,296	4.120	0.829	0	6.590
ln(Daft rental listings)	9,296	4.159	1.128	0	8.521
ln(RTB registrations)	9,296	4.441	0.856	0.693	7.415
ln(Daft sharing listings)	9,296	2.927	1.677	0	7.967
ln(RTB inividual regs.)	9,296	4.300	0.828	0	7.274
ln(RTB company regs.)	9,296	1.932	1.359	0	6.178
RPZ dummy	9,296	0.224	0.417	0	1
ln(Rent price index), lagged	9,296	6.737	0.451	5.739	8.155
Δln(Rent price index), lagged	9,296	0.047	0.093	-1.154	1.616
ln(Sale price index), lagged	9,296	11.976	0.475	10.688	13.582
Δln(Sale price index), lagged	9,296	0.005	0.139	-1.282	0.575
Local unempl. rate, lagged	9,296	0.134	0.057	0.025	0.338
ΔLocal unempl. rate, lagged	9,296	-0.006	0.016	-0.093	0.105

To provide more granular insight into developments in the main series, 7 presents the three main outcomes over time, for the full and adjacency samples, with vertical lines representing changes in the policy or its spread. The first vertical line represents the introduction of the first RPZs in late 2016, while the second indicates when the other main wave of RPZ designations, spreading the impact of the policy outside the Greater Dublin region and Cork City. The dashed vertical line represents the change in permissible rent increases.

For second-hand listings (Panel A), there exist relatively similar trends in the adjacency sample, unlike in the full sample, where in particular during the period 2012-2014 different trends applied across treatment and control

samples. In both left- and right-hand panels, there is an increase in sale listings after the RPZ policy was tightened in 2021, although this coincides with other economic changes (including strong housing demand). More broad, the Covid-19 pandemic appears to have had a clear impact across all four samples: an initial drop in listings and a subsequent recovery. These broader economic conditions underscore the importance of inclusion of the suite of control variables, to capture the impact of housing and economic market conditions not related to RPZs.

In panels B and C in figure 7, the trends are presented for the average number of rental listings on Daft.ie and the average number of registrations with the RTB for the four geographic groupings of LEAs. Considering the daft.ie data, there is general downward trend across all of the areas in line with the supply tightness in the Irish rental sector. Focusing on the border sub-sample, it appears the rapidity of the decline increased somewhat in the RPZ areas relative to the adjacent non-RPZs following the policy tightening in 2021. This appears also to be the case for the RPZs fully surrounded by other RPZs (black line in panel B). These trends are also evident in the RTB tenancy data: notable downward trends in registrations with some acceleration occurring after the policy tightening. Naturally, these trends do not indicate a causal impact of the RPZs, as other confounding factors are at play but these are the dynamics we are looking to test in the econometric section of the paper.

We present, finally, trends in room-rental listings and in registrations by landlord type, in figure 8. For the adjacency sample, it appears that the decline in room-rentals (Panel A) corresponds to the RPZ period and is prevalent for RPZ LEAs to a greater degree than the non-RPZs. A similar trend is evident in the non-border sub-sample but with a much steeper decline around the Covid-19 period for the RPZs. For corporate and household registrations (Panel B), the border sub-sample the trends appear similar across RPZs and non-RPZs. For the non-border sub-sample, the RPZ areas which include Dublin and Cork city have experienced a notable increase in the average registrations by company landlords since 2010, reflecting the emergence of institutional landlords described above. As it has no previous rent, a new rental dwelling is exempt from the RPZ limits on rent increases. However, there is likely to be significantly less institutional corporate rental activity outside the big cities in Ireland, meaning corporate activity in the adjacency sample is likely to be smaller companies.

5 Results

In this section, we present the results of our econometric analysis, starting first with the baseline findings, in relation to the key hypotheses across our two samples, full and adjacency. We test whether the effects are time-varying and undertake some robustness checks, in particular deploying new staggered treatment effect specifications to check our baseline approach. Finally, we present results of analysis on room-rental listings as well as results by landlord type.

5.1 Full Sample

The main findings of the baseline two-way fixed effects estimates are presented in table 5, for a sample with all LEAs from 2010, regardless of adjacency-status. The first three columns contain the results of a simple specification without controls but including time and LEA fixed effects. The two RPZ policy variables are included, for before and after July 2021. The final three columns include our suite of controls which capture differences in housing and economic market conditions across LEAs over time. Within both sets of columns, the first column has the number of sale listings as the outcome of interest, the second has the number of rental listings, and the third the number of registrations.

Column (1) in Table 5 shows a positive and statistically significant effect of both the RPZ treatment variables on properties listed for sale. The magnitude of the coefficients suggest an increase in sales listings of nearly 12 per cent relative to the non-treated group for the initial RPZ period up to July 2021. The effect is even larger for the period post July 2021, at just over 18 per cent. Column (4) includes control variables and while the result is somewhat smaller, there remains a statistically significant and positive effect of the RPZs on sale listings: 7 per cent increase in listings for RPZ areas in the period to July 2021 and roughly twice that since. Our housing market controls have an intuitive interpretation: other factors being equal, lower rents and higher sale prices are systematically linked to more sale listings.

Looking at the volume of rental listings, specifications both without and with controls suggest a negative effect only after 2021, with a positive effect before, although as with sales the magnitudes differ after the inclusion of controls. Rental listings are 2 per cent higher 2016-2021 but 12.5 per cent lower between July 2021 and December 2023 in RPZs relative to non-RPZs. Looking, lastly, at registrations, rent controls are systematically linked with

Table 5: Baseline results, all LEAs

	(1) Daft sales*	(2) Daft rentals	(3) RTB regs.	(4) Daft sales*	(5) Daft rentals	(6) RTB regs.
RPZ pre-July21	0.115***	0.058***	-0.076***	0.074***	0.027**	-0.036***
	(0.012)	(0.012)	(0.009)	(0.013)	(0.013)	(0.010)
RPZ post-July21	0.183***	-0.043**	-0.060***	0.138***	-0.125***	-0.092***
	(0.014)	(0.018)	(0.014)	(0.015)	(0.018)	(0.014)
Rent price				-0.270***	-0.189***	-0.375***
				(0.063)	(0.069)	(0.047)
ΔRent price				0.061	-0.156**	0.035
				(0.059)	(0.069)	(0.045)
Sales price				0.457***	0.099**	-0.138***
				(0.058)	(0.048)	(0.031)
ΔSales price				-0.147***	-0.211***	0.016
				(0.043)	(0.036)	(0.027)
Unempl. rate				1.090***	3.254***	1.015***
				(0.284)	(0.291)	(0.203)
ΔUnempl. rate				-0.545	-0.136	-0.414
				(0.517)	(0.482)	(0.342)
Constant	4.087***	4.155***	4.456***	0.292	3.818***	8.489***
	(0.004)	(0.004)	(0.003)	(0.627)	(0.534)	(0.337)
Observations	9,296	9,296	9,296	9,296	9,296	9,296
R-squared	0.893	0.945	0.940	0.895	0.947	0.942
Fixed effects	Time+LEA	Time+LEA	Time+LEA	Time+LEA	Time+LEA	Time+LEA

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

fewer registrations in both periods, with or without controls. With controls, rent controls were associated with 3 per cent fewer registrations 2016-2021 and 9.2 per cent fewer after.

Combining these results, the evidence from the baseline for the full sample is consistent with the hypothesis outlined above: controls on rent increases across tenancies, especially when more binding (after 2021) are systematically linked with more sales listings and fewer rental listings and registrations. This finding is robust to the inclusion of controls. The magnitude of the effects, post-2021, is estimated to be approximately 10% fewer rental listings/registrations and 14% more sale listings.

5.2 Adjacency Sample

As noted above in Section 3, there are potentially other unobserved characteristics that differ between treated and non-treated groups in the full sample that could bias our estimates. Table 6 presents the results of the baseline specifications applied to the Adjacency sample. In broad terms, the results from the full sample hold. Again, there are more sale listings in rent-controlled LEAs, especially after 2021. And again, there is less

rental activity (either listings or registrations) in rent-controlled districts, in particular after the limits on rent increases become binding. In this adjacency sample, the magnitude of the effect on rental activity after 2021 is large: -17% for listings and -14% for registrations.

Table 6: Baseline results, border LEAs

	(1) Daft sales*	(2) Daft rentals	(3) RTB regs.	(4) Daft sales*	(5) Daft rentals	(6) RTB regs.
RPZ pre-July21	-0.013	0.042**	-0.076***	-0.041**	0.032*	-0.057***
	(0.017)	(0.019)	(0.016)	(0.018)	(0.019)	(0.017)
RPZ post-July21	0.054***	-0.169***	-0.134***	0.058***	-0.175***	-0.138***
	(0.018)	(0.024)	(0.019)	(0.018)	(0.024)	(0.019)
Rent price				0.189**	0.162*	-0.204***
				(0.090)	(0.094)	(0.074)
ΔRent price				-0.229***	-0.183*	0.071
				(0.088)	(0.094)	(0.072)
Sales price				0.295***	-0.048	-0.194***
				(0.067)	(0.060)	(0.053)
ΔSales price				-0.015	-0.055	0.069
				(0.051)	(0.048)	(0.043)
Unempl. rate				-1.466***	0.596	0.409
				(0.381)	(0.417)	(0.335)
ΔUnempl. rate				-0.675	2.454***	0.455
				(0.653)	(0.688)	(0.551)
Constant	4.177***	4.120***	4.295***	-0.372	3.562***	7.891***
	(0.004)	(0.005)	(0.004)	(0.833)	(0.757)	(0.674)
Observations	4,424	4,424	4,424	4,424	4,424	4,424
R-squared	0.853	0.931	0.919	0.857	0.932	0.920
Fixed effects	Time+LEA	Time+LEA	Time+LEA	Time+LEA	Time+LEA	Time+LEA

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

5.3 Dynamic Effects

The findings in tables 5 and 6 point towards an impact following the policy tightening in 2021. Part of the explanation could be that the impacts of these policies accrue over time and change investors appraisal of the assets profitability slowly; investors might not react to the immediate introduction of these policies and rather only react with a lag. For example, the initial RPZs were introduced for a three year period and investors may not have known whether these would become a permanent feature of the market. To test directly whether the impacts of the policies have a notable change in the impact over time, we replace the estimates of the RPZ dummies in the main specifications with quarter dummies times the LEA indicator:

$$ln(Y_{it}) = \beta_0 + \sum_{t=2017q1}^{2023q4} \beta_t RPZ_{it} + \gamma_i + \alpha_t + \mathbf{\Omega X_{i,t-1}} + \varepsilon_{it} \quad (3)$$

The coefficients, β_t, capture the quarterly impact of the RPZ status for each quarter from 2017q1 onwards. The results of these estimates for all three outcome variables (sales listings, rental listings and rental registrations) are presented in figure 9. We present the results for the full sample as well as the contiguous border design sample. In each of the figures, the coloured vertical lines represent 95% confidence interval based on robust standard errors.

Additionally, we also test for a potential bias that may occur in difference-in-difference models with staggered treatment (Goodman-Bacon, 2021). The two models without controls are estimated using the Wooldridge (2021) 'Extended TWFE' estimator:

$$ln(Y_{it}) = \beta_0 + \sum_{c=1}^{12} \sum_{t=2017q1}^{2023q4} \beta_{tc} RPZ_{it} + \gamma_i + \alpha_t + \varepsilon_{it} \tag{4}$$

With this approach, the treatment effect β_{tc} is estimated separately for every period t and every cohort c. There are 12 cohorts of LEAs based on the timing of the RPZ policy. The coefficients are then aggregated into quarterly weighted average and are presented in with grey lines in figure 9.

For all three outcome variables and in both full and contiguous border sample the extended TWFE and the TWFE with controls and quarterly coefficients give nearly identical results.

- **Sale listings**: For the overall sample, the coefficient becomes positive and significant 2017-2019, dropping back somewhat before rising consistently through early 2023. This is likely to represent the period following the tightening of the policy and its initial impacts. These findings are mirrored for the contiguous border sample, with no consistent trend before July 2021 and a notable rise following this change.
- **Rental listings**: In the overall sample, there is little difference between RPZs and non-RPZs 2017-2019, although some periods having a negative and significant effect. Following a spike during 2020 (possibly related to the covid19 pandemic), a negative and significant effect pertains for most of the period 2021-2023. For the contiguous border sample, the effects are similar, with a fall-off post-covid again clear.
- **Rental registrations**: The results for rental registrations have a similar, if clearer, pattern to rental listings. In the period before 2021, the impacts are negative (but with seasonal spikes around Q3 of each year

when third-level education returns), followed by an increase during covid19 and then a notable decrease following this period.

5.4 Heterogeneous effects

As described above, we can extend our analysis to examine the impact of RPZs on room listings (as distinct from full properties) and registrations across household and corporate landlords. Room listings can help distinguish between two key effects of rent controls. On the one hand, rent controls may simply reduce mobility, with the rental stock largely constant but changing hands less frequently. Room listings should be unaffected by this, as they are not covered by rent controls. On the other hand, rent controls may reduce the rental stock; in this case, room listings would be affected, as the rooms to be rented would be removed from the rental stock.

In table 7, we present the estimates, using the full sample, for room rents (columns (1) and (4)), rental registrations by household individual landlords (column (2) and (5)), and registrations by company landlords (column (3) and (6)); as before, the earlier specification is without controls, while the latter includes controls. Focusing on results with controls, we find that sharing lettings declined significantly in the period since July 2021 in areas with rent controls, evidence in favour of market exit rather than reduced mobility. In terms of the impacts across landlords, we find that the majority of the impact occurs for household/individual landlords: registrations are nearly 20% lower after rule changes in 2021. This is consistent with a number of hypotheses, including financial constraints varying by landlord type, and warrants further research.

Finally, in table 8 we present the results for the same outcome variables but using the adjacency sample. In relation to room rentals, our results hold: room rentals decline after controls are introducted and markedly so after the 2021 policy change. The same policy change affects both individual and company registrations, which are lower. In this sample, however, the impact on registrations of the tighter policy is greater for companies than household individuals. This may reflect differences between (larger) corporate landlords that dominate the 'always treated' sample and (smaller) corporate landlords in the adjacency sample.

Table 7: Heterogeneity, all LEAs

	(1) Daft share	(2) Reg. indv.	(3) Reg. comp.	(4) Daft share	(5) Reg. indv.	(6) Reg. comp.
RPZ pre-July21	0.076***	-0.095***	-0.043	0.003	-0.057***	0.010
	(0.016)	(0.009)	(0.027)	(0.018)	(0.009)	(0.030)
RPZ post-July21	-0.504***	-0.176***	0.121***	-0.455***	-0.195***	0.064
	(0.028)	(0.013)	(0.037)	(0.029)	(0.014)	(0.039)
Rent price				0.992***	-0.235***	-0.628***
				(0.101)	(0.047)	(0.126)
ΔRent price				-0.641***	-0.000	0.133
				(0.116)	(0.045)	(0.117)
Sales price				-0.146**	-0.217***	-0.050
				(0.065)	(0.031)	(0.089)
ΔSales price				0.014	0.075***	0.002
				(0.058)	(0.027)	(0.077)
Unempl. rate				-2.108***	0.870***	2.277***
				(0.354)	(0.190)	(0.536)
ΔUnempl. rate				2.591***	0.452	-3.698***
				(0.688)	(0.330)	(0.988)
Constant	2.966***	4.329***	1.926***	-1.642**	8.401***	6.422***
	(0.006)	(0.003)	(0.008)	(0.700)	(0.338)	(0.972)
Observations	9,296	9,296	9,296	9,296	9,296	9,296
R-squared	0.940	0.941	0.786	0.941	0.942	0.788
Fixed effects	Time+LEA	Time+LEA	Time+LEA	Time+LEA	Time+LEA	Time+LEA

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

6 Conclusions

In this paper, we have explored the introduction of rent stabilisation measures in Ireland after 2016. In particular, we examine whether these rent controls reduce supply, by bringing about exits from the market. To do this, we combine data on sale listings, rental listings and rental registrations, as well as data on room rent listings, and registrations by landlord type. Our approach uses a local electoral area level panel data set to identify whether there was an increase in the sale of properties, and a decrease in rental listings and rental registrations with the regulator after rent controls were applied. The setting is a useful one, both because of the specific design of rent controls in Ireland (applying across tenancies) and due to the change in strictness of allowed rent increases in July 2021, after which real rents could not rise. With over 160 districts and quarterly data for almost 15 years, our setting has rich variation in the timing of application of rent controls. These are not random, however, so to control for otherwise unobserved factors, we include both control variables and use a contiguous border set-up, limiting the sample to only those RPZ and non-RPZ districts that border a district

Table 8: Heterogeneity, contiguous border LEAs

	(1) Daft share	(2) Reg. indv.	(3) Reg. comp.	(4) Daft share	(5) Reg. indv.	(6) Reg. comp.
RPZ pre-July21	-0.062**	-0.071***	-0.091**	-0.063**	-0.051***	-0.066
	(0.028)	(0.017)	(0.044)	(0.029)	(0.017)	(0.046)
RPZ post-July21	-0.491***	-0.133***	-0.168***	-0.457***	-0.139***	-0.161***
	(0.041)	(0.020)	(0.052)	(0.040)	(0.020)	(0.053)
Rent price				0.701***	-0.198***	-0.146
				(0.158)	(0.074)	(0.215)
ΔRent price				-0.324**	0.055	0.153
				(0.160)	(0.076)	(0.201)
Sales price				-0.337***	-0.219***	-0.205
				(0.121)	(0.052)	(0.162)
ΔSales price				0.078	0.068	0.256**
				(0.099)	(0.042)	(0.126)
Unempl. rate				-4.197***	0.497	1.222
				(0.702)	(0.329)	(0.981)
ΔUnempl. rate				4.763***	0.710	-1.961
				(1.158)	(0.550)	(1.618)
Constant	2.726***	4.188***	1.601***	2.672*	8.030***	4.813**
	(0.008)	(0.004)	(0.012)	(1.498)	(0.658)	(2.019)
Observations	4,424	4,424	4,424	4,424	4,424	4,424
R-squared	0.902	0.919	0.690	0.904	0.920	0.691
Fixed effects	Time+LEA	Time+LEA	Time+LEA	Time+LEA	Time+LEA	Time+LEA

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

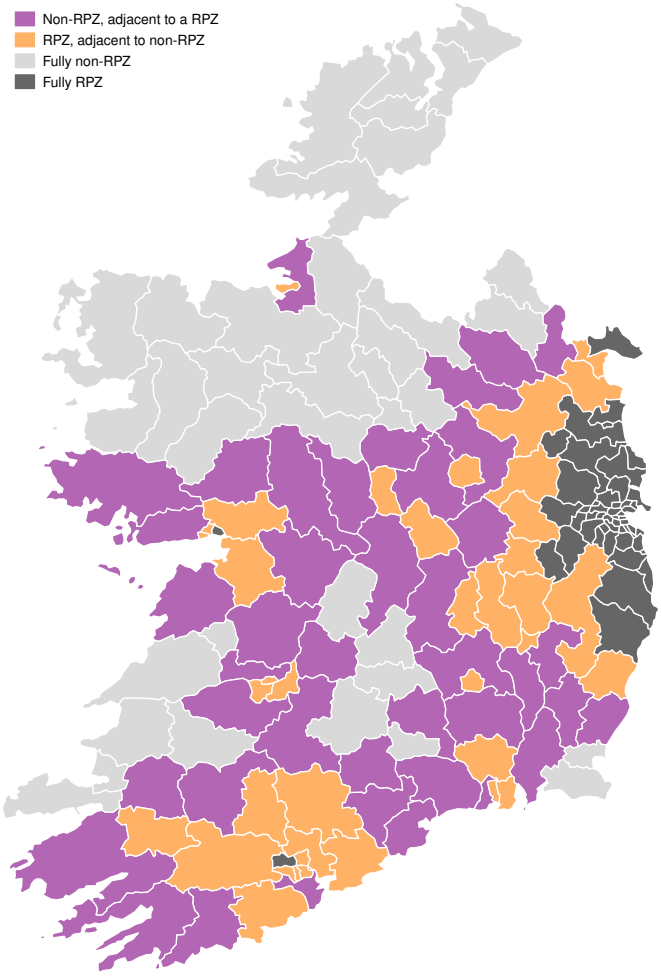
of the other type. We also use staggered treatment approaches, given the nature of treatment.

Across all our specifications, we consistently evidence that rent controls, especially after they were tightened, brought about market exits, with RPZ status associated with significantly fewer rent listings and registrations and significantly more sale listings. An analysis of room rentals supports the hypothesis of market exit over reduced mobility: room rentals were unaffected by rent controls but the volume of room listings fell dramatically with tight rent controls. These findings hold in the full and restricted samples, with and without controls for local housing market and wider economic conditions and using staggered DiD approaches. We also find evidence that household (i.e. non-corporate) landlords are driving the results, although this differs across the full and restricted samples.

There are a number of implications of our research for policy. First, in line with other studies, our findings suggest that supply-side effects of rent controls are evident in Ireland. This is measured as less rental supply and more sales in rent controlled areas. In particular we find this dynamic to have been more prevalent after the rules were more strictly calibrated in

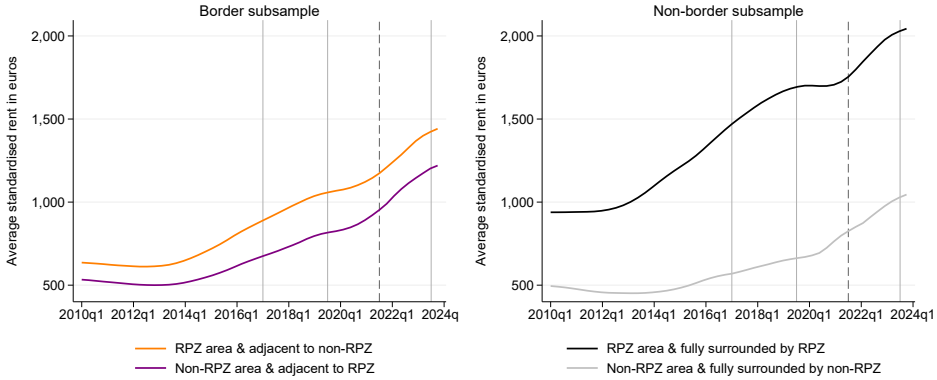
2021, prohibiting any real increases in rents but allowing positive capital gains through sale. This suggests that the impact of these rules is very much dependent on the calibration with supply side effects materialising most when real returns are negative. Policymakers looking to deploy these tools must be very cognisant of the balance between the impact of supply side externalities and the benefits to tenants on the affordability side.

Figure 3: Local Electoral Areas by there RPZ status and RPZ adjacency in 2021



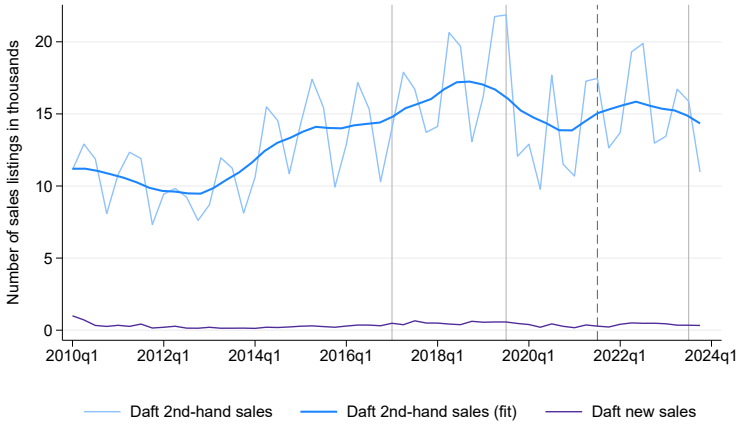
Source: Analysis of RTB data.

Figure 4: Average Standardised Rents Across RPZ Groups



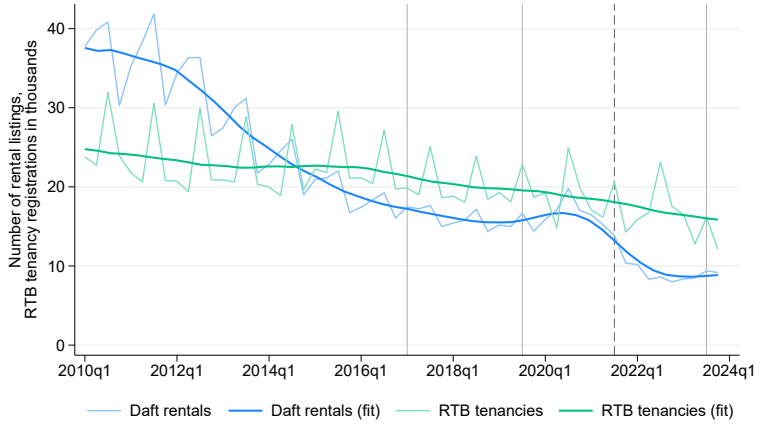
Source: Daft.ie.

Figure 5: Total number of sales listings on Daft.ie



Source: Analysis of daft.ie dataset

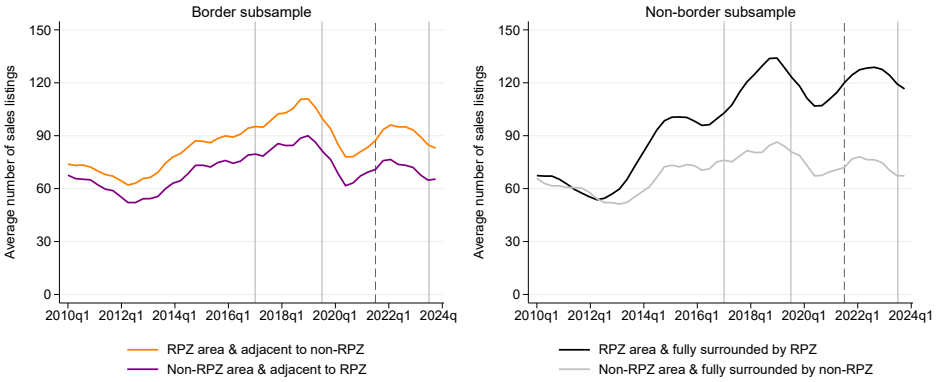
Figure 6: Total number of rental listings on Daft.ie and new tenancy registrations at RTB



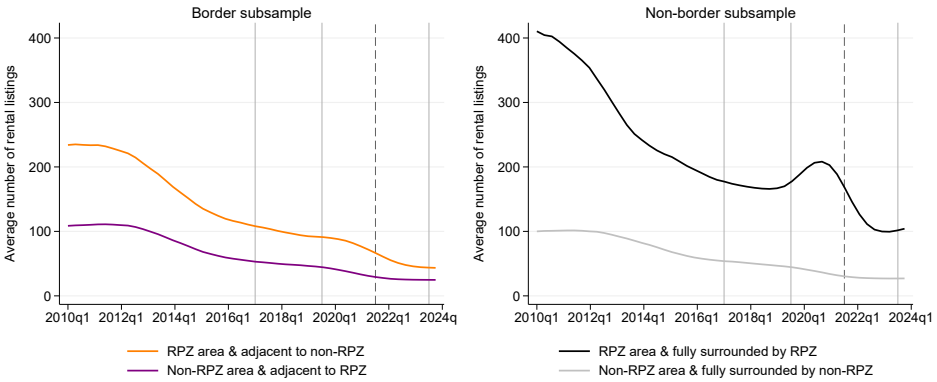
Source: Analysis of daft.ie and RTB datasets.

Figure 7: Main outcome variables by adjacency status

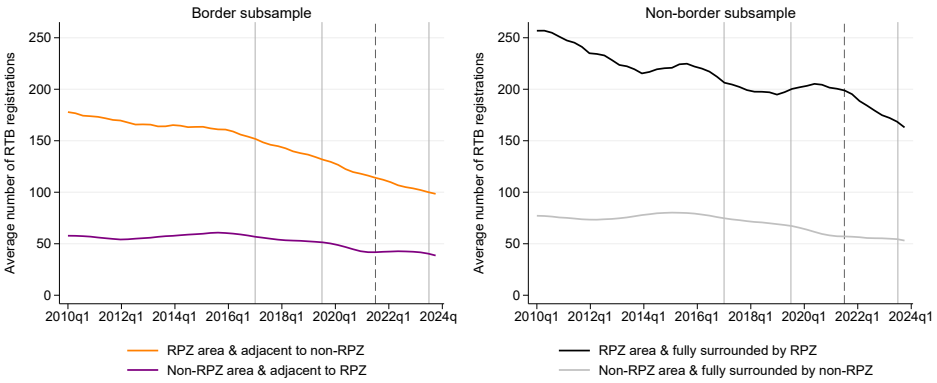
A. Daft.ie second-hand sales listings



B. Daft.ie rental listings



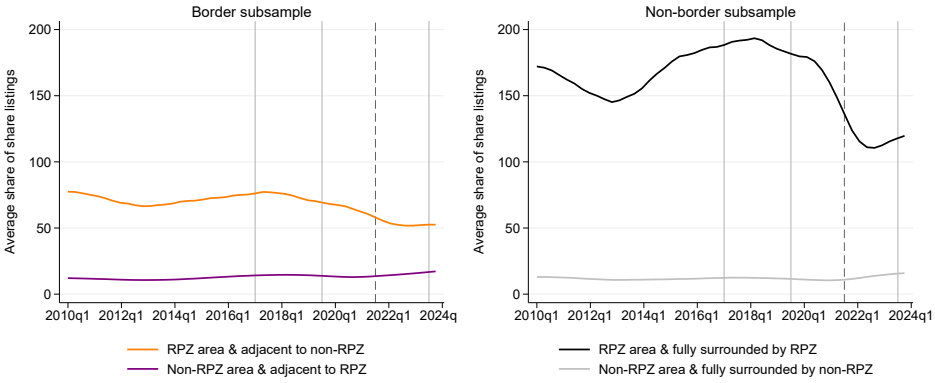
C. RTB tenancy registrations



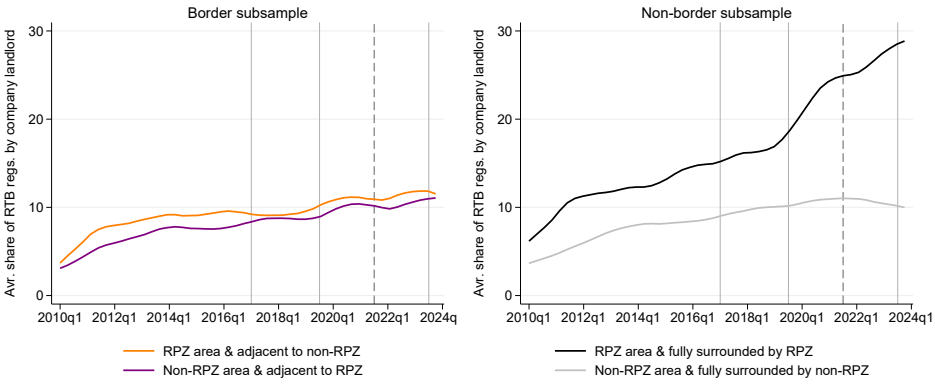
Source: Daft.ie and RTB

Figure 8: Other outcome variables by adjacency status

A. Daft.ie sharing rental listings



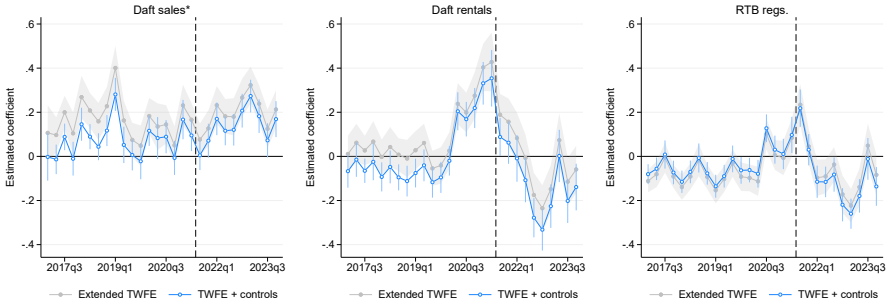
B. Share of company landlords



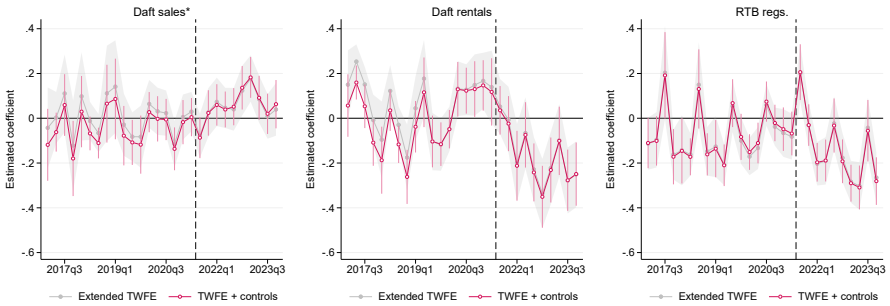
Source: Daft.ie and RTB

Figure 9: Quarterly dynamic effects, all LEAs

All LEAs - With controls



Contiguous border sample



Coloured vertical lines represent 95% confidence intervals of TWFE regression with all six controls included and robust standard errors.
Shaded area represent 95% confidence intervals of extended TWFE regression (Wooldridge, 2021) without controls and using clustered standard errors.
Dashed vertical line represent change in RPZ policy in July 2021.

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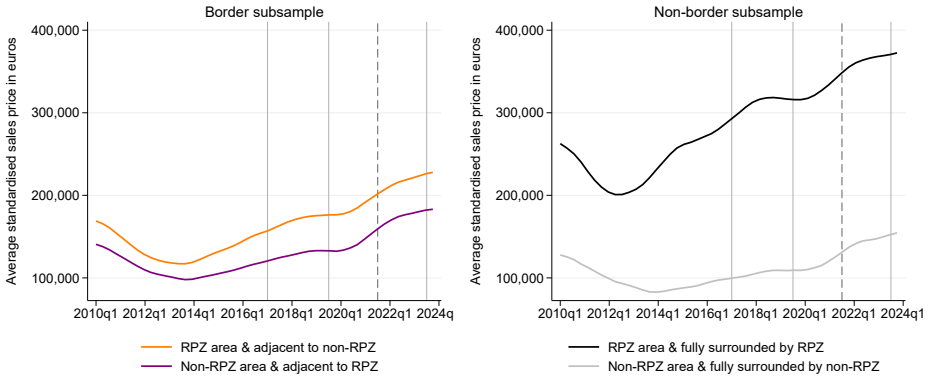
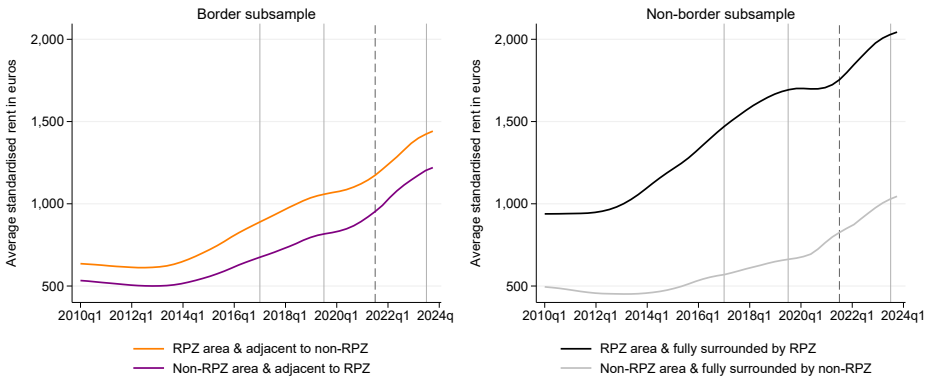
Annex

Figure 10: Control variables by adjacency status

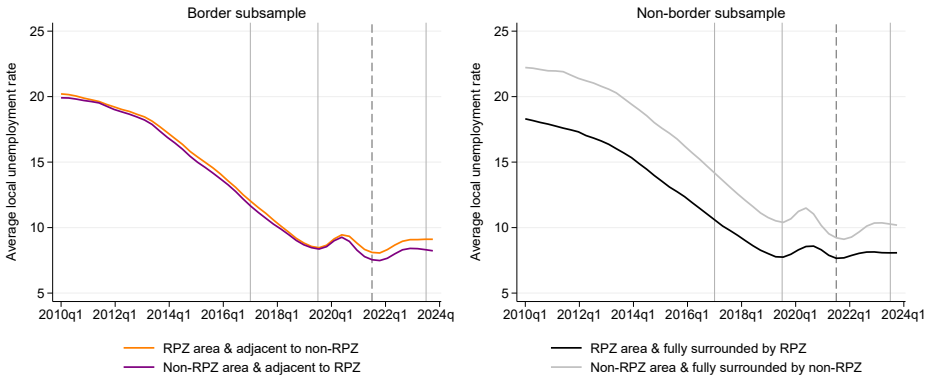
A. Sales price index



B. Rental price index



C. Local unemployment rate



Source: Price indices from Daft.ie data. Unemployment rate estimates based on Central Statistics Office Live Register, and 2011, 2016 and 2022 Census.
Note: Unemployment based on the Census self-declared status, which is higher than the official unemployment rate based on ILO definition.