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Price regulation, inflation, and nominal rigidity in housing rents[☆]

Conor O'Toole [a,b,*], Maria Martinez-Cillero [a,b], Achim Ahrens [c]

[a] Economic Analysis Division, Economic and Social Research Institute, Ireland
[b] Department of Economics, Trinity College Dublin, Ireland
[c] Public Policy Group, ETH Zürich, Switzerland

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ABSTRACT

In this paper, we explore the impact of a 4 per cent inflation control on price changes for rental contracts in Ireland. Testing across the price distribution, we explore the cap's impact on the share of the market experiencing: 1) a price decline; 2) unchanged rents (nominal rigidity); 3) a positive growth rate below the cap; 4) the maximum allowable growth (4%); and 5) growth above the cap. Our identification strategy uses a contiguous border difference-in-difference approach on a novel property-level tenancy panel. We find the overall rent growth rate fell by 1–2 percentage points following the regulations. However, we find different impacts across the distribution: not all high growth rates converged to the cap as the distribution generally shifted to the left and nominal rigidity rose. Furthermore, we find some inflationary pressures on rents that previously were unchanged as landlords priced up to the cap. Heterogeneous effects by company or household landlord are evident.

1. Introduction

The economic dynamics of price inflation for rental housing has long been complicated by two aspects of rental markets. The first issue relates to rent control regimes aimed at limiting price rises, boosting tenant affordability and ensuring security of tenure (Lind, 2001; Haffner et al., 2008). The research on the costs and benefits of rent control measures is extensive, with considerable focus on investment and housing quality (Sims, 2007; Arnott and Shevyakhova, 2014), community composition (Sims, 2011), the impact on unemployment duration (Svarer et al., 2005), tenancy length (Munch and Svarer, 2002) and homelessness (Early and Olsen, 1998). Haffner et al. (2008) also explored the degree to which the balance of tenant and landlord rights matter for the efficacy of regulations. Diamond et al. (2019) find that rent controls impact tenant mobility and drive up rents in the long run.

The second issue in housing rental markets is the existence of nominal rigidities where prices are unchanged for the same property from period to period. The impact of price rigidities in housing is critical in terms of the broad debate about price stickiness and the impact of monetary policy changes. In theory, central bank interventions do not have a full pass through into inflation because prices do not respond sufficiently quickly (Kehoe and Midrigan, 2015) or rigidities prevent price changes. Given the high share of the household budget that is allocated to housing,[1] the extent of nominal price rigidities in the housing market is a critical aspect in understanding the transmission of monetary policy and the measurement of inflation (Ambrose et al., 2017, 2018; Diewert et al., 2009; Dougherty and Van Order, 1982).

Despite the extensive literature, there are few studies which directly quantify either the impact of rent controls on rental price inflation across the price distribution and the degree of nominal rigidity in the housing market. This paper addresses this gap in the literature. In order to do this, we exploit a quasi-natural experimental setting before and after the introduction of rent controls in the Republic of Ireland in 2017. As a result of the regulations, certain local electoral areas (LEAs) in the country were classified as rent control areas with other LEAs remaining unclassified. In the rent control areas, the rate of nominal rent increases were limited to 4 per cent annually. We use regulatory micro-data on tenancy agreements in Ireland at the property-level taken from the supervisory returns collected by the Irish rental regulator, the Residential Tenancies Board (RTB). For the purpose of this study we extract a property-level panel dataset. Our main identification strategy

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* Corresponding author at: Economic Analysis Division, Economic and Social Research Institute, Ireland.
E-mail addresses: conor.otoole@esri.ie (C. O'Toole), maria.martinezcillero@esri.ie (M. Martinez-Cillero), achim.ahrens@gess.ethz.ch (A. Ahrens).

[1] Various estimates suggest that renters face high housing costs relative to income internationally. A summary of the literature can be found in Corrigan et al. (2019) with US examples in Quigley and Raphael (2004). Dutch examples in Haffner and Boumeester (2014) and the Turkish case in Aysoy et al. (2014).

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uses a contiguous border design difference-in-difference which limits the control and treatment groups to geographically bordering areas.

Our empirical testing is as follows. First, we directly test the impact of the controls on rental growth to explore whether the regulations slowed price inflation (a non trivial result as even if the controls trimmed the tail of the distribution this does not necessarily mean the overall level might fall). Second, we go further than the existing literature to assess the implications of the regulations on the share of the market experiencing differing growth rates. For example, landlords could opt to set prices: (a) above the 4 per cent maximum; (b) at the cap; (c) lower than the cap but with positive growth; (d) leave rents unchanged (nominally rigid) or (e) reduce rents. Which of these options they choose is likely to depend on their bargaining power in the market, the ability to recoup the cost of tenant turnover, and the desire to protect real returns by maximising rental growth. Our research design to identify these effects uses a multinomial logit difference-in-difference approach which splits the distribution into these five groups. We can then test the relative likelihood of being in each of the groups, as compared to growing above the 4 per cent cap. We also have unique data on the type of investor which allows us consider whether the landlord is a company or household. We can use this to test for heterogeneous effects that may arise from the different constraints these landlords face.

In relation to this contribution, our research is closely linked to a number of studies. Two recent studies use micro data to explore changes in German rent regulations in 2015 (Breidenbach et al., 2019; Mense et al., 2018) with both finding the regulations had a negative impact on rental inflation. Sims (2007) considered the impact of ending rent control in Massachusetts. He found that rent control had reduced prices. More recently, Fitzenberger and Fuchs (2017) looked at the impact of rent regulation on prices in Germany using a quantile regression while Oust (2018) found little affect of the removal of rent control in Oslo on the level of rents. Another notable study is Diamond et al. (2019) which uses a law change in San Francisco to test the impact of regulation on tenant mobility and prices levels. As our focus is on the price growth distribution, we feel our research is complementary to this study. By exploiting the quasi-natural geographic variation of our panel data which is unlikely to be impacted by omitted tenant variables, and exploring the distributional effects in more details, our research builds on these studies.

Indeed, our paper would be closest to Fitzenberger and Fuchs (2017); so it is important to document the innovations in our research. First, we specifically look at the distribution of growth rates above and below the cap of 4 per cent. They do not narrow in on the distribution at the point of the cap. Indeed, they do not show how many tenancies would be explicitly affected by the cap in Germany which we do (above and below 4 per cent). Building on this, testing how the growth rates move differently across the distribution overall, and in particular depending on the type of landlord, is a novel aspect of our paper. This includes our explicit consideration of the issue of nominal rigidity which is an important economic concept in its own right. Thus the distributional assessment we undertake is different to the quantile approach in their paper. The policy guiding their assignment rules depends on the level of rent above and below certain thresholds: it is more difficult to know whether the rules bind in the case of their law change whereas our rent cap applies to all tenancies regardless of the starting rent level.

Second, we exploit a different identification strategy. While Fitzenberger and Fuchs (2017) explore a national law change which impacts different duration tenancies but is common across all geographies, we use geographic variation in assignment of the rent control rules as our identification variation. If there were other aspects of the legal environment that would impact the length of tenancy, but were also correlated with the introduction of the German reform, then this may affect their identification. Finally, the case study of Ireland is useful as it is a market with very limited rental protections which is in contrast to the strongly protected rental market in Germany. Thus our findings should give a different country context and has a distinct contribution.

Another novel aspect of our research is the focus on nominal rigidity. Our third empirical test is to directly consider the impact of rent controls on nominal price rigidity. There are two potential competing hypotheses as to how landlords may react to the regulations. First, nominal rigidity may rise if landlords are not able to recoup the cost of tenant turnover. In this case, the cost of losing a tenant due to large rent increases outweighs the loss in real earnings growth with rises now capped by 4 per cent increases per annum. The second hypothesis suggests that nominal rigidity may fall if landlords increase rents by the allowed regulatory level in order to maximise cash flow from their investment. Disentangling these hypotheses is an empirical question. Three previous papers consider the issue of nominal rigidity in housing rents but none interact this with price controls. Genesove (2003) explored the presence of nominal rigidity in apartment rents in the US over the period 1974–1981, focusing on areas without rent controls in operation. He found a high share of properties displayed nominal rigidities (approximately 30 per cent). Shimizu et al. (2010) considered the stability of the Japanese CPI during the 1990's credit boom and focused on the degree of nominal price rigidity in housing as an explanatory factor. They found that 90 per cent of rents are unchanged annually and this depends on the level of the rent as compared to the market. Finally, Aysoy et al. (2014) used a national panel of housing units in Turkey to explore the degree of nominal rigidity over the period 2008 to 2011. They found that 31.5 per cent of rents did not change over time and that this was affected by tenant characteristics and search and moving costs.

A number of findings emerge. First, overall rent controls had a deflationary impact on the market with the inflation rate dropping by approximately 1–2 percentage points after the introduction of the regulations. Second, we uncover a considerable fall in the share of rent price increases above the 4 per cent limit consistent with the regulatory framework. These findings indicate that the regulations have been broadly effective in lowering the level of rental inflation in the classified areas. Considering the impact across the price distribution, we find that, relative to previously setting prices above 4 per cent, landlords were most likely to price at the regulatory maximum followed by just below the limit but with above zero growth.

In relation to the impact of rent controls on nominal rigidity, we find that the share of zero growth contracts increased after the introduction of the measures. An increase in no price change contracts may arise due to the risk of tenants leaving after a rent increase and such costs not being recoupable. Furthermore, the findings of the multinomial logit estimates present an interesting picture of the impacts of rent controls across the price distribution. We find that, relative to no change, the likelihood of price increases at, or just below, 4 per cent has increased after the regulations in the treated areas. This may be due to landlords attempting to protect the real value of contracts given their inability to reset rents between tenancies. While the overall effect of the policies has been to dampen rents, they have also caused inflation at other points in the distribution consistent with a protection of real return by landlords (a movement from previous unchanged to positive growth).

We find differences by landlord type with non-professional investors (individual households) more likely to have nominally rigid rents and also less likely to have increases at, or below, the maximum allowable. This may be driven by the fact that such households are unable to absorb tenant turnover costs as easy as professional companies. Overall, we find that, faced with a common, market-wide regulatory maximum, landlords react heterogeneously to set prices at different points across the distribution.

A final test that we include is the extent to which there has been an impact on tenancy turnover. We find that the number of agreements drops by 3 on average in treated areas which is a fall of 6.5 per cent. This may indicate (a) lower mobility as our data cover only new rents or (b) a push factor towards unregulated tenancies.

The rest of the paper is structured as follows. Section 2 presents an overview of the rent control legislation and the rental market in Ireland; Section 3 presents the data and summary statistics. Section 4 presents the analysis of the impact of rent controls on rent inflation. Section 5 considers the interaction between rent controls and nominal rigidity and Section 6 concludes.

2. Background and data

2.1. Rent controls and the rental market in Ireland

The onset of the 2008 financial crisis in Ireland led to a dramatic fall in house prices and rents which continued well into 2013. However, from 2014 onwards the economy began to recover and rental prices began to increase rapidly. A continued fall in unemployment, coupled with both rising demographic pressures, low housing supply and tighter mortgage credit conditions for prospective buyers from 2015 all conspired to put further price pressure on the private rental market.

In reaction to the growing public pressure over the cost of private rents, the Government introduced legislation in late 2016 aimed at limiting the rate of price inflation in private rental contracts. While rent controls are a feature of many markets internationally, and were used historically in an Irish context, the explicit inflation cap introduced by these measures represented a marked policy shift for the sector. Indeed, the impact of these regulations can be seen as largely unanticipated as while some pressure was building to introduce limitations, the policies were brought in quite suddenly which would not have left market participants with much time to react for the first designation.

The controls were introduced as part of the Planning and Development (Housing) and Residential Tenancies Act 2016. Under this provision, areas can be designated as Rent Pressure Zones (RPZs) by the Minister with responsibility for Housing, Planning and Local Government. This designation limits rental inflation in these areas to a maximum of 4 per cent annually (in a similar vein to the German 10 per cent cap as outlined in Deschermeier et al., 2016). This limit is applied to rents agreed at the start of the tenancy (i.e., the previous rent on the property, or rent history, is used as the anchor for allowable rent increase) and to rents reviewed in an ongoing tenancy. Two geographic boundary areas can be designated as Rent Pressure Zones: Local Electoral Areas (LEAs) or local authority areas (LAs). At these levels, there are two criteria for determining whether or not the area can be classified as a rent pressure zone: First, the rent inflation must have grown at a rate of 7 per cent or more on an annual basis in four of the previous six quarters. Second, the average rent in the current quarter must be higher than the average national rent (i.e., the Rent Index national standardised rent).

Two exceptions to the 4 per cent maximum increase are currently allowed in the legislation. First, properties new to the rental market (i.e. properties without a rent history for the previous two years relative to when the area in which the property is located was designated an RPZ) are exempt. Second, properties that have experienced a substantial change in the nature of the accommodation (i.e., renovations or reforms of such nature that they involved significant alterations that increased the value of the property) are also exempted. This provides some incentive for landlords to invest and maintain their properties which has been a criticism of the rent control regimes in other countries.

In total, five local authority areas and 16 other additional LEAs around the country have been declared RPZs by Q3 2018 (see Appendix A for details). Most of the current RPZs were designated between December 2016 and January 2017, although two more designation rounds have taken place since as a result of applying the criteria described (the specific dates of each designation are also provided in a table in Appendix A). Visualisation of the declared and undeclared areas are presented in the maps in Fig. 1.

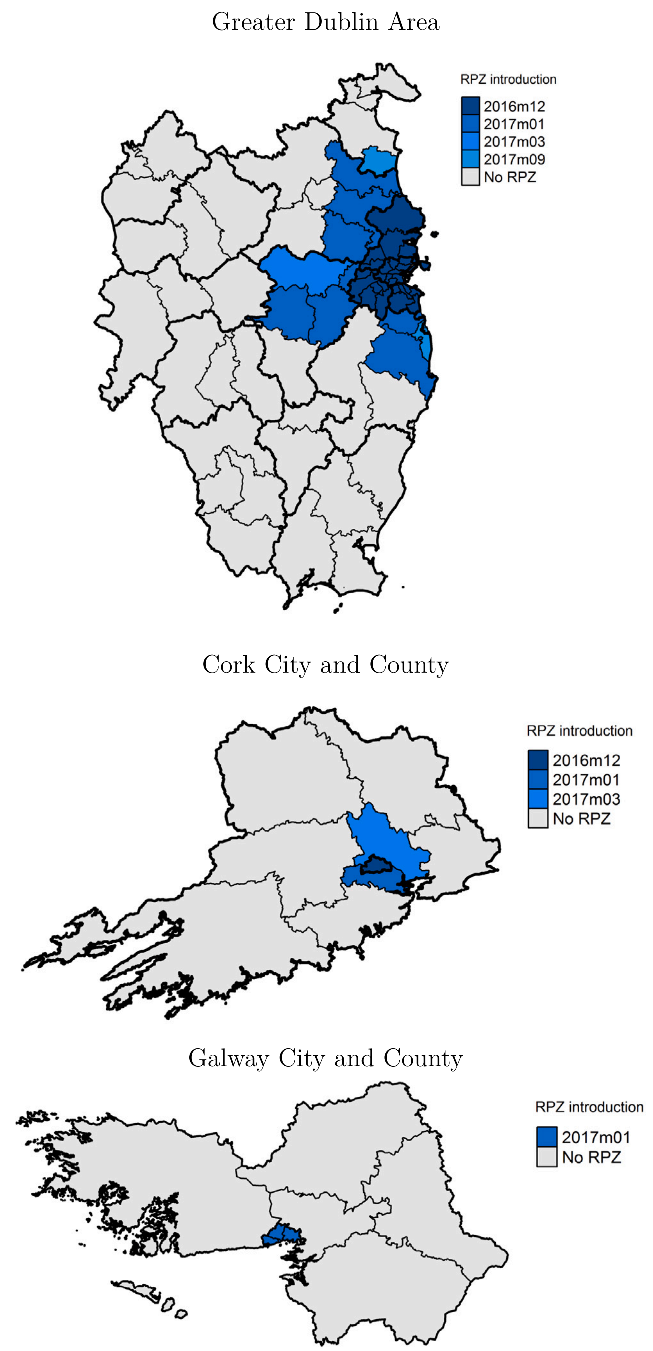

Fig. 1. Areas Classified as Rent Pressure Zones.

2.2. Data overview

The data used for this assessment are provided by the RTB, the state regulator for the Irish private rental sector. By law, all new tenancies in Ireland must be registered on commencement with the RTB and details of the contract provided. This includes the level of the rent and characteristics of the dwelling such as number of bedrooms, number of tenants, address, and other features. The information does not include characteristics of the tenants. These data have been collected and monitored by the RTB since quarter 3 (Q3) 2007 and a tendency level database of all newly registered tenancies is available from that date onwards. This provides a dataset of approximately 1.3 million registered tenancy agreements from Q3 2007 until Q3 2018. While a majority of the data relate to new tenancies, some tenancy renewals are also contained in the dataset. Renewed tenancies are those re-registered with the RTB after they have existed for between 4 and 6 years (Part IV renewals). There is no requirement in Ireland for tenancies to be re-registered on an annual basis or where the tenancy continues under four years. These data therefore do not measure the stock of outstanding rental contracts and are more in line with the data used in Ambrose et al. (2015) to measure new market rental prices.

For the purpose of this research, a specific extract from the database was used. Our interest was in looking at the degree of price stickiness as well as the impact of rent controls on price inflation. We therefore required sufficient data at a within-property level to calculate growth rates. A number of transformations and data cleaning steps are required. The RTB database contains first an "Eircode" (i.e., Irish postal code) which is a building ID. However, this does not provide a unit-specific ID for multi-unit dwellings, i.e., does not distinguish between properties that might share the same Eircode (e.g. apartments in the same building complex). We combine Eircodes with information from the address field in the dataset to match buildings to properties for example the apartment or unit number within a building. This provides us with a unique property identifier. We identify 201,500 distinct properties that appear more than once (these properties are associated with 614,004 tenancy agreements). Using the property identifier, we can calculate compound annual growth rates for each property using the formula:

$$CAGR_{it} = \left(\frac{rent_{it}}{rent_{it-s}}\right)^{12/(t-s)} - 1 \tag{1}$$

where s is the time gap between the two tenancy agreements in months. This computation yields 396,251 property-specific growth rates which are used in the econometric estimations.[2]

2.3. Identification strategy

Once we develop a property level dataset, we can assign each property into LEAs, and, therefore, identify those properties that are located in RPZs and those that are located in non-RPZ areas. This provides us with our treatment (RPZ) and control (non-RPZs) definitions. Our first comparison group that we present in this paper is to simply compare RPZs to the full basket of non-RPZs. However, one concern for a standard difference-in-difference approach is that the treatment is non-randomly assigned and that their could be a structural difference in the growth trend in control and treatment groups. This is quite possible in our case as the assignment to RPZ status is dependent on the growth and level of rents.

To attempt to address this concern, we follow a contiguous border design approach and drop tenancies in 72 local electoral areas who do not share a common geographic border with any of the treated areas[3] We also drop the LEA areas in Dublin (mainly around the city environs) which border only other Dublin LEAs.[4] These areas do not ever border non-RPZs after the policy. They are also the area around the City centre which is quite unique as a rental market thus may be unlikely to be comparable to the areas bordering Dublin county in terms of economic and geographic similarities. The LEAs bordering RPZs can clearly be seen in Fig. 1.

The aim of this approach is to compare markets that are as similar as possible across every dimension and limiting the control group to this more narrow comparison of geographically close areas should allay some of aforementioned concerns. Therefore throughout this paper, we use two groups in our empirical results: (a) Full sample (Full); (b) border design sample (Border).

Table 1
Property characteristics comparison.

	Total sample (%)	Property sample (%)
1 Bedroom	16.6	15.2
2 Bedrooms	36.9	36.1
3 Bedrooms	31.3	33.7
4 Bedrooms	12.8	12.7
5+ bedrooms	2.4	2.2
Apartment	44.0	42.5
Detached	10.2	5.8
Semi-detached	25.1	29.3
Terrace	14.2	16.7
Other property	6.4	5.7
Part house	1.4	1.0
1 Tenant	47.6	45.4
2 Tenants	35.5	36.4
3 Tenants	7.7	8.4
4+ Tenants	6.5	7.4
1–6 months tenancy	8.2	8.3
7–9 months tenancy	4.7	4.4
10–12 months tenancy	66.4	67.6
1+ year tenancy	20.7	19.8
Fortnightly rent	0.2	0.3
Monthly rent	86.9	88.7
Yearly rent	1.3	1.0
Quarterly rent	0.1	0.0
Renewal	5.6	7.5

2.4. Summary statistics

Since a number of observations had to be dropped when constructing the property level dataset, there may be concerns that the data omission induces a selection bias, and issues concerning the representativeness of the property level dataset. To rule out this concern, we present summary statistics of the total sample and the reduced property level sample in Table 1. Since there are not substantial variations between the two datasets, we conclude that the property level dataset is representative. Most tenancies correspond to two or three bedroom properties (68 per cent) and nearly half of the registered properties are apartments. The most frequent tenancy length is between 10 and 12 months.[5] The property sample has a slightly higher share of renewal properties at 7.5 per cent than the overall sample at 5.6 per cent. A simple proportional t-test on the share of observations in the property sample relative to the other sample observations shows a statistically significant difference. This is due to the fact that renewal properties are more likely to observed twice in the property sample, therefore allowing us to calculate a growth rate. Almost half of the rental agreements included only one tenant, while a further 35 per cent of agreements included two tenants. This highlights the high demand for smaller units in the Irish rental market. It should be noted that any tenancy characteristics presented here are *ex ante*: capturing the information at the point of agreement.

It must be noted regarding our sample that, as the data in our database cover new tenancies, we do not observe how long a tenancy stays in place other than cases where tenancies become re-registered after 4 years as part IV renewals. Therefore, if we observe a property again in a short space of time, it is likely to be due to a new tenant coming in. Our sample therefore is likely to have more turnover properties than the overall sample.

[2] We have discarded the 1 per cent of the smallest and largest growth rates to avoid the results being distorted by outliers as standard.

[3] Please contact the authors for the list of the areas dropped.

[4] In alphabetical order, these are Ballyfermot–Drimnagh (6), Ballymun (7), Beaumont–Donaghmede (9), Blackrock (6), Cabra–Finglas (7), Clontarf (6), Crumlin–Kimmage (6), Dundrum (7), Dún Laoghaire (8), Howth–Malahide (8), North Inner City (8), Pembroke–South Dock (8), Rathgar–Rathmines (6), Stillorgan (6), Tallaght Central (6), and Templeogue–Terenure (6).

[5] A simple t-test of the differences in the *ex ante* contract duration shows a statistically significant difference of only half a month.

If turnover properties are likely to provide an opportunity for landlords to increase the rent (the well know residency discount; see Fitzenberger and Fuchs, 2017), then our sample should have a higher growth rate than that for the overall population of renters in Ireland. Therefore, it could be expected that any reduction of the growth rate found by our analysis of RPZs is likely to bias the effects to be the high end of the effects faced by tenants, i.e., is the effect on new market rents not average rents; if our sample had more above 4 per cent growth rates relative to the stock of rents over time, then this will provide a larger effect in our data.

To compare rental price inflation in Ireland across the price distribution as well as across RPZ and non-RPZ areas, we present a number of comparisons. Fig. 2 presents the mean growth rates across RPZ and non-RPZ areas for both the full sample and the more closely aligned contiguous border design sample. The relative trend in the means appear to be similar across both RPZ and non-RPZ areas, up until 2016 when the regulations came into force. While there is some larger divergence in 2014, the common path in 2016 breaks down at the same time as rent pressure zones were introduced suggesting a correlation between the rent controls and changing price dynamics.

To illustrate the change in the rental price distribution before and after the introduction of RPZ legislation, Fig. 3 presents the percentage of contracts across the price distribution for each 1 per cent of the growth distribution. We have censored the data at growth rates plus or minus 30 per cent for illustration. The time period presented is from Q2 2015 to Q3 2018 which is split into the periods before and after the introduction of rent controls. The classification is time varying so that, within the period Q2 2015–Q3 2018, each observation is classified into the RPZ group at the point when each area was designated as an RPZ. Both the treated (RPZ) and non-treated (non-RPZ) areas are presented separately, thus highlighting the temporal variation within each area. Data are presented for both the full and border samples.

Fig. 3 suggests that even before the introduction of the RPZ legislation, a high degree of nominal rigidity existed in the data with approximately 15 per cent of RPZ areas not changing their rental levels and 30 per cent in the non-RPZ areas. After the introduction of rent controls, the share of nominal rigidity rose in RPZ areas to just under 20 per cent with no discernible change in the non-RPZ areas. Of note also is the bunching at 4 per cent in the RPZ areas after the introduction of rent controls. This is the legislative allowable increase for contracts in these areas. Given the data, it could be the case that the 4 per cent is acting as an anchor effect which reinforces the point that the parameterisation matters: growth rates are drawn to this point. These patterns are not evident in the control areas. It is also noteworthy that the share of growth rates between 0 and 4 have increased after the legislation was introduced. As the Irish regulations are first generation rent controls, in the sense that no reset is allowed between tenants,[6] this may have increased the share of landlords increasing rents to ensure they maintain real returns (setting the price growth as high above inflation as possible).

3. How do rent controls impact inflation rates?

We begin our empirical examination of the impact of rent controls on price inflation by exploring the direct effect of the treatment on the growth rate in the areas of Ireland that were declared RPZs. We first outline our identification strategy then present the results of our overall estimates and then subsequently undertake some estimates across the distribution of rental growth.

3.1. What happened after rent controls overall?

Our first *a priori* expectation is that the inflation rate should fall after the introduction of the rules. To test these effects, we use a standard difference-in-difference fixed effects model to test for the average treatment effect on the treated, i.e., the change in the inflation rate following the introduction of rent controls. The specification is as follows;

$$\Delta R_{ijt} = \beta RPZ_{jt} + \gamma_j + \tau_t + \mathbf{x}'_{ijt}\delta + \epsilon_{ijt} \tag{2}$$

where ΔR_{ijt} is the annualised change in the rent of property i in time t in local electoral area j. This is determined as a function of an RPZ dummy, which is a time and LEA varying binary indicator which is 1 if an area has been classified as an RPZ and zero in other time periods. To saturate out the time varying effects and the area specific effects, we then include LEA level fixed effects, (γ_j), and time fixed effects (τ_t).[7] Time fixed effects include a dummy for each specific calender month from May 2015 to September 2018 (with April 2015 being the omitted category). We then include a vector of property-level control variables in a similar vein to a hedonic regression to standardise the growth rates across property types. The variables included in this vector are: the number of bedrooms, the number of tenants, the tenancy length, and the housing type (apartment, detached house, semi-detached house, terrace, other). As the dependent variable is a compound annual average growth rate, the periods across which the growth rates are potentially different. We therefore include three variables to capture the time gap between the observations in the panel. We include the log and level of the time gap and the time gap squared as control variables.[8] We also control, in all specifications for a dummy if the tenancy was a renewal and a dummy for whether the landlord was a company. Given our specification, we would therefore expect that $\beta < 0$ and is statistically significant.

A standard challenge in impact studies such as these is determining the time window to be used before and after the policy. The measures in Ireland were introduced in late December 2016 and in effect began to operationally impact the Irish market from January 2017 onwards. Taking the time period following the onset of the regulations for which we have data, this leaves a window of 7 quarters between Q1 2017 and Q3 2018. Our main comparison that we undertake is to use a symmetric time window thus 7 quarters before and after the policy intervention, Q2 2015–Q3 2018. However, to ensure that our estimates are not affected by the selection of the time window we also expand our analysis to cover the period Q1 2010 to Q3 2018 as a check.

The results are presented in Table 2. We include the main RPZ_{jt} indicator which varies by LEA and time, picking up the changing classification over time of the different areas. In columns (1) and (2) we present the estimates for the full sample for the broad and short time windows. In columns (3) and (4) we present the estimates for the contiguous border design sample for the long and short time windows.

In all four specifications, the RPZ dummy is negative and significant at the 0.1 per cent level. These results indicate a statistically significant reduction in rent inflation after the introduction of the regulations. The magnitude of the effect varies across the specifications. In the full sample, the coefficient is circa −2 percentage points for the shorter time period and −2.6 percentage points for the longer time window. The border design sample, which is likely to be the cleaner identification strategy, presents estimates of −2.2 percentage points for the longer time window and −1.1 percentage points for the shorter time window.

[6] First generation rent controls refer to a situation where the property is covered by the regulations and even if a tenant changes, rent increases are linked to the previous rent. Second generation controls relate to the protection of the tenant from rent increases. These allow increases to market rates if the tenant changes.

[7] We also run additional specifications which include a property-level fixed effect and the results are identical to those with the higher level fixed effects. Results are available on request from the authors.

[8] The results are not sensitive to the functional form or the set of controls included for the time gap. Other specifications can be provided as requested from the authors.

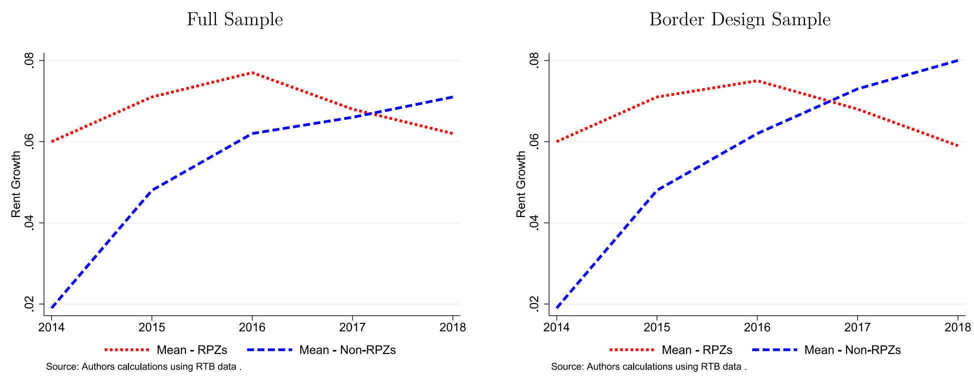

Fig. 2. Growth Rate Trends in Treatment and Control Areas.

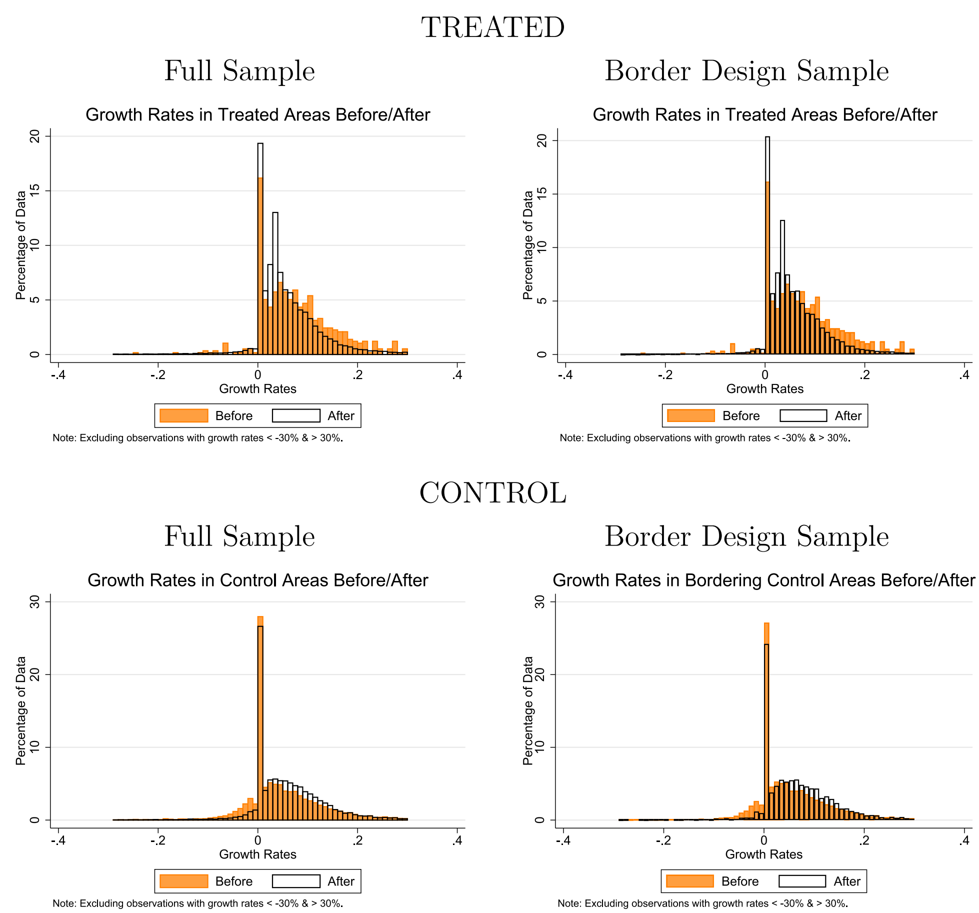

Fig. 3. Growth Rate Distributions — Before and After RPZs.

A critical question in terms of understanding the economic magnitude of these findings is to explore how large these estimates are given the prevailing inflation rates in the market prior to the onset of the regulations. Given that the regulations were introduced as a reaction to high rental inflation in the period 2015 and 2016, we consider the impact for this time period. The average inflation rate for all properties from Q2 2015 to the onset of the regulations in each LEA was approximately 7 per cent for both the full and border design samples. The estimated coefficient range of −0.01 to −0.02 suggest a reduction in the inflation rate of between 14 and 28 per cent. While this is a relatively wide range, both of these effects suggest a material reduction in the inflation rate following the introduction of rent controls.

3.2. Did the timing matter? an event analysis

To provide a more time specific assessment which does not require such a strict selection of the timing window, we undertake an event analysis. This check can also address any concern that rents were decreasing in RPZs anyway in a way that the initial rent gap controls in the section above do not control for. For brevity, we use here the Border Design Sample which is our benchmark identification sample.[9]

We address this analysis in two parts. First, we present a chart of the average growth rate across all RPZ areas centred on when they

[9] Similar findings hold for the full sample and are available from the authors.

Table 2
Testing for average effects.

	(1)	(2)	(3)	(4)
Company_{ijt}	−0.002**	−0.005***	−0.006***	−0.009***
	(0.001)	(0.001)	(0.002)	(0.003)
Renewal_{ijt}	−0.029***	−0.039***	−0.031***	−0.040***
	(0.000)	(0.001)	(0.001)	(0.001)
Tenants_{ijt}	0.004***	0.002***	0.003***	0.002***
	(0.000)	(0.000)	(0.001)	(0.001)
$\text{Tenancy Length}_{ijt}$	−0.000	0.000***	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
$\text{No of Bed Rooms}_{ijt}$	−0.000	−0.002***	0.000	0.000
	(0.000)	(0.001)	(0.001)	(0.001)
PT: Detached_{ijt}	0.000	−0.003	0.000	−0.003
	(0.001)	(0.002)	(0.002)	(0.003)
PT: Semi-Det-_{ijt}	0.001	−0.000	−0.000	−0.001
	(0.001)	(0.001)	(0.001)	(0.002)
PT: Terrace_{ijt}	−0.001	−0.001	−0.000	−0.000
	(0.001)	(0.001)	(0.001)	(0.002)
PT: Other_{ijt}	−0.000	0.001	−0.009***	−0.004
	(0.001)	(0.002)	(0.003)	(0.004)
RPZ_{jt}	−0.026***	−0.021***	−0.022***	−0.011***
	(0.001)	(0.002)	(0.002)	(0.002)
Observations	361,597	149,166	133,683	56,081
From	2010m01	2015m04	2010m01	2015m04
To	2018m09	2018m09	2018m09	2018m09
Sample	All	All	Border	Border
LEA FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Time Gap Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses
Standard errors clustered at time-LEA level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

qualified and taking a 12 month period before and after this point. This is presented in Fig. 4. It can clearly be seen that growth rate was relatively constant in RPZs for the 12 months before the policies and declined relatively quickly after assignment. The average differences is very close to our econometric estimate from Table 2 above of 1 per cent.

Additionally, we estimate a second, more formal, approach. We run a model, as presented in Autor et al. (2014), on a monthly basis assuming a constant group of RPZ areas and pull out the series of dummies differentiating the RPZs and non-RPZ groups. The model is presented below and is identical to Eq. (2) in terms of the control variables. However, the differences comes from the coefficients, $\beta_{k,RPZ=1}$ which is the different inflation rate between the subsequently assigned RPZ areas and the rest of the bordering regions for each time period in the data.

$$\Delta R_{ijt} = \sum_{t=Q2\,2015}^{N=Q3\,2018} \left(RPZ_i \times \mathbb{1}\{t=k\}\right)\beta_{k,RPZ=1} + \gamma_j + \tau_t + \mathbf{x}'_{ijt}\delta + \epsilon_{ijt} \quad (3)$$

The chart of the marginal effects of treatment ($\beta_{k,RPZ=1}$'s) for each month is presented below in Fig. 5. Two vertical lines have been included on the x-axis; the first is the initial RPZ qualification and the second is the month (only two months later) where the majority (46/48) LEAs have qualified. There is no statistically significant difference (at the 5 per cent level) between the groups beforehand. When all RPZs are qualified (second line), a statistically significant and negative effect emerges for nearly all months. Please note that at a monthly level there is volatility as frequently there could be low numbers of observations. It is clear from this approach that the drop in inflation occurred in line with the rent controls being introduced.

An interesting aspect of the introduction of any policy such as this is whether announcement effects can occur which makes market participants react in the period between the scheme being announced and taking regulatory effect. In our case, if landlords expected the cap to come in, they may have acted to raise rents before the regulations applied. If this is the case, we should see an increase in inflation in RPZ areas just before the rules. This event study regression above does not provide any evidence that rents significantly changed before the policy was introduced.

These findings are unsurprising in an Irish context as the policy was relatively unexpected to the market and the qualification criteria data needed to assess assignment status were not released to the market until after an area was set as an RPZ. There was also no gap between announcement of qualification and the activation of binding rent caps.

3.2.1. Further robustness checks on identification strategy

Our study to date has relied on a general difference-in-difference strategy. We motivated the use of this approach as the change in the parallel trends appears to be closely aligned to the introduction of the policies as well as using a contiguous border design approach. However, it could also be the case that economic influences could be different across the areas. To address this concern, we introduce an error correction term which accounts for the growth rate in each tenancy relative to the growth rate in the local electoral area.

More specifically the confounding issue could be related to the fact that the specific areas where the regulations were introduced were set as rent pressure zones specifically due to the level of rents being high in these areas. In these areas, the rate of growth may have been slowing before the regulations solely due to the fact that in these markets the growth rates were converging to their local equilibrium (having potentially grown rapidly in the period prior to the introduction to the regulations). While an ideal way to control for this would be to find some source of exogenous variation which would allow us to purge the estimates of these biases, we have not been able to find such variation in our data. As an alternative, we run a range of robustness checks. First, we simply control for the level of rent (in lagged form) to test whether the effect of the RPZ dummy is dependent on the initial level of the rent. Second, we introduce a disequilibrium term into the rental growth regression which allows each specific rental growth price to be a function of how far the initial average is from the market average. The specification is as follows:

$$\Delta R_{ijt} = \beta RPZ_{jt} + \gamma_j + \tau_t + \mathbf{x}'_{ijt}\delta + \omega\left[R_{ijt-1} - \bar{R}_{jt-1}\right] + \epsilon_{ijt} \quad (4)$$

where R_{ijt-1} is the level of the rental contract i in LEA j in period $t-1$ and $\bar{R}_{jt-1}$ is the average inflation rate in LEA j in period $t-1$. Given our panel data are unbalanced, the lagged figure refers to the previous period in which that rental property is observed. The number of observations falls in this case. The parameter ω should capture the degree to which each rental growth rate is converging at a slower or faster rate due to how much it deviates from the local market conditions. For brevity, we run this specification for only the limited sample as in column (4) of Table 2. The results are presented in Table 3. Column (1) reproduces the estimate without controls for comparison purposes. Columns (2) and (3) include the new proposed controls. It is clear across all the specifications that the main RPZ effect is negative, statistically significant and similar in magnitude regardless of the control. We are therefore reasonably confident that the findings are not affected by this specific source of potential bias. We therefore proceed without these controls for the rest of the analysis as they case a loss of data due to the unbalanced nature of the panel.[10]

3.3. Testing the impacts for notable price points

The previous section documents a fall in the inflation rate following the introduction of the rent controls. Given the parameterisation of the regulations, this effect is likely to be driven by a reduction in the share of growth rates above the allowable 4 per cent increase in rents. However, we can see from the summary statistics that (a) there were still growth rates above 4 per cent and (b) there were changes across

[10] Estimates including these data are available from the authors on request.

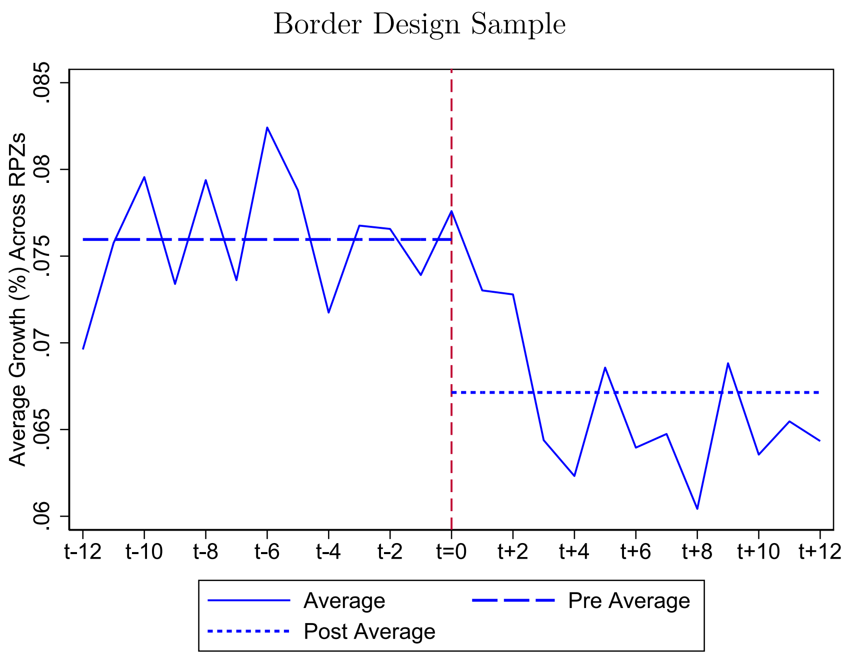

Fig. 4. Event Study Chart: Change in Means for RPZs.

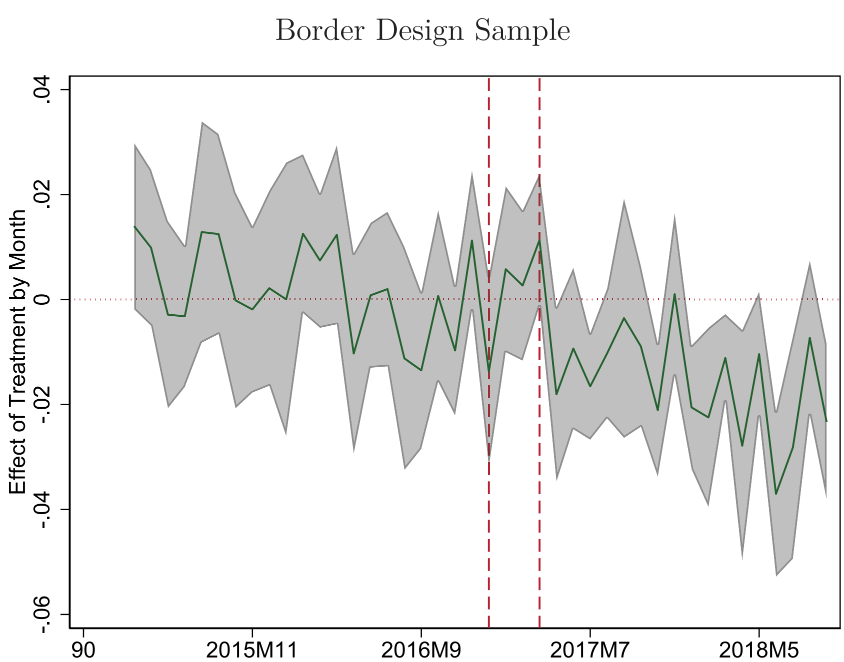

Fig. 5. Event Study Chart: Time Varying Parameter Impact.

the price distribution, in particular to the share of properties that do not change the rent, i.e., the degree of nominal rigidity. In this subsection, we test both of these particular aspects of the change to the price distribution.

3.3.1. Still above 4?

Our second *a priori* expectation is that the set-up of the regulations should reduce the share of growth rates over 4 per cent. This hypothesis provides a direct test of how effective the regulations have been in meeting their central objective of reducing growth rates above the 4 per cent cap. To test this proposition, we define an indicator variable which takes the value of 1 if $\Delta R_{ijt} > 4\%$ and 0 otherwise. This variable is regressed as a function of the RPZ dummy as previously defined, LEA fixed effects, time fixed effects and the control variables outlined in Eq. (2):

$$Pr\left(\Delta R_{ijt} > 4\%\right) = f\left(\beta RPZ_{jt} + \gamma_j + \tau_t + \mathbf{x}'_{ijt}\delta\right) \tag{5}$$

We saturate the specifications with month and LEA fixed effects as well as time gap controls. These controls ensure that the common trends over time as well as the LEA specific time invariant heterogeneity is removed thus allowing the RPZ coefficient to pick up the average effect of RPZ status on the areas after classification. We also include variables capturing the duration of the tenancy, the number of tenants, the number of rooms in the property, the type of housing (with apartment being the omitted group), a control for whether the lease was a new or renewal agreement, and a company type dummy. These variables

Table 3
Controlling for disequilibrium effects — border sample.

	(1)	(2)	(3)
RPZ_{jt}	-0.011^{***}	-0.010^{***}	-0.012^{***}
	(0.002)	(0.002)	(0.002)
R_{ijt-1}		-0.147^{***}	
		(0.005)	
$[R_{ijt-1} - \bar{R}_{jt-1}]$			-0.148^{***}
			(0.005)
Observations	56,081	36,002	36,002

Standard errors in parentheses
Standard errors clustered at time-LEA level.
Property type controls and fixed effects included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4
Growth rates above 4 per cent, linear probability model estimates.

	(1)	(2)	(3)	(4)
RPZ_{jt}	-0.159^{***}	-0.178^{***}	-0.136^{***}	-0.122^{***}
	(0.005)	(0.007)	(0.008)	(0.010)
Observations	361,597	149,166	136,201	56,081
From	Q1 2010	Q2 2015	Q1 2010	Q2 2015
To	Q3 2018	Q3 2018	Q3 2018	Q3 2018
Sample	All	All	Border	Border
Hedonic Controls	Yes	Yes	Yes	Yes
LEA FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Time Gap Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses
Standard errors clustered at time-LEA level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

should capture variation in the growth rates across different property types. Again, we would expect that $\beta < 0$.[11]

We estimate the model for the same four samples presented in Table 2, using differing time frames and RPZ coverage. The estimates are presented in Table 4 in columns (1) through (4).

Across all specifications, the share of properties with growth rates above 4 per cent fell dramatically. Focusing on the findings in columns (2) and (4) covering the short time widow, we find that for all properties the impact is a 17 per cent reduction in the probability of having a rental growth rate above 4 per cent. The figure for the border design sample, the more appropriate comparison from a regulatory compliance perspective, is 12 per cent. Across all estimates, the coefficients are statistically significant at the 0.1 per cent level. These findings indicate a strong impact of the regulations on the share of properties whose rent is growing at above 4 per cent.

3.3.2. Discussion: Why still above 4?

A critical question arising from our research is why there are so many properties still registering growth rates above the 4 per cent threshold. There are a number of potential reasons for this. First, the use of valid exemptions to the regulations. Two types of exemption are available (a) for new properties to the market and (b) for significant renovations. Second, pro-rata increases which are allowable (for example if a landlord did not raise the rent in year X they could increase the rent by 8 per cent in year X+1). Finally, non-compliance with the regulations. We explore each of these issues now in more detail.

3.3.2.1. Valid exemptions. Firstly, in relation to new properties, these would be automatically dropped from our sample as we require repeated observations of the same property to build our panel model growth rates. Therefore, the above 4 per cent growth rates in our sample will not be driven by these. In terms of significant renovations, this is a possibility. There are statutory guidelines which the regulator has released governing significant renovations. These related to permanent increases in the floor area of greater than 25 per cent, a major energy efficiency upgrade (a seven point increase in the Irish energy efficiency rating scale) or a combination of three of five more minor changes which includes a permanent increase in the number of rooms, better disability access and smaller energy efficiency upgrades. Unfortunately from our point of view, no official record of the use of such exemptions is provided by the regulator until after our sample period (late 2019) therefore we do not know how many of these properties used these exemptions in the period. However, reporting by the regulator did begin in 2019 and, in the fourth quarter of 2019, only 37 exemptions were sent to the RTB (annual report) which suggest a very low usage of these exemptions.

In our dataset, we do not have directly observed data to test all these exemptions; for example, we do not observe the energy efficiency rating for the full sample properties, nor do we know about disability access. However, we can check the use of the floor area condition as we have data on the floor area in square metres for a number of properties. With this variable, we can calculate how many of these properties reported a change in the floor area of over 25 per cent after the introduction of the regulations using the panel dimension of our data. Please note this is not a mandatory field in the regulatory form so the reporting is low. Of the 22,238 properties that report growth rates above 4 per cent after 2017 in RPZs, very few properties added a floor area increase of 25 per cent; 6 per cent of the total. This suggests that the pricing increase was not driven by this particular condition. Coupled with the low reporting of results when these became required in 2019, the very low number of reported breaches by tenants, it is unlikely that exemptions are the reason for the excess above 4 per cent.

3.3.2.2. Pro-rata increases? Secondly rents are allowed to be increased by 4 per cent per annum and can be done on such a basis that the average rate of growth should be 4 per cent or less over a period of years; if no increase is provided in year X, an increase of 8 per cent can be applied in X+1. One check on this that we can apply is to calculate the growth rate of properties in RPZs for which multiple observations are available after the regulation. We can then test, for this sample, whether the average growth rate is less than or equal to four per cent for the cumulative periods reported after the rules. This might explain some of the high growth rates. Please note that in our sample, not all properties have multiple observations after the rules were introduced. Of the 43,000 observations in RPZs after the introduction of the regulations just under 30,000 are for properties with two or more registrations after the classification. We use these data to calculate the average growth rate at a property level in the post RPZ period. The average growth rate was 6.8 per cent, the median was 4.7 per cent and 54 per cent were still above 4 per cent. Indeed, the histogram below demonstrates the distribution of the average growth rates showing a large number at or below four but still a considerable amount above (see Fig. 6).

3.3.2.3. Non-compliance? The final explanation is non-compliance. In Ireland, over the period in question, the rental market was highly strained with considerable excess demand. The practise of tenants agreeing to above 4 per cent growth rates just to secure the property is anecdotally widespread. In terms of the sanctions that would be faced for this breach, the only time there would be an investigation is if the tenant makes a complaint of which there are very few given the circumstances of chronic supply shortages. The annual report from the regulator notes that fewer than 2 per cent of tenancies registered a dispute in 2019; a total of 6,185 cases. This follows 4,800 in 2016, 5,823 in 2017 and 6,398 in 2018. However only 60 per cent of tenancy disputes are initiated by the tenant and the main reasons provided for disputes are for arrears on the landlord side and withholding the deposit on the tenant side. Fewer than 4 per cent of disputes or 263 cases in 2019 were for breaching an RPZ rent increase; these data

[11] As would be the marginal effect if a non-linear model is used in estimation.

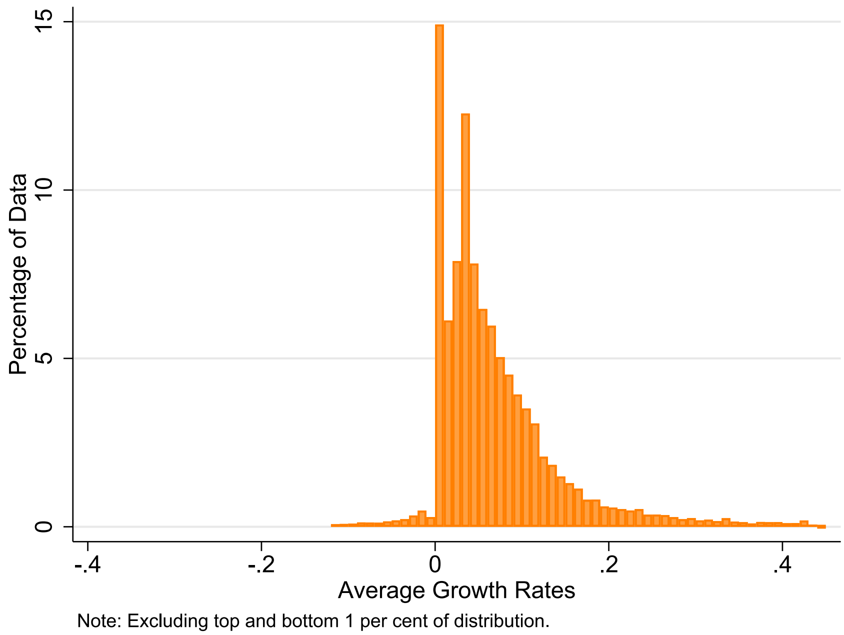

Fig. 6. Average Growth Rates for Post 2017 RPZ Sample.

suggest that these increases are acquiesced to from the tenant side. Indeed, it could also be the case that tenants bargain and agree to pay a higher rent to secure the property. These factors, coupled with little transparency through compliance monitoring or a transparent rent history register (so that tenants could check what the previous rent figures are to benchmark any payments on a newly acquired property) is likely to mean that non-compliance is a valid explanation in Ireland.

3.3.3. Any impact on nominal rigidity?

The impact of rent controls on the degree of price stickiness are potentially ambiguous from a theoretical point of view. Landlords' behaviour will depend on aspects such as the degree of excess demand or supply in the market, the parameters of the regulations or any exemptions that are allowable. The Irish regulations can be considered first generation rent controls, because they do not allow a reset of rent amounts between tenants (i.e., rents charged to new tenants must also comply with the 4 per cent growth cap). Given this feature, and the fact that the allowable increase in real terms is under 3 per cent,[12] it may incentivise landlords who previously did not change rents to build in price increases to ensure the real return remains intact over time. Therefore it may reduce the share of nominal rigidity as landlords build in small but legal increases. Alternatively, landlords may choose not to increase rents, as the costs of tenant turnover (a key determinant of nominal rigidity as shown by Aysoy et al., 2014) are not recoupable and therefore the risk of tenant exit is not compensated by the marginal increases.

To test these effects, we first explore changes in the share of zero growth rents before and after rent control implementation. Unchanged rents are assumed to be a proxy for increased nominal rigidity (i.e., if the share of unchanged contracts increases, nominal rigidity increased). We specify the following empirical model:

$$Pr\left(\Delta R_{ijt} = 0\%\right) = f\left(\beta RPZ_{jt} + \gamma_j + \tau_t + \mathbf{x}'_{ijt}\delta\right) \tag{6}$$

where the dependent variable is a binary indicator which takes the value of 1 if the rental growth was zero on an annualised basis and zero otherwise (Shimizu et al., 2010). This variable is regressed on the RPZ dummy, LEA and time fixed effects, and the vector of property type, tenants and lease duration controls described in the previous section. The coefficient on the RPZ dummy will provide insight into whether nominal rigidity increased following the introduction of rent controls in Ireland. The results of estimating equation (6) by linear probability OLS is presented in Table 5. The samples presented are as before. We find that the impact of the RPZ status on the probability of nominal rigidity is positive and significant. This means that the share of properties whose rent did not change increased following the introduction of the rental measures. One potential economic reason why this might be the case is that, given the cost of turning over tenants is not recoupable, this lowers the likelihood that a landlord will raise the rent and thus risk a potential tenant exit.

The magnitude of the coefficients suggest that being in a RPZ increases the probability of nominal rigidity by between 1.8 and 2.7 percentage points. Given there were approximately 17 per cent of zero growth rents in treated areas before the policy intervention, the economic impact of the policies suggests an increase of about 11–16 percent which is sizeable.

Table 5
Probability of nominal rigidity — marginal effects.

	(1)	(2)
RPZ_{jt}	0.027***	0.018***
	(0.004)	(0.007)
Observations	149,166	56,081
From	Q2 2015	Q2 2015
To	Q3 2018	Q3 2018
Sample	Full	Border
LEA FE	Yes	Yes
Month FE	Yes	Yes
Time Gap Controls	Yes	Yes
Hedonic Controls	Yes	Yes

Standard errors in parentheses
Standard errors clustered at time-LEA level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Hedonic controls include company landlord, renewal, tenancy length, number of tenants, number of bedrooms, and property type.

[12] At the time of writing the Irish CPI was increasing on an annual basis by 1.1 per cent, leaving the annual real allowable return at 2.9 per cent. See www.cso.ie for details.

Table 6
Splitting the rental distribution.

$Y(R)$	Rental Change Group
1	$\Delta R_{ijt} < 0$
2	$\Delta R_{ijt} = 0$
3	$0 < \Delta R_{ijt} < 4$
4	$\Delta R_{ijt} = 4$
5	$\Delta R_{ijt} > 4$

4. A full exploration across the price distribution

4.1. Testing across the distribution

It is clear from the preceding sections that the rate of overall rental inflation moderated in the Irish market, driven by a reduction in growth rates above 4 per cent, following the introduction of tenancy rent controls. However, to understand the impact of the policies on the distribution of rental prices it is noteworthy to explore what price growth did landlords set after the policies, as it is not clear from Fig. 3(a) that they all reduced to exactly the regulatory allowable maximum. Indeed, landlords price setting behaviour is likely to be affected by a number of factors such as profit maximising behaviour in maximise real returns (price growth above inflation), the probability of tenant turnover given the price increase and the ability to recoup the cost of turnover. Given the Irish regulations, and the fact that rents cannot be reset between tenancies except by the allowable 4 per cent per annum, the cost of tenant turnover becomes more difficult to recoup.

Within these parameters, and to explore price setting right across the distribution, landlords have the following pricing options after the regulations: (1) reduce rents; (2) leave rents unchanged; (3) increase rents by less than 4 per cent; (4) increase rents by 4 per cent; or increase rents by above 4 per cent. To explore, which of these pricing points were chosen by landlords, we conduct an analysis where we separate out the rental growth distribution into 5 segments as defined by variable $Y(R)$ in Table 6.

Splitting the distribution in this manner allows us to consider the rental changes in a multinomial sense. In other words, we can test the relative probability of a landlord choosing to price in other parts of the distribution, relative to the base case of above 4 per cent. The shares of growth rates split into these categories are presented in Fig. 7 for the period Q2 2015–Q3 2018, split out before and after the policies for the treatment and control groups. Focusing on the treated area, it is clear that there was a reduction in the share of growth rates above 4 per cent. This group is not eliminated completely as the regulations contain allowable exemptions for properties not previously rented or which underwent substantial renovation. There will also be some growth rate above 4 per cent if landlords did not increase the rent in the previous year as they are allowed accumulate on a pro-rata basis. The fall in the share above 4 per cent is concurrent with a rise in growth rates at 0, between 0 and 4 and at 4 per cent suggesting that landlords have priced differently across the distribution due to the regulations being introduced.

4.2. Methodology: A multinomial logit model across the distribution

To explore where across the price distribution landlords decided to set prices after the regulations, we use a multinomial logit model to explore the relative probability of pricing in each of the buckets as compared the above 4 per cent (our base category). Using a multinomial logit allows us to estimate relative risk ratios which provide the odds of any category relative to a base, and can be depicted in the following identity considering the relative probabilities as:

$$p_{ij} = \frac{exp(\mathbf{w}_i' \boldsymbol{\pi}_j)}{\sum_{l=1}^{m} exp(\mathbf{w}' \boldsymbol{\pi}_l)} \qquad j = 1, \ldots, m, \tag{7}$$

Table 7
Where have the high growth rates gone? MNL Relative Risk Ratios.

All Relative Risk Ratios from variable RPZ_{jt}		
	(1)	(2)
$\Delta R_{ijt} < 0$	2.147***	1.533***
	(0.121)	(0.156)
$\Delta R_{ijt} = 0$	1.805***	1.553***
	(0.069)	(0.088)
$0 < \Delta R_{ijt} < 4$	2.194***	1.931***
	(0.091)	(0.130)
$\Delta R_{ijt} = 4$	2.529***	2.283***
	(0.135)	(0.228)
$\Delta R_{ijt} > 4$	Base	Base
	(.)	(.)
Observations	149,166	56,081
From	Q2 2015	Q2 2015
To	Q3 2018	Q3 2018
Sample	Full	Border
LEA FE	Yes	Yes
Month FE	Yes	Yes
Time Gap Controls	Yes	Yes
Hedonic Controls	Yes	Yes

Exponentiated coefficients; Standard errors in parentheses
Standard errors clustered at time-LEA level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

where $\mathbf{w}_i$ are case specific regressors of which their are m cases. The model ensures that the sum of the probabilities across m cases is equal to 1, $\sum_{l=1}^{m} p_{ij} = 1$. The model is identified by setting a base case with 0 as parameters and each other category is compared to this case. In our model the explanatory variables will, as before, be:

$$\beta RPZ_{jt} + \gamma_j + \tau_t + controls \tag{8}$$

where the controls are the time gap and hedonic regressors previously outlined.

The results are presented in Table 7. Two specifications are presented covering the period Q2 2015–Q3 2018; the first includes the full sample and second is the border design sample. In each case, we present only the coefficient of the difference-in-difference interaction effect, RPZ_{jt}. All specifications include fixed effects for LEA, month and year as well as the time gap and hedonic controls discussed earlier.[13] In column (1), all relative risk ratios are statistically significant at the 1 per cent level and positive. This suggests that landlords were more likely to price in all other points of the distribution relative to above 4 per cent after the regulations. They were over two times more likely to price at 4 per cent and just under 2 per cent more likely to price between 0 and 4 per cent. While it is unsurprising that landlords are more likely to set prices at 4 per cent, it is somewhat surprising to see landlords are more likely to set 0 growth or lower rents relative to the above 4 per cent (by nearly 50 per cent in odds terms). The findings from this section are clear: faced with a common, market-wide regulatory maximum, landlords react heterogeneously and set prices at different points across the distribution.

The differential impacts here are worthy of further discussion. Looking at these findings, the distribution has shifted left to all points at or below the cap. The economic drivers of this shift are likely to depend on the balance between the wish to maintain real economic returns while on the other hand reducing the potential cost of having to change tenant. The latter point is particularly important for Ireland as the controls are linked to the property not the tenancy which makes raising the rents between tenants not a valid way to increase inflation or to recoup tenant turnover costs. These effects may be driven by landlord size, their financial wealth, and other operational resources that may mean a different pricing strategy is optimal. It is clear, however, that the imposition of a common regulatory cap, which would be expected

[13] Results for these variables are available on request.

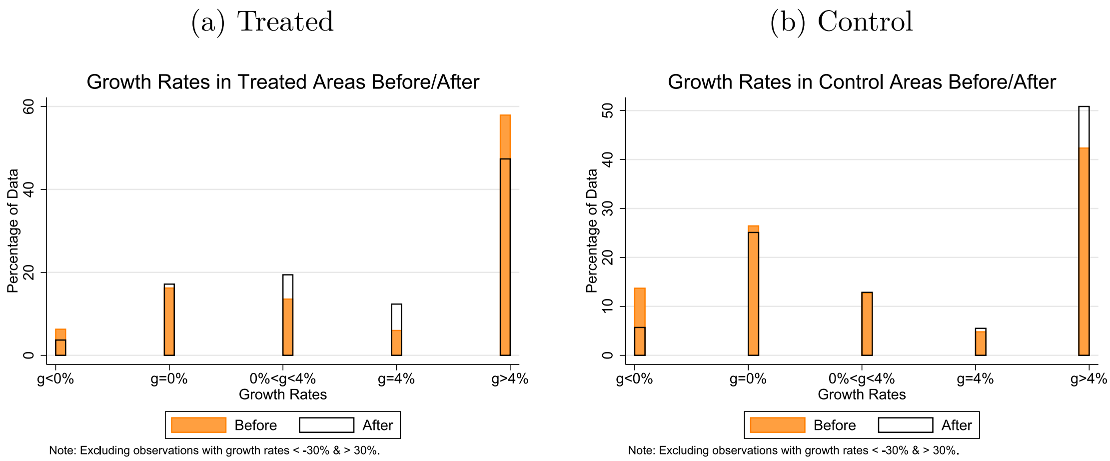

Fig. 7. Growth Rate Distributions — Treatment and Control LEAs.

to just bunch prices at the calibrated level can have quite differing impacts across the price distribution. Indeed, as the relative risk ratios are greater for the inflation groups between 0 and (or at) 4 relative to the nominal rigidity or deflation groups, these effects may event have caused some upward pressure on inflation as landlords react to the regulations by increasing the rents by the allowable rate.[14] It is likely these effects are caused by a balance between trying to maximise returns and minimise disruption and cost. It may also be caused by behavioural factors such as the landlords' belief that caps may be tightened in the future.

4.3. *Exploring heterogeneous effects by landlord type*

Findings in previous sections point towards heterogeneous effects across different landlords, depending on their circumstances and the supply, demand dynamics in their local market and with their tenant. To provide a more detailed insight into the behaviour of landlords, we exploit the rich micro dataset to gain a better understanding of which landlords may react differently. We do this by looking at the type of landlord. Our data provides information on whether the landlord is a company or an individual.[15] We use these data to test whether the differential pricing across the distribution using the multinomial logit approach, differs for landlords relative to companies. In our data, for the full sample, approximately 10 per cent of the observations are accounted for by company landlords' while for the border sample, the figure is 8.7 per cent (in Table B.10 we present the data for the treatment and control groups separately for the border sample).

There are a number of reasons to think that companies might react differently. First, if the landlord is a private individual, they may be managing only a single (or at least limited) number of housing units. Therefore, it is more costly for them to carry the cost of changing tenants. They may also be have a lower level of financial knowledge and not be as well placed to make optimal financial decisions as compared to a professional company who are likely to have trained finance staff. Given this intuition, our *a priori* hypothesis is that companies are less likely to keep prices nominally rigid after the regulations, and more likely to price at the regulatory maximum as compared to individuals. Individuals may also have built up a personal relationship with the tenant and this may affect their pricing decisions.

Indeed, the histogram (Fig. 8) for the five growth categories across companies and individuals after rent controls were implemented provides suggestive evidence of differences. Individual landlords appear to have a higher share of nominal rigidity, while companies appear to have a higher share of growth rates at or above 4 per cent. We therefore explore whether these patterns hold in a difference-in-difference setting.

We now present the multinomial logit estimates obtained after including the interaction between the difference-in-difference and the company indicator. Again, relative risk ratios are presented in Table 8. The interpretation of the new interaction term included is that it increases or lowers the overall effect captured by the main *RPZ* dummy (if less than one, the effect is lower, if greater than one, the effect is higher as the impacts are multiplicative). Focusing on the border sample, the estimates indicate that there is no difference for companies and individuals between the nominal rigidity group and falling rents and rental growth between 0 and 4 per cent categories. However, companies are significantly more likely to set growth rates at the regulatory maximum of four per cent. This might be arising from professional investors following cash flow maximisation strategies, and therefore setting maximum rent increases allowed by the regulations where feasible.

To summarise, our estimates indicate that differences exist in the reaction to the regulations depending on whether the landlord is a company or an individual. We find that individual landlords are more likely to set prices unchanged after the regulations. This behaviour might arise from these landlords being less likely to be able to carry tenant turnover costs, having an valued tenancy relationship with the occupier or being less financially sophisticated than a professional company. We also find that companies are more likely to set prices at the regulatory maximum (relative to no-change) which would be in line with professional cash flow management practises.

5. Quantity response and areas of future research

5.1. *Was there a quantity response?*

While the focus of this paper has been to explore the impacts of the price cap on the price distribution, there are a range of other impacts which may be caused by rent controls. While many of these are outside the scope of this paper (and some discussion of future research is presented in Section 5.2), one aspect which we can explore is whether there was any impact of the regulations on the number of tenancies registered, i.e., the market turnover. There are a number of reasons why the market turnover might change. First, if the regulations are known to the market, then landlords might bring forward agreements so as to increase prices before the cap becomes binding. Alternatively,

[14] A test of setting the base category at the zero change group and re-estimating the models shows these differences to be statistically significant.

[15] When registering with the RTB, the landlord is required to provide either a company registration number if they are a commercial operation or a personal household social security number. We can use these data to split the landlords between commercial companies and individual households.

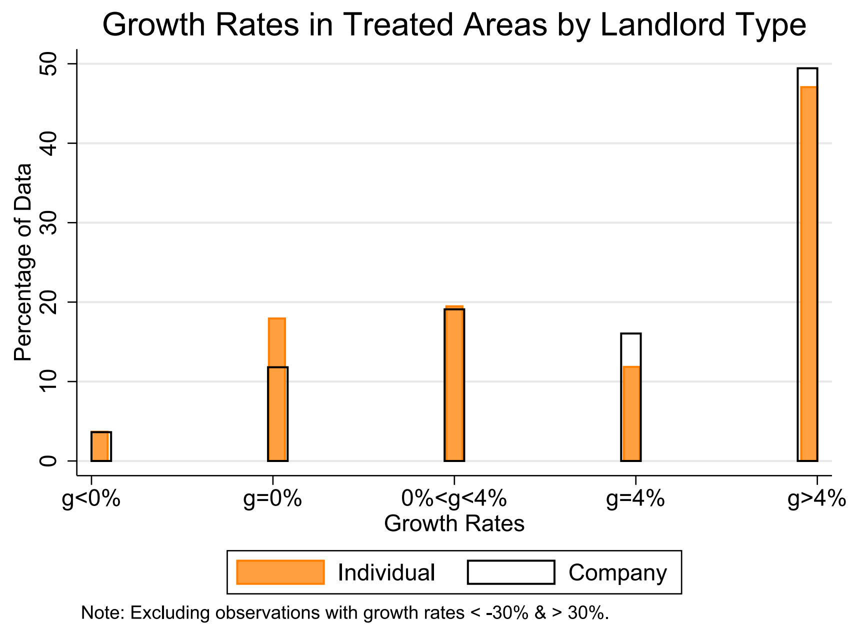

Fig. 8. Growth Rate Distributions — Treatment Areas by Landlord Type.

after qualification, market turnover might fall for two reasons. First, if tenancy turnover costs are high then tenants might stay longer. In our sample, this could be observed with a fall in the number of new registrations. Second, if landlords and tenants wish to bi-pass the regulations, they may act in the shadow or unregulated market. We cannot directly disentangle these two dynamics but they would be a consistent explanation for a fall in tenancy registrations.

To explore this issue, we first undertake an event window exercise (as we did for the price growth above) which benchmarks the average number of tenancies across all RPZ LEAs at 0 when they qualified and presents numbers for the preceding and following 12 months. This is presented in Fig. 9. The event window will allow us to observe whether tenancies increased before the qualification. There does not appear to be any major evidence of tenancies being brought forward. The large spike which can be seen both before and after qualification (here at $t-5$ to $t-3$ and $t+8$ to $t+9$) is related to the seasonal effects of student tenancies being agreed before university in August/September/October period of any given year (3–5 months before January 2017 when most classifications happened, and similarly 8–9 months after). Other than these two natural peaks, there is little change that one could visually attribute to the regulations.

However, despite the lack of any major visual trend, it is worth exploring this in a regression format. We estimate a simple panel model at the LEA level (j) in each period (t):

$$T_{jt} = \beta RPZ_{jt} + \gamma_j + \tau_t + \epsilon \tag{9}$$

We need to aggregate to the LEA level to obtain a count of registered tenancies. The results are presented in Table 9.

We find that there was a reduction in the number of tenancies agreed in RPZs after the introduction of the regulations. This is estimated in an LEA-level model with the number of tenancies as the dependent variable and fixed effects for time, LEA and a time varying RPZ qualification dummy. The fall is approximately 3 tenancies per quarter per LEA on average. The average number of tenancies per LEA in the border sample before the regulations were introduced was approximately 46 per month which suggests a drop of 6.5 per cent in registrations. While this drop may be driven by either longer staying tenants or increased used of the shadow market, either way, it is noteworthy and future research could attempt to disentangle this where data may be more suitable.

5.2. *Future research*

There are a number of areas of research that are not covered in this paper but would be very interesting to explore further. While direct price effects of the rental cap are identified in this research, one of the impacts of these regulations may be to increase the number of non-core price payments that households have to make in response to the regulations. If landlords are not able to charge higher rents, they may ask households to make side payments or else to cover more of the utilities and other charges associated with the property than they previously did. Future research should attempt to consider the questions as, if such payments occur, the benefits of lower growth in terms of rental market affordability may not fully materialise.

A second main mechanisms through which such regulations can have economic externalities is through reducing supply. As we do not have appropriate data at a local electoral level on housing supply activity, it is not possible for us to test these dynamics. However, in future, if such data become available, researchers should attempt to assess in more detail whether these effects are present; and in particular whether supply shifted between markets such as owner-occupation markets and social housing.

Another issue that may arise with rent caps is the issue of tenant selection or discrimination. Our indicators of ex-ante tenancy characteristics do not have rich information on the household taking up the tenancy. For example, landlords may change their views on families versus single households etc. While an initial difference-in-difference analysis suggests no change in the number of tenants per agreement in our data, this may be due to our limited indicators and future research should explore this topic in more detail

6. Conclusions and discussion

This paper explores the impact of rent control regulations on rental price inflation, as well as on nominal rigidity in housing rents. Our research objective is therefore two-fold. First, using a difference-in-difference approach, we try to isolate the effect of the introduction of rent controls on rent inflation in Ireland. Given the exemptions allowed in the regulation, as well as potential non-compliance, we also analyse whether the regulations affected the share of rents growing above the 4 per cent cap imposed in the Irish legislation. Our second research objective focuses on analysing the effects of rent controls on the degree

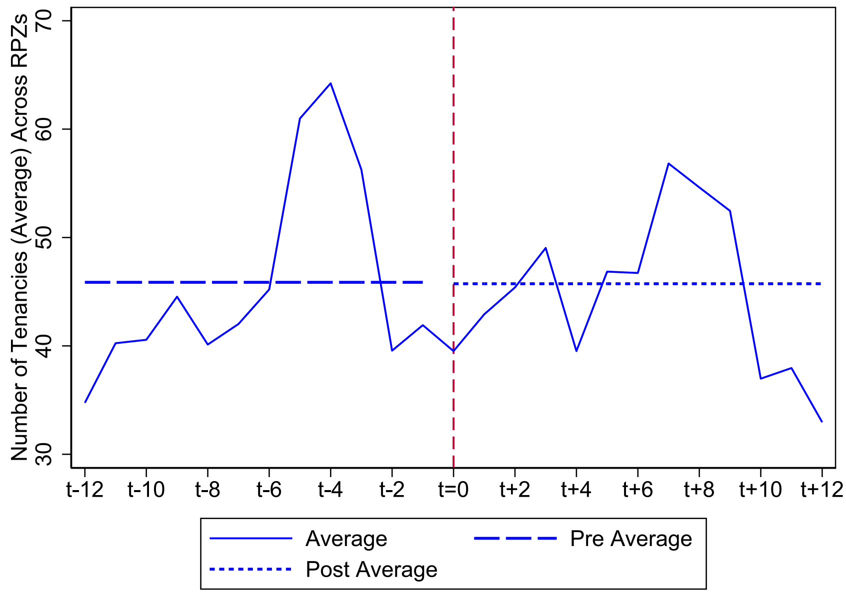

Fig. 9. Event Window Analysis of Number of Tenancies.

of nominal rigidity. In order to test these effects empirically, we focus first on changes in the share of zero growth rents. Afterwards, using a multinomial logit model, we explore the impacts across the price distribution.

A number of findings emerge. We find that overall rent controls had a deflationary impact, with the rental inflation rate dropping by approximately 1–2 percentage points after the introduction of the regulations. We also find a considerable fall in the share of rent price increases above the 4 per cent limit imposed by the regulatory framework. These findings indicate that the regulations have been effective to a certain extent in lowering the level of rental inflation in the areas of the country where rent controls were implemented. The effect that we demonstrate in this paper shows a clear, and economically meaningful, decline in the high growth rents but a rise in growth at the cap in the short run after the introduction of the regulations. However, a question arises as to whether in the long run, some of the drift in price setting up to the cap will actually lead to less rent being paid in a tenancy over a period of years. It could be the case that large but infrequent increases in rents are replaced by small but frequent changes which may leave the levels relatively unchanged over time. We do not observe properties for a sufficient amount of years after the regulations to test these effects, but this is certainly an area of research that should be considered should data become available to do so.

Furthermore, it must be noted that our property-level sample relates to new tenancies which has implications for the findings. If old tenancies are likely to have a residency discount, and the inflation rate is lower for these properties, the findings in our sample are likely to be high relative to the impact on all tenants in Ireland. Our sample simple relates to new market rents.

In relation to the impact of rent controls on nominal rigidity, we find evidence pointing to both increases and decreases of rigidity. First, the share of zero growth contracts increased after the introduction of the measures. This might be consistent with the regulatory structure implemented in Ireland, which does not allow rent resets between tenants. Therefore, the increase in zero growth rent agreements may arise due to the risk of tenants leaving under a rent increase and such costs not being recoupable, resulting in increases in nominal rigidity. Second, the findings of the multinomial estimates present an more complete picture of the impacts of rent controls across the price distribution. The estimates indicate that rents were more likely to stay unadjusted after the implementation of rent controls, as opposed to increase above the 4 per cent threshold. However, the estimates also suggest that the regulations have increased the share of contracts with inflation at or below the regulatory limit. This indicates that many landlords may have increased the rents in an effort to profit maximise (maximise the real value of tenancies) given their inability to reset rents outside exceptional circumstances under the framework. Therefore, while the overall effect of the policies has been to dampen rents, they have also caused inflation at other points in the distribution consistent with a protection of real return by landlords.

We find differences by landlord type with non-professional investors (individual households) more likely to have nominally rigid rents and also less likely to have increases at, or below, the maximum allowable. This may be driven by the fact that such households are unable to absorb tenant turnover costs as easy as professional companies.

This research has interesting policy implications. First, our evidence suggests that while rent controls can be effective in dampening house price inflation, the effect is not uniform across the distribution. Indeed, some tenants, who previously enjoyed nominal rent freezes, may face rent rises up to the regulatory maximum allowance as landlords look to protect real returns. Two elements of the scheme calibration are therefore important to reflect upon. The Irish rent controls, which do not allow a rent change between tenancies, may be the reason landlords see the requirement to protect real returns. Rent restrictions which do not share this feature may not experience this inflationary impact. Second, the increase in the share of rent increases at the regulatory maximum (in this case 4 per cent) highlights the fact that the parameterisation of this element is considerably important. Policy makers should carefully consider the numerical parameter that is set as this will be essential in guiding behaviour. Regular reviews of this parameter based on granular data would be important, in particular, where rent control price allowances interact with inflation rates and Central Bank inflation targets.

CRediT authorship contribution statement

Conor O'Toole: Conceptualization, Methodology, Formal analysis, Writing - original draft, Writing - review & editing. **Maria Martinez-Cillero:** Methodology, Data curation. **Achim Ahrens:** Methodology, Data curation.

Table 8
Multinomial Logit Model Estimates — Relative Risk Ratios.

	(1)	(2)
$\Delta R_{ijt} < 0$		
RPZ_{jt}	2.173***	1.501***
	(0.125)	(0.154)
Company	1.005	1.050
	(0.056)	(0.120)
$RPZ_{jt}\times$ Company	1.228*	1.241
	(0.151)	(0.269)
$\Delta R_{ijt} = 0$		
RPZ_{jt}	1.872***	1.543***
	(0.073)	(0.089)
Company	0.962	0.847*
	(0.046)	(0.079)
$RPZ_{jt}\times$ Company	0.927	1.028
	(0.071)	(0.145)
$0 < \Delta R_{ijt} < 4$		
RPZ_{jt}	2.296***	1.876***
	(0.096)	(0.128)
Company	1.167***	0.960
	(0.061)	(0.079)
$RPZ_{jt}\times$ Company	1.076	1.390***
	(0.078)	(0.164)
$\Delta R_{ijt} = 4$		
RPZ_{jt}	2.609***	2.167***
	(0.145)	(0.214)
Company	1.145***	1.053
	(0.058)	(0.099)
$RPZ_{jt}\times$ Company	1.259**	1.585***
	(0.121)	(0.281)
$\Delta R_{ijt} < 0$		
RPZ_{jt}	Base	Base
	(.)	(.)
Company	Base	Base
	(.)	(.)
$RPZ_{jt}\times$ Company	Base	Base
	(.)	(.)
Observations	149,166	56,081
LEA FE	Yes	Yes
Month FE	Yes	Yes
Time Gap Controls	Yes	Yes
Hedonic Controls	Yes	Yes

Exponentiated coefficients; Standard errors in parentheses
Standard errors clustered at time-LEA level.
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 9
Impact on number of tenancies.

	(1)	(2)
RPZ_{jt}	−3.893***	−3.069***
	(0.883)	(0.679)
Observations	5684	2097
From	Q2 2015	Q2 2015
To	Q3 2018	Q3 2018
Sample	Full	Border

Standard errors in parentheses
Standard errors clustered at LEA level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A. RPZ designation dates by LEA in Ireland

	Designation round	Date
County Dublin		
Dublin City Council	1	December/2016
Cabra–Finglas		
Ballymun		
North Inner City		
Beaumont–Donaghmede		
Clontarf		
Ballyfermot–Drimnagh		
Crumlin–Kimmage		
Rathgar–Rathmines		
Pembroke-South Dock		
South Dublin County Council	1	December/2016
Lucan		
Clondalkin		
Templeogue–Terenure		
Tallaght Central		
Tallaght South		
Rathfarnham		
Dun Laoghaire–Rathdown County Council	1	December/2016
Glencullen–Sandyford		
Dundrum		
Stillorgan		
Blackrock		
Dun Laoghaire		
Killiney–Shankill		
Fingal County Council	1	December/2016
Balbriggan		
Swords		
Castleknock		
Mulhuddart		
Howth–Malahide		
County Galway		
Galway city	2	January/2017
Galway City Central		
Galway City East		
Galway City West		
County Cork		
Cork City Council	1	December/2016
Cork City North-Central		
Cork City North-East		
Cork City North-West		
Cork City South-Central		
Cork City South-East		
Cork City South-West		
Ballincollig–Carrigaline	2	January/2017
Cobh	3	March/2017
County Kildare		
Maynooth	3	March/2017
Celbridge–Leixlip	2	January/2017
Naas	2	January/2017
Kildare–Newbridge	2	January/2017
County Meath		
Ashbourne	2	January/2017
Laytown–Bettystown	2	January/2017
Ratoath	2	January/2017
County Wicklow		
Bray	2	January/2017
Wicklow	2	January/2017
Greystones	4	September/2017
County Louth		
Drogheda	4	September/2017

Appendix B. Additional summary statistics

Table B.10 provides an overview of the landlord types by company and individual.

Table B.10
Data split: Landlord type – before and after treatment – border sample.

Landlord Type	Before	After	Total
Individual	31,359	19,839	51,198
Company	2,830	2,057	4,887
Total	34,189	21,896	56,085

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