Tenants, Landlords and the Housing Benefit: Who Gets It and Where the Money Goes

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Abstract

Housing allowances are the main instrument with which many governments address housing affordability for low-income renters. This paper analyze whether landlords capture a housing benefit and asks how it shapes household choices. We exploit the introduction of a housing allowance in Spain, paid under two designs: an income grant and a proportional rent subsidy. By estimating a staggered difference-in-differences research design, we find that landlords do not raise the rents of recipients, not even those with higher capacity to reprice their tenants. With prices fixed, most of the cash transfer appears as disposable income and is spent rather than saved. Little of it goes to housing: recipients of the income grant do not upgrade their dwellings, although recipients of the proportional subsidy move more. The main offset is on the labor margin, where the youngest recipients reduce their earnings. Our interpretation is that, when rent adjustment is constrained, a small-scale housing benefit can be an effective income support for low-income households, while their housing choices depends on the design.

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1 Introduction

Housing is the largest expenditure of low-income households across Europe, and its weight has grown in recent years. In Spain, roughly half of the renters in the bottom income quintile spend over 40% of their income on rent (OECD)[1], and rents rose by about 30% between 2016 and 2023, placing growing pressure on rental affordability. To address this challenge, governments in several countries have introduced demand-side housing benefits: income-targeted cash transfers paid directly to renters. Such programs are widespread in Europe and the US, and recently in Spain. Whether they work depends on whether the transfer stays with the tenant or is absorbed by the landlord through higher rents, and on this the evidence is mixed. Estimates range from near-complete pass-through to negligible price effects, and the literature has come to read this dispersion as the result of two market determinants: the scale of the program, and the elasticities of housing supply and demand in each market[2].

These determinants all work through the market rent, and this paper steps back from market level incidence to look at the mechanisms underneath. A tenant's rent is not only a market price: moving is costly, prices are sticky, and information is incomplete. Due to these frictions, the benefit can change what a recipient pays even when the market rent does not move, through the bargaining between landlord and tenant, a channel that market-wide reforms cannot isolate from the equilibrium response. The other side of the tenancy has received little attention: housing choices have been studied less and just to explain the movement of rents, not as an outcome in themselves. In addition, there is no evidence on what recipients do with the part of the transfer that does not go to housing. Both margins matter because a housing benefit may pursue two goals: lowering the rent burden of low-income households or improving the housing in which they live. The theory of housing subsidies says that which one it achieves depends on its design. An income grant is spent as any other income, whereas a subsidy proportional to the rent makes housing cheaper relative to other goods and should buy more of it (Brueckner, 2011).

This paper asks how a housing benefit changes the bilateral bargaining negotiation and the housing choices of the tenant. We study the direct rental assistance that Spain introduced in 2022, a transfer paid to low-income tenants that reached a small share of eligible renters, too few to move any rental market, and paid under two designs, an income grant and a proportional rent subsidy. We use fiscal records that link each tenant to the landlord who

[1] OECD (2025), OECD Affordable Housing Database - indicator HC 1.2. Housing costs over income, https://oe.cd/ahd.

[2] See (Fack, 2006; Gibbons and Manning, 2006; Kangasharju, 2010; Eriksen and Ross, 2015; Collinson and Ganong, 2018; Brewer et al., 2019; Eerola and Lyytikäinen, 2021; Eerola et al., 2026)

declares the rent of the same dwelling. Exploiting the rationing of a fixed budget on a first-come, first-served basis, we compare recipients with eligible tenants who did not receive the benefit. We find that landlords do not raise the rents of recipients at any of the margins at which the law allows a price change, not even those landlords for whom the cost of repricing is lowest. The transfer therefore reaches the tenant as disposable income. Recipients do not consume more housing: the design that lowers the relative price of housing induces more moves, and the grant is associated with household formalization, especially among the young, but neither group upgrades its dwelling. Labour income falls among the youngest recipients and liquid savings do not increase. A housing benefit paid to the tenant in a small program thus reaches its intended beneficiaries and is used as an income transfer, and the design of the transfer decides whether it changes housing at all.

Spain's 2022 rental assistance consists of two programs[3], both paid into the tenant's bank account and both subject to an income threshold and to a cap on the monthly rent. The *Bono Alquiler Joven* (young track) pays tenants aged 35 or under a fixed €250 per month for 2 years. The general program of the State Housing Plan pays tenants of any age the share of their rent, up to 50%, subject to the same income and rent caps as the young track.[4] Although the two programs paid similar amounts in practice, they are two unconditional transfers with different incentives: the income grant (lump-sum) leaves the relative price of housing with respect to other goods unchanged, while the proportional rent subsidy lowers it. We exploit this difference when analyzing how recipients adjust their housing consumption. Three features of the programs matter for whether landlords can capture the benefit. The benefit is paid to the tenant, so the landlord is not informed by the program. It was introduced when annual rent updates within running contracts were capped by law, that represent repricing frictions. And its budget was fixed and allocated in order of arrival of applications: in most regions the funded slots were exhausted within the first week of the call, and eligible applicants who had not been processed by then received nothing. About 168,000 tenants received the benefit in 2022, 2.9% of eligible renters and under 2% of all renters, too few to generate a meaningful shift in the aggregate rental demand.

Our data come from the *Panel de Hogares*, an administrative panel built by the Spanish Tax Agency that follows a representative sample of households from 2016 to 2023. Its defining feature for our purposes is that it records the dwelling of every tenant and every owner, which lets us link each tenant to the landlord of the same dwelling and observe both sides of the lease: the rent the landlord declares for that dwelling, which is the price the tenant

[3]See Royal Decree 42/2022

[4]The general program could be granted for up to five years, but its duration was set by each region: none granted the full five years, and most granted two as in the young track.

pays, the landlord's full property portfolio, and the tenant's income by source, benefit receipt, household composition, moves and bank balances, so that we can follow the transfer through the tenant's budget.

To identify the effect of the benefit, we estimate a staggered difference-in-differences design with individual fixed effects that compares recipients, in the years in which they receive the benefit, with tenants who met the income and rent conditions but did not receive it. Conditional on eligibility, receipt was decided by the order of arrival of applications and not by any assessment of the applicant, so there is no selection into treatment by the administration. The remaining concern is selection into applying early, which we do not observe. We address it with a matched comparison group that restricts the control group to eligible non-recipients with similar pre-treatment observables as recipients, and we complement the baseline with event studies and with estimators robust to staggered and non-absorbing treatment.

We first check whether landlords change the price paid by recipients at any of the margins that bargaining leaves open. On average, they do not. The rent of recipients follows the same path as that of eligible non-recipients, before and after receipt, and so does rent per square meter. A null average could nonetheless hide a response where the conditions for one are met, so we look at those configurations. The first is repricing capacity. The annual update within a contract is capped and has to be initiated by the landlord every year, a fixed cost per dwelling that falls with portfolio size. We document that large landlords have 18 percentage point higher probabilities to exercise the update compared to small landlords, and that the rent of a sitting tenant rises faster over the contract term with a large landlord than with a small one. By contrast, this capacity is not used against recipients: the effect of the benefit on rents does not differ by landlord type, and large landlords raise rents for all their tenants alike. The second configuration is the landlord's own portfolio, where a recipient can be compared with a non-recipient of the same landlord in the same year; recipients pay no more. The third is segment pricing: a landlord who cannot identify recipients could reprice the eligible segment after 2022, which no landlord type does. The fourth is information. The benefit appears in the tenant's tax return the year after receipt, and tax returns are commonly requested when a new contract is signed; rents in contracts signed in 2023 are no higher for tenants whose return shows the benefit, nor in a placebo of 2022 contracts. The fifth is local market power, which we measure with the landlord's share of the local rented stock and the concentration of the local market; neither predicts a rent response. The only margin at which rents move is the renewal of a contract in the same dwelling, where the law leaves the price free and recipients who renew pay about 5% more; because staying through a renewal is a choice of the tenant that the benefit itself can affect, and because such renewals are rare, we present this as suggestive evidence of renegotiation rather than as a test.

We then ask what recipients do with the transfer. With the price of housing fixed, the benefit shifts the recipient's budget constraint, and the two designs shift it differently. The income grant moves the budget line outward in parallel and works through an income effect alone; the proportional rent subsidy lowers the relative price of housing and adds a substitution effect, so for the same public cost it should buy more housing. In our setting this difference can only appear in mobility, because the subsidy is computed on the rent of the contract in force when the benefit is awarded, so that a recipient who stays receives similar amount under both designs. We do not observe the rent that a mover pays in the new dwelling, which is the main limitation of our data; but we observe whether tenants move and where they move into. Recipients of the proportional rent subsidy become more likely to move, recipients of the income grant do not, and the dwellings that recipients move into are no larger and no more valuable than those of non-recipients who move. However, housing consumption can also change without a move, through the number of people who share the dwelling. Recipients of the grant do not reduce crowding significantly, but they become more likely to file a joint tax return and to declare a spouse[5], which points to household formation or formalization rather than to more housing. Housing responses are thus small and follow the design of the transfer: only the subsidy that lowers the price of housing moves tenants, and the grant is used as income. Most of the transfer must therefore go elsewhere, and we account for it. Of each euro received, about 82 cents appear as disposable income after rent. The rest is mainly a labour-income response, concentrated among recipients aged 25 or under, who reduce their earnings by roughly the amount of the benefit without leaving employment. This income effect on the intensive margin is consistent with the youngest recipients using the benefit to buy time rather than goods, for instance to study. On the other hand, liquid savings do not increase, so the allowance is spent as income.

Taken together, the results show the conditions under which a housing benefit reaches the tenant: a program small enough not to move markets, a transfer paid to the tenant rather than to the landlord, and legal frictions that limit price adjustments. Under these conditions, landlords with the capacity to reprice do not use it against recipients. The results also show what the benefit does once it reaches the tenant, and this depends on its design: only the proportional rent subsidy changes housing choices, and modestly; the income grant is spent as any other income, on other consumption and, for the youngest, on time. For lowering the rent burden the grant is the more efficient instrument, but it is then an income transfer like any other, and the housing label serves only to target it, since tenure status proxies for holding no housing wealth and for low income.

This paper contributes to four strands of literature. The first studies the incidence of

[5]Older tenants also experience the increase in probability of declaring a spouse

housing allowances. Early natural experiments found pass-through: Fack (2006) estimates that most of the French allowance went to rents, Gibbons and Manning (2006) that about two thirds of the British cuts were passed on to rents, and Susin (2002) that vouchers raised the rents of unsubsidised low-income units in the US metros where they were concentrated; Kangasharju (2010) finds comparable pass-through in Finland. More recent work finds much less: Eerola and Lyytikäinen (2021) find rent effects close to zero using discontinuities in the Finnish allowance formula across municipality groups, Eerola et al. (2026) find that a large reform left rents and housing choices unchanged under supply-constrained context when housing choices is unresponsive, and Brewer et al. (2019) show that the small average pass-through of the 2011 British cuts hides large local heterogeneity. The literature has converged on the view that incidence depends on the elasticities of supply and demand and on how much of the local market the program reaches, and has moved toward mapping the conditions under which capture occurs (Eriksen and Ross, 2015; Collinson and Ganong, 2018). Our question is different and complementary. We do not estimate the incidence of a market-wide reform; we ask whether an individual landlord raises the rent of an individual subsidized tenant when the market around them is unchanged. Answering it requires a program whose small scale shuts down the market channel and data that identify the landlord of each recipient. With them we test the conditions of bilateral extraction one by one, show that the null is not the net of offsetting forces, and locate the one margin at which rents respond.

The second strand compares cash transfers and housing vouchers (Olsen, 2003; Currie and Gahvari, 2008; Collinson and Ganong, 2018). The modern evidence on conditional programs comes from the Housing Choice Voucher program and Moving to Opportunity: assistance tied to a move raises housing and neighbourhood quality and, when it reaches low-poverty neighbourhoods, children's long-run outcomes (Katz et al., 2001; Kling et al., 2007; Chetty et al., 2016; Bergman et al., 2024), and it reduces labour supply (Jacob and Ludwig, 2012). These programs identify the effect of a constrained choice. We add a comparison between the two unconditional designs of the theory of housing subsidies, an income grant and a proportional rent subsidy (Brueckner, 2011), within the same program. The grant leaves housing unchanged; the subsidy induces moves. The two designs thus map onto the two goals these programs pursue: the grant lowers the rent burden, the subsidy is the one that can improve the dwelling. We also follow the part of the transfer that does not go to housing, on which there is no evidence for housing benefits.

The third strand studies how rents are set when the market does not clear through prices alone. In a market with search frictions and long contracts, the rent of a sitting tenant is negotiated between landlord and tenant rather than re-set by the market (Wheaton,

1990; Piazzesi et al., 2020), and it is nominally rigid, especially for sitting tenants, with a rigidity that differs by landlord type (Genesove, 2003; Gallin and Verbrugge, 2019). A growing literature asks whether landlords exercise market power: acquisitions of competing properties raise rents (Ramoutar, 2025), landlord market power generates markups (Watson and Ziv, 2025), algorithmic pricing facilitates rent price coordination (Calder-Wang and Kim, 2025), and institutional investors change concentration, supply and prices in the single-family segment (Barbieri and Dobbels, 2025; Gorback et al., 2024). Similarly, Diamond et al. (2019) document heterogeneous landlord responses to rent regulation, with larger landlords more likely to exit the rental market. We bring this literature to the bilateral renegotiation between landlord and tenant, and depart from it in two ways. We look within individual landlords, who own most of the rental stock in Spain, rather than between individual and corporate ones. And we separate portfolio management from local market power, two mechanisms usually bundled together and only weakly correlated in our data. Repricing capacity rises with portfolio size, but neither capacity nor market power is used to extract a public transfer from tenants.

The fourth strand concerns fungibility and the labeling of transfers. Labels often matter: households have been found to spend a transfer earmarked for a specific good on that good far more than they would spend an equal amount of ordinary income (Kooreman, 2000; Beatty et al., 2014; Abeler and Marklein, 2017; Hastings and Shapiro, 2018), and Bozio et al. (2015) argue that such effects could explain why French allowances were capitalized into rents even for households whose benefit did not depend on their rent. We find no labeling effect for a temporary allowance paid to the tenant: the housing response does not exceed the income-effect benchmark, and the rest of the transfer finances other consumption. For a housing benefit paid as a grant, the label does not change what the money buys, as adjusting housing consumption is costly; its function is to target the transfer on tenants.

The remainder of the paper is organized as follows. Section 2 describes the Spanish rental market, the regulation of rent adjustment, the two assistance programs, and their rationing. Section 3 sets out the conceptual framework and the predictions we test. Section 4 presents the data and Section 5 the empirical strategy. Section 6 reports the results on prices and landlords, and Section 7 those on the tenant's budget. Section 8 concludes.

2 Institutional Background

2.1 The Spanish rental market and the regulation of rents

Spain is a country of homeowners: about 75% of households own their main residence. The rental sector has nonetheless doubled in size over the last two decades, from 10% to 20% of households between 2002 and 2022, with the increase concentrated among young adults in large metropolitan areas (*Encuesta Financiera de las Familias*, Banco de España). A feature of the Spanish market matters for our setting, because our data cover the rents declared by natural persons: 92% of the primary-residence rental stock is owned by individuals, and only 8% by companies and social housing providers (Banco de España, 2023). During the period of our study, rents rose by about 30% between 2016 and 2023.

Rental contracts are governed by the *Ley de Arrendamientos Urbanos* (LAU), approved in 2019. Three features of this law matter for our analysis, because they define the margins at which a landlord can change the rent of a tenant. First, contracts signed by individual landlords last at least five years (seven for legal entities); under the previous regime, in force between 2013 and 2019, the minimum term was three years[6]. Second, within the term the rent can only be updated once a year, by at most the variation of the consumer price index[7], and only if the landlord notifies the tenant at least thirty days before the anniversary of the contract. If no notification is sent, the rent stays unchanged for another year. Third, when the term expires the parties may sign a new contract at a freely negotiated rent. If neither party gives notice at least one month before expiry, the contract is extended tacitly, year by year, for up to three additional years under the 2019 rules, at the same rent.

The in-contract update is therefore a small, bounded and optional adjustment that the landlord has to initiate every year; the renewal is the only moment in a tenancy at which the price change has no limitation. Both margins are decided by the landlord. Section 6 uses this structure: we show that landlords with large portfolios exercise the annual update far more often than small ones, and we look for a response to the housing benefit at each margin separately.

[6] During the term the landlord cannot terminate the contract, except to recover the dwelling for own or first-degree family use.

[7] From March 2022, in response to the inflation surge, annual updates were capped at 2% by Royal Decree-Law 6/2022, a cap that was later extended, raised to 3% for 2024 and finally replaced by a new rental reference index (IRAV).

2.2 The 2022 rental assistance programs

Royal Decree 42/2022 created two programs of direct rental assistance, both paid to the tenant and both managed by the autonomous communities (NUTS-2 regions) within a national framework.

The *Bono Alquiler Joven* is aimed at tenants aged 35 or under. It pays a flat €250 per month for two years to tenants whose annual income does not exceed three times the IPREM, the public income indicator (€24,318 in 2022), and whose monthly rent does not exceed €600, or €300 for a room. Regions may raise the rent caps to €900 and €450 in municipalities with tense housing markets.

The general rental assistance program of the *Plan Estatal para el acceso a la vivienda 2022–2025* has no age condition. It covers a share of the monthly rent, up to 50% depending on the regional call,[8] with the same income threshold[9] and the same rent caps.[10]

The programs are not separately identified in our data, but age is a good proxy: tenants over 35 can only receive the general program, and tenants aged 35 or under received mostly the BAJ, which was aimed at them. The amounts received are also similar across programs for two reasons. Payments were staggered within the calendar year, so the amount observed in the first year reflects months of entitlement rather than the full annual benefit. And 50% of the average rent of recipients, about €500 per month, is €250, the flat BAJ amount. Appendix D shows that the amounts nonetheless differ in their structure, flat for the young and increasing in the rent for the old, and Section 7 exploits this difference in design.

Three features are common to both programs and matter for our interpretation. The benefit is paid into the tenant's bank account, so the landlord is not a party to the payment and does not learn about it from the program itself. The benefit is reported in the tenant's personal income tax return of the following year, so a landlord who asks a prospective tenant for a tax return, a common practice in new contracts, can observe past receipt; we use this timing to assess the information channel in Section 6. And the benefit is portable: a recipient who moves keeps it as long as the new rent is below the cap. The rent cap is verified at application and when the tenant moves, but the rent of a running contract is not monitored afterwards; a tenant whose rent rose above the cap would in principle have to report it, but there is no enforcement mechanism, so we do not treat the cap as a binding ceiling on what a

[8] The benefit could be granted for up to five years, but most regions granted it for two years, as in the BAJ.

[9] The threshold is raised to four times the IPREM for large families of general category and for persons with a disability, and to five times the IPREM for large families of special category and for persons with a recognised disability of 33% or more (Royal Decree 42/2022). The same adjustments apply to the BAJ.

[10] The two programs are formally compatible, with combined aid capped at 75% of the rent, and have separate application procedures. In practice very few tenants combined them, since both were rationed.

landlord could charge a recipient.

2.3 Rationing and coverage

Each region received a fixed budget for each program and allocated it on a first-come, first-served basis among eligible applications. Demand exceeded the budgets immediately. Figure 1 compares, for the 2022 call, the number of applications received in each region with the number of grants the regional budget could fund, and shows how fast the applications arrived. Catalonia received 39,000 applications for fewer than 10,000 funded slots, 15,000 of them on the first day; Madrid received 32,000 for 10,600 slots; Andalusia received 17,000 applications in three to four days for 11,400 slots; the Valencian Community received 20,000, 7,800 of them in the first week, for 3,800 slots. In most regions the funded slots were exhausted within the first week of the call, after which eligible applicants who had not yet been processed received nothing, even where the call formally remained open.

This rationing rule is the basis of our empirical design. Among tenants who met the income and rent conditions, receipt was decided by the order of arrival of applications, not by any assessment of the applicant: regions did not rank applications by need, income, rent burden or household type, and processed them until the budget ran out. Conditional on eligibility, therefore, there is no selection into treatment by the administration. The remaining source of selection is the decision to apply, and its timing. Applying early turns on awareness of the program and on the time to file in the first days of the call, which are plausibly unrelated to the trends in rents and income that eligible tenants would have followed without the benefit. Receipt is thus not random, but conditional on eligibility it is rationed by timing rather than by characteristics. The comparison group of eligible non-recipients includes tenants who applied and were not funded, who are the closest counterfactual to recipients, together with tenants who did not apply. We do not observe applications and cannot separate the two; Section 5 describes how we assess this limitation by using a matching difference in difference design.

Coverage was correspondingly low. We estimate that about 168,000 individuals received the benefit in 2022, roughly 85,000 through the young track and 83,000 through the general program, out of about 5.8 million eligible renters, a coverage rate of 2.9% among the eligible population and 1.8% of all 9.4 million renters in Spain. The amount actually received averaged about €1,800 per recipient in the first year, because many recipients entered the program during the year and part of the 2022 entitlements were paid in 2023. Section 5 returns to this timing. For the price analysis, the relevant implication of low coverage is that the program could not have shifted aggregate rental demand in any local market: with fewer than 2% of

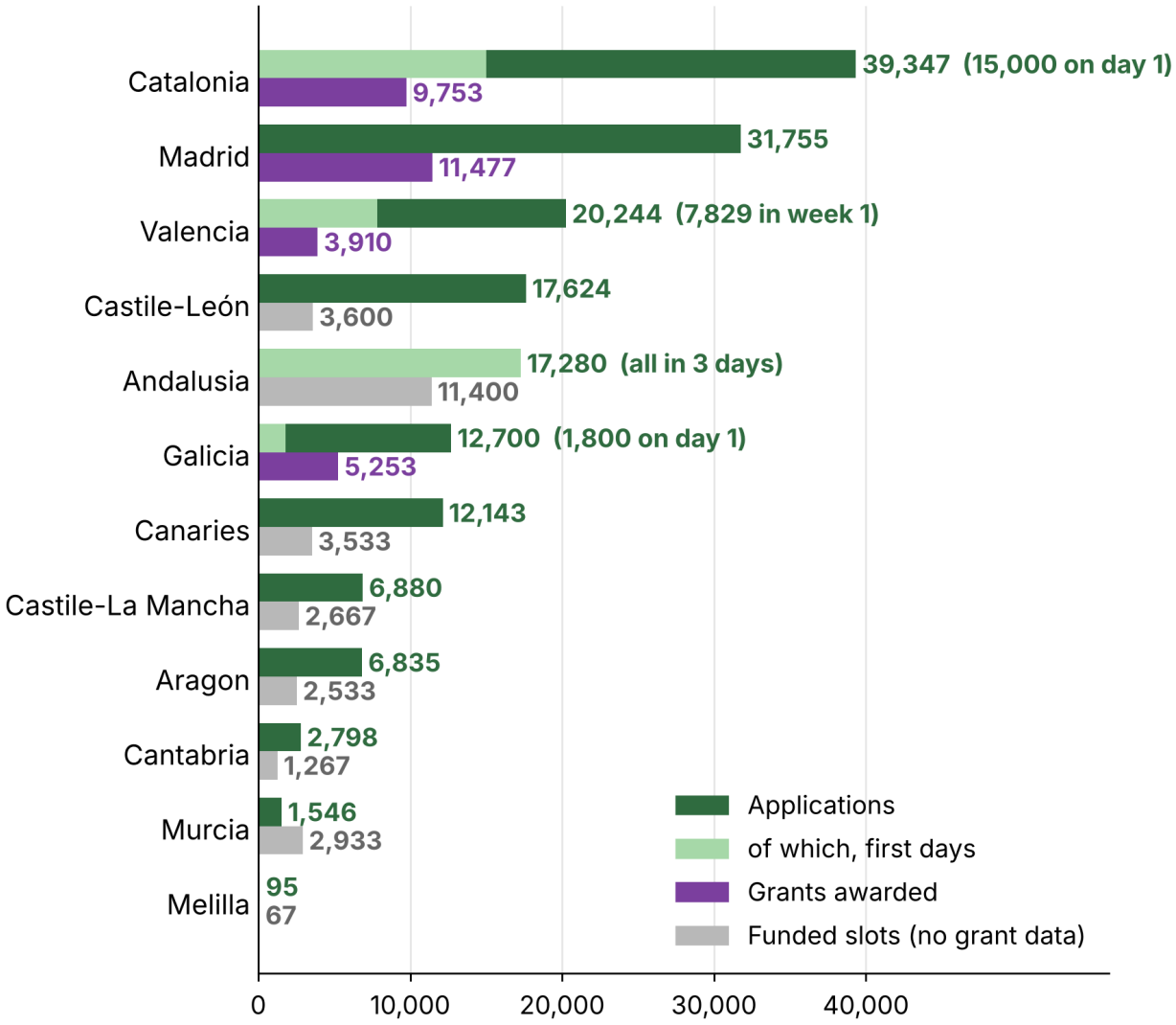


Figure 1: Applications, funded slots and grants in the 2022 call of the Bono Alquiler Joven, by region

Notes: Applications received in the 2022 regional calls of the Bono Alquiler Joven, of which those received in the first days or first week were reported; theoretical funded slots, computed as the regional budget divided by €6,000 (24 months at €250); and grants awarded, where a plus sign indicates a figure reported at an intermediate date. Regions without data on applications or grants are marked. Source: regional calls and resolutions compiled by Provivienda.

renters treated, any rent response we detect must arise within the relationship between an individual landlord and an individual tenant.

3 Conceptual Framework

This section organises the empirical analysis around the two mechanisms through which a housing benefit can change what a tenant pays and consumes once the market channel is shut down. We first show that the scale of the Spanish program rules out a market-level rent response. With the market rent fixed, we decompose the rent of a recipient into a price and a quantity component. The price component is the outcome of a bilateral negotiation between landlord and tenant, and we set out the conditions under which the landlord can move it. The quantity component is the tenant's housing choice, and we describe how the two designs of the transfer, an income grant and a proportional rent subsidy, change the budget constraint and the housing choices. The section ends with the predictions we take to the data.

3.1 Market incidence and program coverage

Consider a local rental market with downward-sloping demand and upward-sloping supply, and let r_0 denote the equilibrium rent before the program (Figure 2). Suppose a benefit b is paid to a share ρ of tenants. If recipients are spread across the demand curve, aggregate willingness to pay rises by ρb on average, demand shifts from D to D', and, to a first-order approximation, the equilibrium rent moves from r_0 to r_1 with

$$r_1 - r_0 \approx \rho\, b\, \frac{\varepsilon_D}{\varepsilon_D + \varepsilon_S}, \tag{1}$$

where ε_D and ε_S are the demand and supply elasticities in absolute value. The response is bounded above by ρb, the case of perfectly inelastic supply. The literature on the incidence of housing allowances studies programs where ρ is large, as in panel (a): housing benefits in France and the United Kingdom cover a substantial fraction of all tenants, so that even moderate elasticities translate into sizeable rent effects. In our setting, ρ is 1.8% of all tenants (2.9% of eligible tenants), as in panel (b). Even with fully inelastic supply, the case in which the fraction in equation (1) equals one, the market rent would rise by at most ρb, less than 2% of the benefit, two orders of magnitude below the pass-through rates documented elsewhere.[11]

[11] Coverage could in principle be concentrated in a few local markets. It is not: across the local markets defined in Appendix C, the share of tenants who receive the benefit is below 2% in most markets and exceeds 10% only in a very few markets (Appendix Figure C2, panel d).

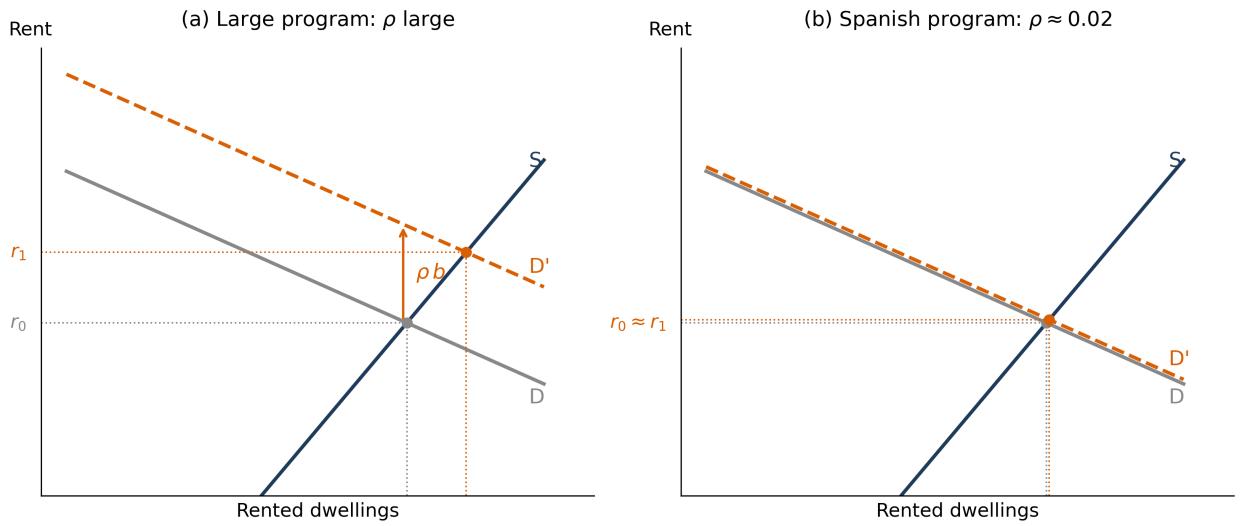


Figure 2: Market-level incidence depends on program coverage

Notes: Stylised local rental market with inelastic supply. Panel (a): a benefit received by a large share ρ of tenants shifts market demand from D to D' by ρb and raises the equilibrium rent from r_0 to r_1, so that part of the transfer goes to landlords. Panel (b): with $\rho \approx 0.02$, as in the Spanish program, the demand shift is negligible and the equilibrium rent is unchanged; any rent response must arise within the individual tenancy.

Two implications follow. First, any rent change we detect among recipients cannot be a movement along the market equilibrium; it has to originate in the individual tenancy. This is not a remote possibility. Equation (1) describes a competitive market in which every tenant pays the market rent at every point in time. The rental market is not that market as it contains frictions: tenants and dwellings are heterogeneous, finding a match takes time and effort, and once formed a match is protected by a multi-year contract. At any point in time most tenants are therefore sitting tenants, whose rent is not re-set by the market but by the two parties to the lease, within the limits of the contract and the law, and the rent of a new match is the outcome of a negotiation between a tenant with a costly outside option and a landlord with a vacancy to fill (Wheaton, 1990; Piazzesi et al., 2020). A market rent that does not move is thus compatible with a landlord raising the rent of a particular tenant, and whether a recipient pays more is a question about this bilateral price rather than about the market price. Second, eligible non-recipients, who form our comparison group, face the same market rent as recipients, so the difference-in-differences comparison is not contaminated by the general-equilibrium spillovers that a high-coverage program would generate.

3.2 The rent of a recipient: price and quantity

The rent paid by tenant i is the product of a price per unit of housing services and a quantity of housing services, $R_i = p_i h_i$, so that

$$\Delta \ln R_i = \underbrace{\Delta \ln p_i}_{\text{bilateral price change}} + \underbrace{\Delta \ln h_i}_{\text{housing choice}} . \tag{2}$$

With the market rent fixed by equation (1), a change in the rent paid by a recipient can come from the two sources that equation (2) separates. The first term is a change in the price of the dwelling the tenant occupies, which within a running contract is set by the landlord within the limits of the law and, at renewal, is negotiated between the two parties. The second term is a change in the quantity of housing the tenant consumes, which the tenant adjusts mainly by moving to a different dwelling.

Our data allow us to separate the two terms in a simple way. For a tenant who stays in the same dwelling, h_i is fixed and any change in the rent declared by the landlord is a price change; rent per square meter confirms it. For a tenant who moves, we observe the move and the characteristics of the new dwelling, but we do not observe the rent of a dwelling whose landlord is not in the panel, so the rent of movers is not available to us. We therefore use rents to study the price term, in the first part of the paper, and use mobility, dwelling characteristics and household composition to study the housing-choice term, in the second.

3.3 Bargaining over the price paid by a recipient

Three conditions have to be met for the price term in equation (2) to be positive. We write the expected bilateral price change in year t as

$$\Delta \ln p_{it} = p(q_i, m_{it}, s_i) \tag{3}$$

where q_i is the probability that the landlord knows, or infers, that the tenant receives the benefit; m_{it} indicates that a margin at which the rent can be changed is open in year t; and s_i is the share of the benefit the landlord can appropriate at that margin given the tenant's bargaining position. Each term maps into a feature of the setting and into a test in Section 6.

Information. The benefit is paid to the tenant, and the landlord is not informed by the program. A landlord can learn about receipt only if the tenant discloses it, or if the landlord sees the tenant's tax return, in which the benefit appears the year after it is received. The second channel is open mainly when a new contract is signed and the landlord asks for the documents. We test whether rents in contracts signed in 2023 are higher for tenants whose

2022 tax return shows the benefit, against a placebo in 2022 contracts, when the benefit is not yet visible.

Margins. Section 2.1 described the two margins. Within the contract term, the landlord may update the rent once a year by at most the reference index (2% from March 2022) and only after notifying the tenant 30 days in advance to the anniversary of the contract. This margin is bounded: for a rent at the €600 cap, the maximum update is €12 per month, about 5% of a €250 benefit. Extraction at this margin can therefore only mean exercising an update that would otherwise have been forgone. At renewal, the rent is free, and if a landlord knows about the benefit he could renegotiate. Both margins are decided by the landlord, and the cost of exercising the in-contract update, a yearly notification per dwelling, is a fixed cost per unit that falls with portfolio size (Genesove, 2003; Guasch and Marshall, 1987; Hubert, 1995; Gallin and Verbrugge, 2019). We therefore expect m_{it} to differ by landlord type, and we document it directly: how often each type exercises the annual update, and how the rent of a sitting tenant evolves over the contract term with each type. We then test for a response of the benefit at each margin separately.

Bargaining. Given information and an open margin, the share s_i is the outcome of a negotiation in which the tenant's threat point is the outside option: the cost of finding another dwelling and moving to it, and the credibility of the threat to do so. In a market with search frictions this cost is substantial, and it is higher for households with children and for tenants with long tenure, who have accumulated dwelling- and neighbourhood-specific capital. The benefit itself affects the outside option, because it is portable to any dwelling below the cap, which strengthens the tenant's hand. Local market power is a further candidate determinant of s_i: a landlord holding a large share of the local rented stock faces tenants with fewer alternatives (Barbieri and Dobbels, 2025; Ramoutar, 2025). We test both, using tenant characteristics fixed before the reform and measures of local concentration built from the landlord side of the data.

Equation (3) implies that a null average price effect could simply mask heterogeneity, the subsidy may still be captured in settings where landlords have both the incentive and the ability to raise rents. Large landlords have the operational capacity to reprice, lease renewals offer a contractual margin for adjustment, and immobile tenants have weak outside options. If the subsidy is not captured even under these favorable conditions, we can more confidently conclude that landlords do not extract the benefit under the absence of aggregate demand shifts and with legal price adjustment frictions.

3.4 Housing choices under the two designs

With p_i fixed, the benefit changes the recipient's housing consumption choice only through the budget constraint, and how it does so depends on the design of the transfer. The theory of housing subsidies distinguishes two goals a program may serve: raising the living standard of low-income households, and raising their consumption of housing, in quantity or quality (Brueckner, 2011). Among transfers that do not condition on the dwelling, two canonical designs serve these goals to different degrees. An income grant pays a fixed amount regardless of what the household spends on housing. A proportional rent subsidy pays a fraction of the rent bill. The Spanish program contains one of each: the young track is an income grant, the general track a proportional rent subsidy.

Let the tenant have income y, consume housing h at price p and a composite good x, and let $\bar{R}$ be the rent cap. Without the program, $x + ph = y$. The income grant pays G as long as the rent is below the cap:

$$x + ph = y + G \cdot \mathbf{1}[ph \leq \bar{R}]. \tag{4}$$

The proportional rent subsidy pays a share β of the rent under the same condition:

$$x + \left(1 - \beta \cdot \mathbf{1}[ph \leq \bar{R}]\right) ph = y. \tag{5}$$

The grant shifts the budget line outward in parallel: the relative price of housing is unchanged. The subsidy lowers the price of housing to $(1 - \beta)p$ and rotates the budget line around their x intercept, $(x, h) = (y, 0)$. In both cases the budget set has a notch at $\bar{h} = \bar{R}/p$: one more unit of housing above the cap costs the whole transfer, so tenants whose desired housing would exceed the cap are pushed to stay below it (Kleven and Waseem, 2013).

Figure 3 compares the two designs at the same public cost, following Brueckner (2011). Without the program the tenant faces the black budget line and chooses h_0. The proportional rent subsidy lowers the price of housing to $(1 - \beta)p$: the budget line rotates around the intercept y and becomes steeper, and the tenant moves to a new tangency. Since housing is a normal good, this tangency lies above the original one, at $h_{PRS} > h_0$. The cost of the subsidy to the government is $G = \beta p h_{PRS}$. Now suppose the government gives the tenant the same amount G as an income grant. The budget line shifts outward in parallel, and because G is exactly what the subsidy cost, the bundle chosen under the subsidy is still affordable: the grant line passes through the PRS point. The tenant, however, does not stay there. At that point the indifference curve is tangent to the steeper subsidy line, so it cuts the flatter grant line, and the tenant reaches a higher indifference curve by moving down the grant line

towards other goods. With both goods normal, the new tangency lies to the north-east of the original bundle, so that $h_0 < h_{IG} < h_{PRS}$. The gap $h_{PRS} - h_{IG}$ is the substitution effect of the lower price of housing, and $h_{IG} - h_0$ the income effect that both designs share (Slutsky, 1915). For a given public cost, the proportional rent subsidy therefore buys more housing and the income grant more welfare: subsidizing a particular good raises its consumption by more than an equivalent grant, and raises the household's utility by less.

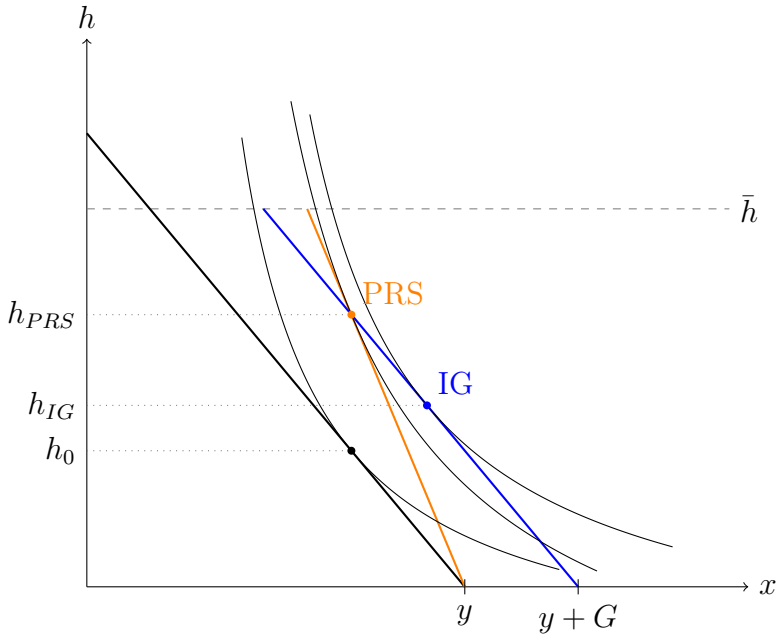


Figure 3: Housing choices under an income grant and a proportional rent subsidy of equal cost

Notes: Horizontal axis: other goods x; vertical axis: housing h. Black: budget line without the program, $x + ph = y$, with the tenant's choice h_0. Orange: proportional rent subsidy, equation (5); the line rotates around $(y, 0)$ and the tenant chooses h_{PRS}. Blue: income grant $G = \beta p h_{PRS}$, equal to the public outlay under the subsidy, equation (4); the line shifts in parallel through the PRS bundle and the tenant chooses h_{IG}. Thin curves are indifference curves, drawn schematically with $\beta = 0.5$. Above the cap $\bar{h}$ the transfer is lost and the budget set reverts to the black line.

Two features of our setting shape where this comparison can show up. First, housing is adjusted by moving, and moving is costly, which generates an inaction band around current housing (Chetty and Szeidl, 2007); and the transfer lasts two years, an upgrade financed by it would have to be reversed when it ends unless the tenant expects higher earnings by then. Both frictions make the housing response small under either design. Second, the lower price of housing under the proportional rent subsidy enters the tenant's decision when a dwelling is chosen: between two otherwise similar recipients, the one with the proportional rent subsidy has a reason to move that the one with the income grant does not. The substitution effect should therefore appear in mobility, and the difference in moving between the two groups

is our measure of it. The income effect, common to both designs, can also change housing consumption without a move: h measures housing per person as much as the size or quality of the dwelling, and a recipient can consume more of it by sharing the dwelling with fewer people.

What the data add to the figure is where recipients actually end up. The theory fixes the ordering of the three bundles but not their distance: the income grant could leave housing almost unchanged, with the whole transfer going to other goods, or raise it noticeably if housing demand is income-elastic; the proportional rent subsidy could induce many moves or very few, depending on how the lower price weighs against the cost of moving. With the price of housing fixed, the choices of the two groups of recipients reveal where the tangencies lie: how much of the transfer housing absorbs under each design, and whether the lower relative price moves tenants at all. The distance between the bundles is what matters for policy. A government that wants low-income households to live in more or better housing should subsidize the rent in proportion; a government that wants to raise their living standard, or lower their rent burden, obtains more welfare per euro from the grant, and the larger the gap between h_{PRS} and h_{IG}, the more the choice of design matters. Where recipients end up also speaks to the housing label. If the label works as the literature on earmarked transfers suggests, recipients of the grant would locate above the tangency that the income effect alone predicts. Although, as housing is costly to adjust, a euro for housing could be treated as a euro like any other. If this is the case, the function of labeling as housing would be just the target of the policy: tenure status would act as a proxy for holding no housing wealth, and for low income.

3.5 Predictions

On the price side, we expect the rent paid by a recipient who stays in the same dwelling to follow the same path as that of eligible non-recipients. With coverage below 2% of renters, equation (1) rules out a market response, and the three conditions of equation (3) make a bilateral response unlikely in a running contract: the landlord is not informed by the program, the annual update is capped, and the benefit is portable, which strengthens the tenant's outside option. A null on average could nonetheless hide a response where the landlord's bargaining position is strongest. This would be the case among large landlords, who bear a lower cost of exercising the annual update and the marginal cost of a vacancy is lower. A landlord with several tenants also sets the rents of recipients and non-recipients side by side, so any differential treatment should be visible within the same portfolio, where the landlord's general repricing policy is held fixed. A landlord who cannot identify recipients but knows

that low-income tenants in cheap dwellings are the likely beneficiaries could reprice that segment as a whole after 2022. A new contract is the one moment at which the two missing conditions coincide: the tax return is requested, so receipt becomes visible, and the rent is free, so there is no cap on the increase; a response could therefore appear in contracts signed once receipt is visible. Finally, the share a landlord can extract depends on the tenant's outside option. Tenants with high moving costs, such as households with children or long tenure, are more willing to accept an increase in order to stay, and tenants in concentrated local markets have fewer landlords to turn to; a response should thus be larger in both cases. If no response appears in any of these configurations, the null is not the average of offsetting forces but the absence of extraction.

On the tenant's side, the two designs and shift the budget constraint differently and by their incentives, they should produce different housing choices. Both raise housing demand through the income effect; but the proportional rent subsidy adds a substitution effect, which in our setting can only materialize when a dwelling is chosen. We therefore expect recipients of the proportional rent subsidy to move more than recipients of the income grant. However, since moving is costly and the transfer lasts only two years, the overall housing response should be small. Changes in household composition are a housing response common to both designs, since sharing a dwelling with fewer people raises housing per person without a move. With rents fixed, whatever is not spent on housing appears in the household budget, and we follow it through three margins. The first is labour supply: a transfer that raises unearned income could lower earnings through an income effect, and the income threshold of the program adds a reason to hold earnings down close to it, so we look for responses on both the intensive and the extensive margin. The second is saving: a temporary transfer to liquidity-constrained households should be spent rather than saved. The third is the housing label itself. In theory a euro received for housing is a euro like any other, but households have often been found to spend more of a transfer on the good it is labeled for; housing, however, is a good that is costly to adjust, so the label may have less traction here than for goods that can be bought in small increments.

4 Data

Our analysis draws on the *Panel de Hogares*, an administrative panel constructed by the Spanish Tax Agency (AEAT)[12]. The panel combines personal income tax returns with third-

[12]In collaboration with the National Institute of Statistics (INE) and the Institute for Fiscal Studies (IEF). The data can be requested through the IEF data request portal and are described in the accompanying methodological report.

party reported records, social security registries, census information and wealth data for a representative sample of Spanish households followed between 2016 and 2023. In 2023 it covers 1.19 million households (6.66% of households in the common fiscal regime territory[13]) and 3.55 million individuals (7.83%), with about 1,000 variables per household, individual and real estate property. The sample follows a stratified design by region, household type and income bracket, with 1,530 strata.[14]

The landlord-tenant link. The defining feature of the panel for our purposes is that it links landlords and tenants who participate in the same rental relationship. The real estate modules records, for every individual, their tenure status (owner, tenant or other) and an anonymised cadastral reference identifying the dwelling. We identify a landlord-tenant link whenever an individual who declares renting a given housing unit can be matched, within the same panel wave, to one or more individuals who hold an ownership right over that same unit. For every linked tenancy we observe the tenant's income by source, household composition, age, location down to the census tract and benefit receipt, alongside the landlord's property-level rental income and deductible expenses, the contract start date, dwelling characteristics (surface, cadastral value, year of construction) and the landlord's full real estate portfolio with its declared uses. The landlord's fiscal record is reported at the property level rather than as a portfolio aggregate, which is what allows us to link a specific rental income to a specific tenant. A kinship module lets us flag and exclude rentals between relatives.

Our sample of landlords is restricted to natural persons who declare real estate income in their personal income tax return. Corporate and public landlords report under the corporate income tax and are not linked to individual taxpayers; they are estimated to own 8% of the residential rental units in Spain[15]. We classify landlords by the number of dwellings they rent out, measured over their whole portfolio: small (one dwelling), medium (two to five) and large (six to fifty); portfolios above fifty units are excluded.[16] Because a tenant who moves may change landlord, landlord type is fixed at the tenant's last observation before 2022 and carried over the tenant's panel; tenants first observed in 2022 or later are excluded from the heterogeneity analyses. We classify tenancies as whole-dwelling rentals or room rentals, the latter identified when several unrelated tax households share the same unit, and the data exclude short-term and tourist rentals.

[13] Navarra and Basque Country are excluded

[14] The panel covers all autonomous communities except the Basque Country and Navarre, which operate under a separate fiscal regime (*régimen foral*) and do not transfer administrative records to the AEAT.

[15] See Banco de España, 2023

[16] The classification broadly follows the definition of large holder (*gran tenedor*) in the 2023 Housing Law (Law 12/2023), which designates owners of five or more residential properties as large holders, with regional variation in the threshold.

Rents. Our price measure is the rent that the landlord declares for the cadastral reference linked to the tenant, not an amount reported by the tenant. Landlord-reported rent is the relevant price for the question we ask, and it is taxable income for the landlord, who faces a direct fiscal incentive to report it accurately. We use the rent of the whole dwelling, in logs, as the main outcome, and rent per square meter, using the cadastral surface, as the price per unit of housing. For tenants who move, the rent of the new dwelling is observed only if its landlord is also in the panel; the rent outcomes therefore describe tenants in observed tenancies, and the housing-choice outcomes in Section 7 rely on mobility and dwelling characteristics rather than on the rent of movers.

Benefit receipt and other outcomes. Housing benefit receipt and the amount received are reported to the tax agency by the paying administration, and obligates the receiver to fill the personal income a tax return. We define a recipient as a tenant who receives the benefit in a given year, through either program. The two programs are not separately identified in the data; tenants over 35 can only receive the general program, and the amounts received by tenants aged 35 or under are consistent with the flat young track amount for most of them (Appendix D). Disposable income after rent is gross income, including the benefit, minus annual rent, and measures the resources available for non-housing consumption.[17] We also observe labour income, social security contributions, unemployment benefits, self-employment, capital and transfer income, the balance of bank accounts at 31 December, financial asset holdings, and the composition of the household at the dwelling: number of tax filers and fiscal family units, joint filing, presence of a spouse and of descendants, and the number of non-owner residents recorded in the unit. Residential mobility is a change of cadastral reference between consecutive years.

Sample. We keep individuals who are not owners of their main residence, exclude minors and dependants living with their parents, keep monthly rents between €75 and €3,500 to remove implausible values, and require non-negative adjusted gross income.[18] The resulting tenant panel contains about 300,000 tenant-year observations with price information. The estimation sample consists of recipients and eligible non-recipients, where eligibility is defined by the income and rent conditions of the programs: about 70,000 tenant-year observations, with about 1,100 recipients and 19,000 eligible non-recipients observed at least once.

We further require recipients to be observed in at least one year before their first receipt,

[17] Annual rent is the rent paid by the tenant, that is, the dwelling rent divided by the number of tax filers at the dwelling, times twelve.

[18] We use the adjusted gross income measure that does not net out subsidies, to avoid a mechanical link between treatment and the income-based sample restriction.

so that the treatment effect compares years with and years without the benefit for the same tenant, and we drop eligible non-recipients who share a dwelling with a recipient, whose outcomes could respond to the benefit of a co-resident. Appendix Table F1 reports pre-period characteristics of recipients and eligible non-recipients. In both groups, tenants sort in a similar way across landlord types.

5 Empirical Strategy

This section describes our identification strategy for estimating the effect of housing benefit receipt on rental market outcomes. We exploit the gap between program eligibility and actual receipt in a difference-in-differences (DiD) framework, and validate our design through event study specifications and robust estimators for heterogeneous treatment effects.

Our treatment group consists of tenants who received the housing benefit (through either the general or the BAJ track) at any point during 2022 or 2023[19]. The comparison group consists of tenants who satisfied the income and rent conditions of the programs but did not receive the benefit.[20]

Our identifying assumption is that, conditional on eligibility, individual fixed effects and area-by-year fixed effects, the timing of receipt is unrelated to the trends in rents, income and mobility that recipients would have followed without the benefit. This assumption is supported by the institutional features of the program described in Section 2.3: conditional on meeting the eligibility criteria, actual receipt was largely determined by administrative factors, such as the timing of application submission and the pace of regional budget exhaustion, rather than by individual characteristics that also drive rent trajectories. The main threat is that we do not observe applications. If tenants who applied early differ from other eligible tenants in ways that affect their trajectories, the estimates would reflect selection. We address this threat in three ways: event studies test for differential trends before receipt; a matched comparison group restricts the comparison to eligible non-recipients with the same pre-treatment profile as recipients; and estimators that are robust to heterogeneous and staggered effects confirm that the baseline does not rest on comparisons between early and late recipients (Section 5.2).

We do not include time-varying controls. The tenant's age is collinear with individual and year fixed effects, and dwelling characteristics, room rental status and household composition

[19]Receipt is staggered: roughly half of recipients first receive the benefit in 2022 and half in 2023, in part because several regions paid 2022 entitlements in 2023

[20]Receipt requires filing a personal income tax return, which ensures that all recipients are observed in the tax records. Eligible non-recipients who do not file are not in the panel, so the comparison group is restricted to tenants with a comparable administrative footprint.

change when the tenant moves or when co-residents change, so they are outcomes of the behaviour we study rather than controls for it. Appendix Table B3 shows that the estimates are unchanged when contemporaneous controls or pre-2022 covariates interacted with year are added, and Appendix Tables B1 and B2 that they are unchanged under alternative area-by-year fixed effects, from region-by-year to municipality-by-year.

For outcomes tied to the contract cycle, namely the signing of a new contract and the rent at renewal, we add contract-start-year-by-year fixed effects, $\lambda_{c(i),t}$, where $c(i)$ is the start year of the tenant's contract before 2022. Recipients can hold more recent contracts than eligible non-recipients, so their contracts come up for renewal more often in 2022 and 2023; these fixed effects compare tenants whose contracts are at the same point of the cycle. We use the same fixed effects in heterogeneity analyses by tenant age, since age and contract vintage are correlated.

Our baseline specification is

$$Y_{it} = \alpha_i + \mu_{m(i)} + \gamma_{p(i),t} + \beta\, D_{it} + \varepsilon_{it}, \tag{6}$$

where Y_{it} is the outcome of interest for tenant i in year t, α_i are individual fixed effects, $\mu_{m(i)}$ are municipality fixed effects, which absorb time-invariant differences across municipalities, and $\gamma_{p(i),t}$ are province-by-year fixed effects, which absorb region-specific shocks and trends, including the market-level effect of the program itself (equation (1)). D_{it} indicates that tenant i receives the benefit in year t. The coefficient β is the average effect of receiving the benefit in a given year relative to eligible non-recipients, identified from within-tenant variation. Standard errors are clustered at the individual level. We do not include time-varying controls, since dwelling characteristics and household composition are themselves outcomes of the behaviour we study; Appendix Tables B1, B2, and B3 show that adding controls or changing the area-by-year fixed effects leaves the estimates unchanged.

The outcomes follow the two parts of the question. To ask whether the benefit reaches the tenant, we need to know whether the landlord takes part of it. The rent declared by the landlord for the dwelling is the price the tenant pays; for a tenant who stays, it is a pure price change, and rent per square meter confirms whether the price per unit of housing moved. A landlord can only move that price at two margins, so we look at each of them: the probability that the rent is updated within the contract, which is capped by law and has to be initiated by the landlord; and the probability of a new contract together with the rent at renewal in the same dwelling, where the price is free. If extraction occurs anywhere, it should show up at the free margin, and among the landlords and tenants where the conditions for it are met.

To ask what the tenant does with the benefit, we follow it through the budget. Disposable

income after rent, net income minus annual rent, tells us how much of the transfer remains available for non-housing consumption once rent and earnings have adjusted. Residential mobility is the main way a tenant can change housing consumption, so it is our first measure of a housing response; its expected sign depends on the design, since a lower relative price of housing encourages moves while a temporary transfer with a rent cap discourages them. The characteristics of the dwelling, surface and room rental, tell us whether a move comes with more housing. The number of filers at the dwelling, joint filing and the presence of a spouse inform us about changes in household composition and household formation. A transfer to a young tenant can finance settling with a partner or living without flatmates as well as moving, and these responses change housing consumption per person without changing the dwelling. Labour income, the probability of having any labour income and unemployment income test for an income effect on labour supply, on the intensive and the extensive margin, which the income threshold of the program could amplify. Finally, bank balances and financial asset holdings tell us whether the part of the transfer that is not spent on housing is consumed or saved.

To test for pre-trends and trace the dynamics of the effects, we estimate the event-study version of equation (6),

$$Y_{it} = \alpha_i + \mu_{m(i)} + \gamma_{p(i),t} + \sum_{k \neq -1} \beta_k \, \mathbf{1}[t - t_i^* = k] + \varepsilon_{it}, \tag{7}$$

where t_i^* is the first year in which tenant i receives the benefit and $k = -1$ is the reference period. Both cohorts are pooled; $k = 0$ is the first year of receipt for every recipient and $k = +1$ is observed only for the 2022 cohort. Two-way fixed effects estimators can be biased when treatment is staggered and effects are heterogeneous across cohorts, we re-estimate the event study using Callaway and Sant'Anna (2021), which compares each cohort of recipients with eligible tenants not yet treated and aggregates cohort-specific effects, using a common pre-treatment base period. Because our treatment is not absorbing, we also use the estimator of de Chaisemartin and D'Haultfœuille (2020), which allows treatment to switch off and estimates effects from tenants who switch into treatment relative to tenants with the same past treatment, allowing for province-specific trends. Both estimators give average effects on prices and income that are similar to the baseline ones, with flat pre-treatment coefficients.

5.1 Heterogeneous effects

The predictions in Section 3.5 concern groups of landlords and tenants. For a grouping $G_i \in \{1, \ldots, G\}$ defined at the tenant's last observation before 2022, so that group membership

cannot respond to treatment, we estimate

$$Y_{it} = \alpha_i + \mu_{m(i)} + \gamma_{p(i),t} + \delta_{G_i,t} + \beta\, D_{it} + \sum_{g=2}^{G} \beta_g\, D_{it}\, \mathbf{1}[G_i = g] + \varepsilon_{it}, \tag{8}$$

where $\delta_{G_i,t}$ are group-by-year fixed effects. The coefficient β is the effect of the benefit for recipients in the reference group relative to eligible non-recipients in that group, and each β_g is the differential effect for group g relative to the reference group, a triple difference. The total effect for group g is $\beta + \beta_g$, whose standard error we compute by the delta method.[21] We report both the differentials and the total effects, together with the p-value of the test that the effects are equal across groups. The group-by-year fixed effects are essential. Without them, any difference in post-2022 trends between groups, among recipients and non-recipients alike, is loaded onto the differentials and attributed to the benefit. This is not a hypothetical concern: large landlords raise the rents of all their tenants faster than small landlords after 2022, as we show in Section 6.

The groupings are landlord type (small, medium, large), tenant age (over 35 versus 35 or under, which also distinguishes the two programs, and a finer split at 25 for labour outcomes), presence of children and tenure in the dwelling. Rents at renewal are estimated in a specification with tenant-by-dwelling fixed effects, so that the rent is compared within the same tenancy, interacting D_{it} with an indicator for a new contract signed in the same dwelling in year t, and including the contract-cycle fixed effects described above. The coefficient on the interaction is the additional change in rent for a recipient in the year of renewal relative to a non-recipient who also renews.

5.2 Robustness: matching and heterogeneity-robust estimators

We assess the baseline design in three complementary ways; the corresponding tables and figures are in Appendix B.

Matched comparison group. To address selection into application, we build a comparison group of eligible non-recipients who resemble recipients on pre-treatment observables. Within exact cells defined at the province level (48 cells), each recipient is matched to its nearest eligible non-recipients by the Mahalanobis distance (Rubin, 1980) over pre-treatment individual and household income, cadastral value, surface and year of construction of the

[21] The delta method approximates the variance of a function of estimated coefficients using a first-order Taylor expansion around the point estimates: for the linear combination $\beta + \beta_g$, the variance is $\mathrm{Var}(\hat{\beta}) + \mathrm{Var}(\hat{\beta}_g) + 2\,\mathrm{Cov}(\hat{\beta}, \hat{\beta}_g)$, taken from the clustered variance-covariance matrix of the regression.

dwelling, age, family size, number of children, room rental status, and the contract start year. We use 1:3 matching and estimate equation (6) on the matched sample, replacing the region-by-year fixed effects with matched-group-by-year fixed effects, so that each recipient is compared only with its own control in every year. Inference uses a cluster bootstrap at the matched-group level. Besides addressing selection based on observables, this structure addresses the staggered-timing problem of two-way fixed effects estimators: because each matched group contains one recipient and never-treated controls only, the estimates do not use already-treated units as controls for later-treated ones, which is a source of bias (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2024).

6 Results I: Do Landlords Change the Rental Price to Recipients?

This section asks whether landlords change the price paid by recipients through any of the margins that bargaining leaves open. We start with the average effect of the benefit on rents and the other main outcomes, then document how the capacity to reprice differs by landlord type, and look for a response at each price margin and for each landlord type. We then turn to the conditions under which a landlord could learn about or price the benefit: information, the landlord's own portfolio, pricing the eligible segment, and local market power.

Table 1: The effect of the housing benefit: baseline

	(1) Log rent	(2) Log rent/m^2	(3) Disp. income	(4) Labour inc.	(5) Unemployment prob.	(6) Moved
Housing benefit	0.008	0.002	1473.352***	-313.264	0.007	0.005
	(0.009)	(0.010)	(195.826)	(207.385)	(0.017)	(0.014)
Observations	68988	68756	68988	68988	68988	61568
Adj. R^2	0.835	0.923	0.629	0.719	0.384	0.219

Sample: eligible tenants, 2016–2023. Treatment equals one in years of receipt. Individual, municipality and province-by-year fixed effects.

Rent as declared by the landlord for the dwelling. Labour income and net disposable income in euros per year. Standard errors clustered by individual in parentheses.

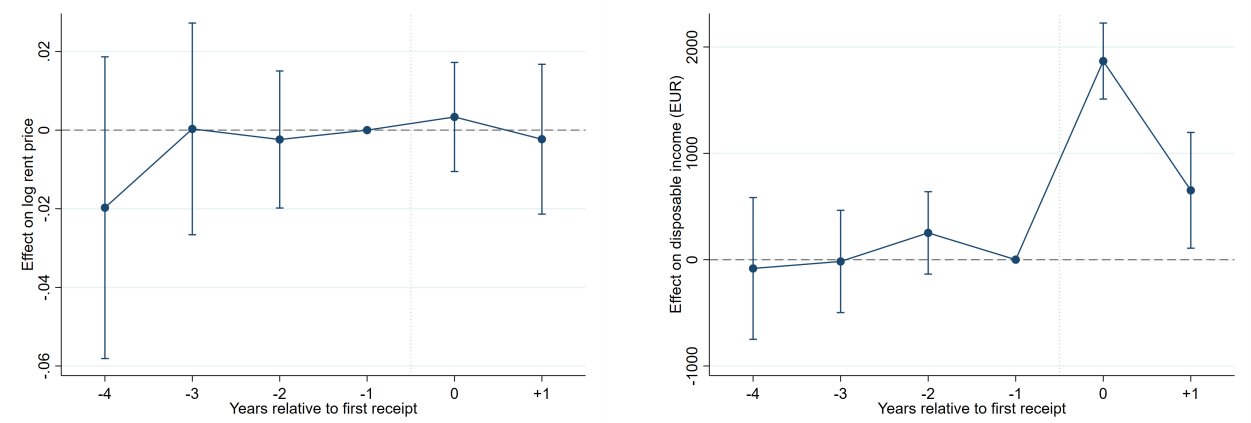
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.1 Baseline: rents do not move and the benefit reaches tenants

Table 1 reports estimates of equation (6) for the six main outcomes. Rents do not move. The rent declared by the landlord for the dwelling of a recipient is 0.8% higher than that of eligible non-recipients, a difference that is not statistically different from zero (standard error 0.9%, column 1); rent per square meter is unchanged (column 2); the 95% confidence interval

for log rent excludes increases above 2.6%. Disposable income after rent rises by €1,473 (column 3), labour income falls by €313 but not significantly (column 4), and the probability of an unemployment spell and of moving do not change (columns 5 and 6). Section 7 returns to the income outcomes.

Figure 4 shows the event studies. For log rent, the coefficients are close to zero and insignificant before and after first receipt: there is no differential trend and no response. With rents unchanged, the transfer shows up in disposable income, which is flat before receipt and jumps in the first year of the benefit. The smaller coefficient in the second year reflects that payments were not evenly distributed across regions and years, and that any labour-income response would appear in the second year (Section 7.2.1). Appendix B shows that the matched comparison group and the heterogeneity-robust estimators give the same picture.



(a) Log rent

(b) Disposable income after rent (€ per year)

Figure 4: Event studies: log rent and disposable income

Notes: Coefficients $\hat{\beta}_k$ from equation (7), with $k = -1$ as the reference period, both cohorts pooled. Individual, municipality and province-by-year fixed effects. Bars are 95% confidence intervals; standard errors clustered by individual.

By themselves, these estimates say that the rents of recipients follow the same path as those of eligible non-recipients, and that the transfer reaches the tenants budget. Under equation (3), a null average on rents can arise because none of the three conditions for extraction is met, or because they are met only for a subset of tenancies that the average dilutes. The rest of the section looks at the subsets.

6.2 Repricing capacity differs by landlord type

The within-contract update is optional and has to be initiated every year by the landlord. Appendix Table F2 shows how often each landlord type exercises it, in the full sample of tenancies between 2016 and 2023, conditional on municipality-by-year fixed effects and dwelling characteristics. The rent of a dwelling changes in a given year in 43% of tenancies with a single-unit landlord, 50% with a medium landlord and 60% with a large one. The gradient comes almost entirely from within-contract updates (39%, 47% and 57%), not from new contracs (8%, 9% and 10%). Large landlords are ∼18 percentage points more likely than small ones to exercise the annual update, and 1.7 points more likely to renew.

Figure 5 shows what this implies for a sitting tenant. It plots the rent of contracts signed from 2019 onwards, all of them within the five-year term of the 2019 LAU, relative to the signing year, with contract fixed effects, separately by landlord type. Four years into the contract, the rent of a tenant with a small landlord is 2.2% above its initial level, with a medium landlord 5.5%, and with a large landlord 8.7%. Large landlords thus apply close to the maximum update allowed every year, whereas small landlords let most years pass without one. This is consistent with a fixed cost of adjustment per dwelling, the yearly notification, that is spread over a larger portfolio.

Although the capacity to reprice a sitting tenant exists and is concentrated among large landlords, this repricing capacity is not used against recipients. The first panel of Figure 6 reports the effect of the benefit on log rent for all landlords and the total effect for each landlord type. The three effects are not statistically different from each other. Only for large landlords is the estimate distinguishable from zero, at 2.7% at the 90% level. However this significance disappears once controlling for contract cycle (Table F4). As this patter is also not present when the oucome is rent per square meter (second panel), we do not interpret it as a robust price response. Large landlords raise rents more often than other landlords, but they raise them for all their tenants alike, recipients or not. This common trend is what the landlord-type-by-year fixed effects of equation (8) absorb.

6.3 Price margins by landlord type

Figure 6 reports estimates of equations (6) and (8) for the three price margins, for all landlords and by landlord type: the rent of the dwelling, rent per square meter, the probability that the rent is updated within the contract. The full estimates are in Appendix Table F3.

For all landlords, none of the margins responds. The rent of the dwelling and the rent per square meter do not change. Recipients are not more likely to see their rent updated within the contract, and not more likely to sign a new contract once the contract cycle is held fixed,

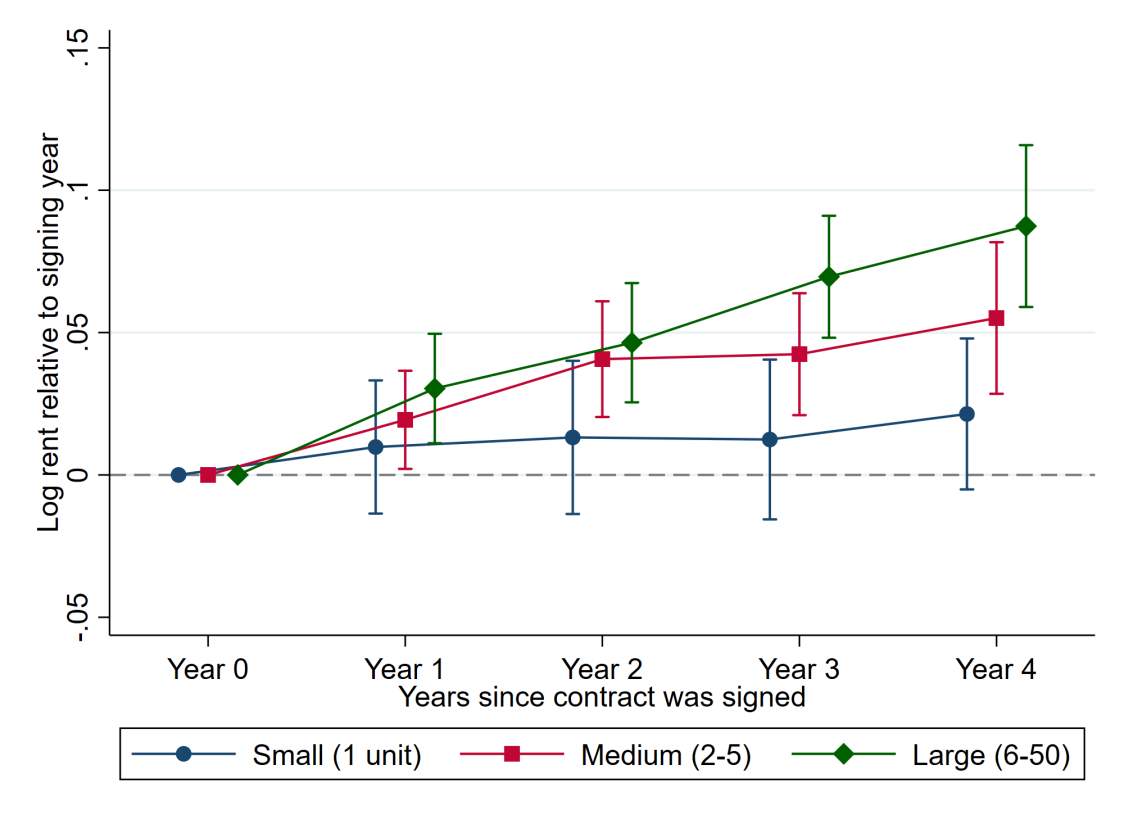
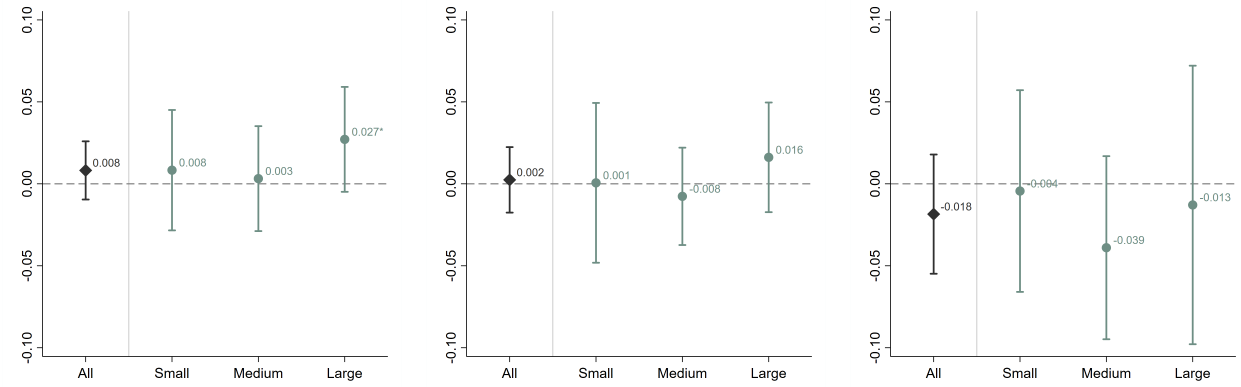


Figure 5: Rent within the contract term, by landlord type

Notes: Log rent relative to the signing year for contracts signed in 2019 or later, within the five-year legal term, cut at the first new contract. Contract fixed effects; years relative to signing interacted with the landlord type of the contract; contracts signed in January, so that the signing year is a full year of rent. Bars are 95% confidence intervals. The differential between large and small landlords in year 4 is 0.066 (standard error 0.020); between medium and small, 0.033 (0.019).



(a) Log rent (b) Log rent per m^2 (c) Probability of price update

Figure 6: Effect of the benefit on price margins, all landlords and by landlord type

Notes: Each panel reports the effect of receiving the benefit on the outcome in the panel title, for all landlords (black, equation (6), baseline sample) and the total effect by landlord type fixed before 2022 (light green, equation (8) with landlord-type-by-year fixed effects, tenancies whose landlord's portfolio is observed). Price update: the dwelling rent differs from the previous year within the same contract. Bars are 95% confidence intervals; standard errors clustered by individual. Full estimates, including the probability of a new contract, in Appendix Table F3.

so neither the capped margin nor the free margin is used more often for them[22].

By landlord type, there are no significant differences between the types; and the estimates for small and medium landlords are close to zero on every margin. The only positive estimate is the one on the rent of the dwelling for large landlords discussed above; it does not appear in rent per square meter, it is not accompanied by more frequent updates, and it shrinks further once the contract cycle is held fixed (Appendix Table F4). The framework placed any extraction among the landlords with repricing capacity; we find that this capacity is exercised on all tenants alike. Tenant characteristics that proxy for moving costs do not change the picture: the effect on rent does not differ between recipients with and without children (Appendix Table F10), and the difference by age reflects a lower dwelling rent among young recipients, with no change in updates or new contracts, which we discuss in Appendix E.

Suggestive evidence from renewals. The one margin at which the LAU leaves the rent free is the renewal of a contract in the same dwelling. Appendix Table F9 compares, within the same tenancy, the rent of recipients and non-recipients in years with and without a renewal. In years without a renewal the two do not differ; in a renewal year, recipients who renew pay about 5% more than non-recipients who renew. The premium is similar across age

[22]Without contract-cycle fixed effects, recipients appear more likely to sign a new contract (Appendix Table F8). This reflects their more recent contracts, which come up for renewal more often in 2022 and 2023, not a response to the benefit.

groups and across households with and without children, and it is of the same size whether the renewal coincides with the first year of receipt or with a later one, so it does not reflect contracts formalised in order to apply.

We present this result as suggestive rather than as a test of extraction, for two reasons. First, the sample is selected. We observe the rent of a tenant only while the tenant stays with a landlord in the panel, so a renewal at a new price is observed only for tenants who accepted it and stayed; a tenant faced with a large increase who leaves disappears from our price data. Staying is a choice of the tenant, and the benefit itself can affect it, since it is portable but lost if the new rent exceeds the cap. Within the group of stayers, the premium is therefore consistent with recipients accepting larger increases to avoid moving. That is a form of extraction, but one that operates through the tenant's housing decision rather than through a landlord singling out recipients. Second, few recipients renew a contract in the same dwelling during the benefit period, so the estimate rests on a small number of events and we cannot split it by landlord type. What the result does show is that the price paid by recipients is not fixed where the law leaves it free, and that the margin is open rarely enough for the average effect to remain at zero.

6.4 Other possible channels: Information, segment pricing and market power

We test three further channels through which a landlord could learn about, or price, the benefit; the estimates are in Appendix Tables F5, F6 and F7, and those on local market power in Appendix Table C3.

Within the landlord's portfolio. If a landlord priced recipients differently, the cleanest test compares a recipient with a non-recipient renting from the same landlord in the same year. Among landlords with at least two linked tenants, with individual and portfolio-by-year fixed effects (within landlord comparison), the rent of a recipient is not significantly different from that of the other tenants of the portfolio of the landlord, and neither are rent per square meter or the probability of an update.[23] Landlords do not single out recipients within their portfolios.

The eligible segment. A landlord who cannot identify recipients could still price the segment likely to receive the benefit. We test whether, after 2022, the rents of eligible tenants rose relative to those of non-eligible tenants by landlord type, in the full sample of tenants.

[23]The identifying variation comes from 278 recipient tenant-years with at least one co-tenant of the same landlord.

They did not: the post-2022 differential for the eligible segment is not different across landlord types nor significantly different from 0 for any landlord type. Large landlords, who reprice more often, do not reprice the eligible segment in a different way.

Information in new contracts. Renegotiation requires information: a landlord can only price the benefit if he knows that his tenant receives it. The tenant may disclose it in conversation, but we cannot observe this. The one channel we can test is documentary. The benefit appears in the tenant's tax return the year after receipt, and tax returns are commonly requested when a new contract is signed. If landlords used this information, rents in contracts signed in 2023 would be higher for tenants who received the benefit in 2022. Among eligible tenants signing a new contract in 2023, the rent of tenant who received the housing benefit in 2022 is not significantly different from that of other tenants, and the same holds in the placebo of contracts signed in 2022, when receipt was not yet visible in the 2021 tax return.[24] Visible receipt is not priced into new contracts.

Local market power. A landlord's bargaining position could also depend on how much of the local rented stock the landlord holds. Appendix C defines local rental markets as ZIP codes in urban areas and comarcas in rural areas, validated by the share of residential moves that stay within them, and builds the Herfindahl index and each landlord's share of the observed rented stock in 2021. Concentration is low everywhere: the median market has a Herfindahl index of about 340 on the 0 to 10,000 scale, far below the 1,500 threshold used by competition authorities to define a concentrated market, and the median landlord holds about 1% of the local rented stock.

Interacting the benefit with indicators for an above-median Herfindahl index, for an above-median landlord share of the local rented stock and for an above-median share of eligible tenants in the market, with group-by-year fixed effects, gives differentials of 0.030 (standard error 0.029), −0.002 (0.028) and −0.008 (0.027) in log rent (Appendix Table C3). Concentration does not predict a rent response. Nor does the local density of eligible tenants: the effect does not differ across markets where the program could reach more tenants, so there is no sign of a demand effect even where the program is most present. Where portfolio size predicts repricing capacity, local market share does not predict extraction.

[24]The estimates rest on 439 new contracts in 2023 and 606 in 2022, so the test has limited power and we present it as suggestive.

6.5 Summary

Taking the three conditions of equation (3) in turn: landlords have no formal information channel through which they learn about receipt in a running contract, and we find no sign that they use the one channel available at a new contract; the in-contract margin is capped and is not exercised more often on recipients, not even by the landlords who exercise it most; and the only margin at which the rent is free, renewal, shows a premium for recipients that is infrequent. The price per unit of housing paid by a recipient who stays in the same dwelling does not change, and none of the configurations in which the framework would place extraction shows it. With the price of housing fixed, the benefit is a shift of the recipient's budget constraint, and what recipients do with it depends on the design of the transfer, which we analyse in Section 7.

7 Results II: What Recipients Do with the Transfer

With the price of housing fixed, the benefit shifts the recipient's budget constraint. This section asks whether the housing choices of recipients respond to the design of the transfer, and where the transfer goes when it does not go to housing: other consumption, labour supply and saving.

7.1 Housing consumption

Section 6.1 showed that the rent of recipients who stay in their dwelling does not move. Housing consumption can nonetheless change in two ways without a change in price: by moving to a different dwelling[25], or by sharing the same dwelling with fewer people, which raises housing consumption per person. The two designs of the transfer bear on these margins differently. The proportional rent subsidy lowers the relative price of housing only when a dwelling is chosen, because the amount is set on the rent of the contract in place; a recipient who stays receives the same amount whether or not the household at the dwelling changes, exactly as under the income grant. The substitution effect of Section 3.4 can therefore only show up in mobility behaviors, and the difference in mobility between the two groups of recipients is our proxy for it. Changes in household composition are a housing-consumption response common to both designs, driven by the income effect.

Table 2 reports housing and household outcomes by age group, which separates the two designs: tenants over 35 receive the proportional rent subsidy, tenants aged 35 or under the

[25]We can only examine the extensive margin, since we do not observe the rent of the destination dwelling. Our intensive margin measures will be the characteristics of the dwelling they move into.

income grant.[26] Because the probability of moving rises when a contract reaches its end, we add contract-start-year-by-year fixed effects, so that the moves we attribute to the benefit are not driven by the stage of the contract (Appendix Table D2).

Table 2: Housing and household responses by age group

	Housing		Household				
	(1) Moved	(2) Room rental	(3) Tax filers in dwelling	(4) Fiscal families in dwelling	(5) Residents (census)	(6) Joint filing (pp)	(7) Has spouse (pp)
Over 35 (proportional rent subsidy)	0.035**	0.005	0.008	-0.030	-0.022	0.057	2.033**
	(0.015)	(0.011)	(0.034)	(0.032)	(0.053)	(1.843)	(0.896)
Difference: 35 or under − over 35	-0.092***	-0.023	-0.074	-0.013	-0.020	8.257***	0.792
	(0.031)	(0.017)	(0.050)	(0.050)	(0.093)	(2.929)	(1.487)
35 or under (income grant), total effect	-0.057**	-0.018	-0.066*	-0.043	-0.041	8.314***	2.825**
	(0.028)	(0.014)	(0.038)	(0.039)	(0.077)	(2.309)	(1.199)
Observations	61450	65996	65996	65996	65996	43453	65996

Age group fixed at the last pre-2022 observation. Individual, municipality, province-by-year and age-group-by-year fixed effects. Total effect for the young group by the delta method.

Tax filers and fiscal families: number of PIT returns and of fiscal family units declared at the dwelling. Residents: non-owner residents at the dwelling (census). Standard errors clustered by individual.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Mobility responds in opposite directions, as the two designs predict: the group facing a lower relative price of housing adjusts by moving, the group receiving the income grant does not. Recipients over 35 become about 3.5 percentage points more likely to move in a year, and this effect survives the contract-cycle fixed effects. Recipients aged 35 or under move less than older recipients, a difference that is significant, although their total effect is not significant once we control for contract cycle. The result for older recipients is in line with the substitution effect of Figure 3, $h_{PRS} > h_{IG}$, with the caveats noted above: age also proxies for moving costs and life-cycle stage, and we do not observe the rent of movers. However, when observing at the characteristics of the dwelling the recipients move into we find that they are not larger nor more valuable[27] than those of non-recipients who move (Appendix Table F11).

For recipients of the income grant, the probability of renting a room rather than a whole dwelling falls, but not significantly. Household composition moves in the same direction: the number of tax filers, fiscal families and residents at the dwelling all fall slightly for the young, consistent with less crowding, but none of these changes is significant. What is significant points to household formation: young recipients become 8 points more likely to file a joint tax return (in line with the negative effects on the number of tax filers) and about 3 points more likely to declare a spouse; older recipients also become more likely to declare a spouse, by 2 points. For young recipients, the transfer coincides with settling in the current dwelling and formalizing a partnership rather than with moving. This is the response the figure predicts

[26] Tenants over 35 can only receive the general program. Among tenants aged 35 or under, 44% of annual amounts are exact multiples of €250, against 9% among older recipients, and the amount received does not increase with the rent for the young but does for the old (Appendix D).

[27] Cadastral value.

for the income grant: the transfer is used as income, not as housing.

The housing responses we find are the ones an income transfer would produce: rents of stayers do not move, dwellings do not get larger, and the changes we observe, more moves among proportional subsidy recipients and household formation among the young, are modest. There is no sign that the housing label channels the transfer into housing beyond what the income effect predicts. Most of the transfer must therefore be going elsewhere, which is the question we address in Section 7.2.

7.2 Where the transfer goes?

Table F13 and Figure 7 account for the transfer. Disposable income after rent is gross income, including the benefit, plus other income, minus annual rent, so the columns of the table add up. Recipients receive €1,792 per year on average while in the program. Disposable income after rent rises by €1,473, or 82 cents per euro. Annual rent absorbs €121 (7 cents), labour income falls by €313 (17 cents), and other income, from self-employment, capital and other transfers, rises by €116 (6 cents). The share that does not reach disposable income is thus a labour-income response, not a rent response.

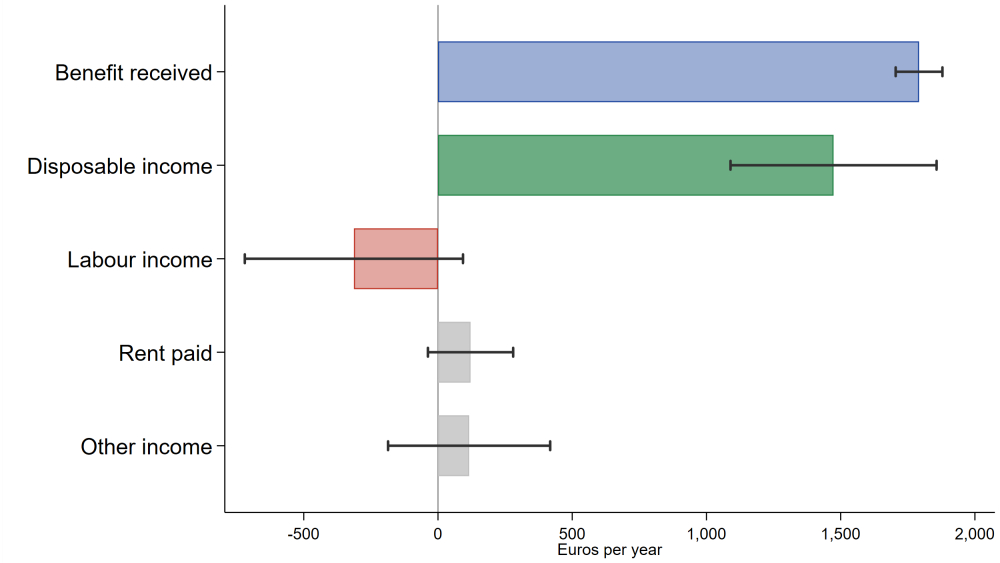


Figure 7: Accounting for the transfer

Notes: Effects of the benefit in euros per year, equation (6): benefit received, labour income, annual rent and disposable income after rent. Bars are 95% confidence intervals; standard errors clustered by individual. Estimates in Table F13.

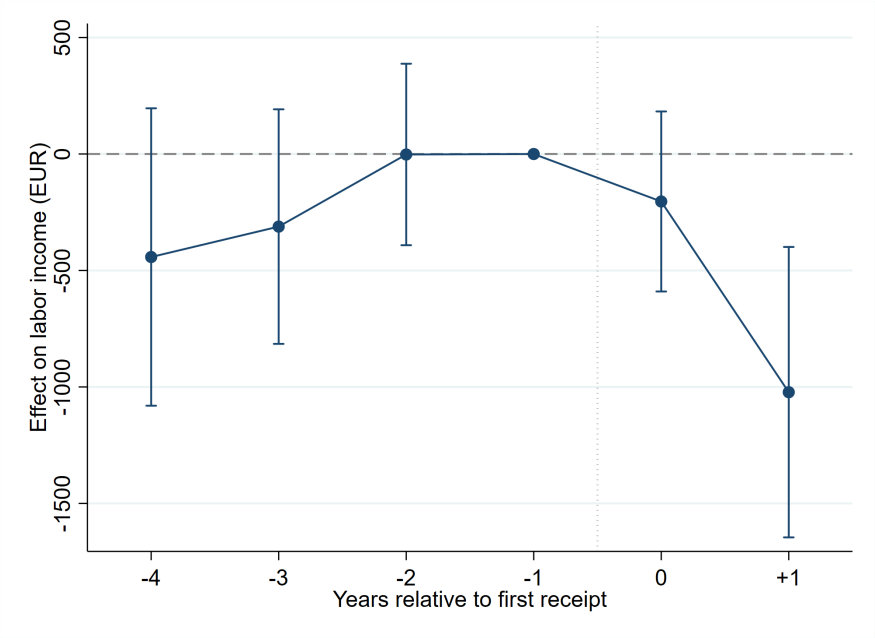


Figure 8: Event study: labour income

Notes: Coefficients $\hat{\beta}_k$ from equation (7) for labour income in euros per year, with $k = -1$ as the reference period, both cohorts pooled. Individual, municipality and province-by-year fixed effects. Bars are 95% confidence intervals; standard errors clustered by individual.

7.2.1 Labour supply

The benefit induces a negative labour-income response, concentrated in the second year of receipt (Figure 8) and among the youngest recipients. Table F14 and Figure 9 split recipients into three age groups. Recipients over 35 show no response on any margin: labour income +€107 (standard error 261), no change in the probability of having labour income, in social security contributions or in unemployment spells. Recipients aged 26 to 35 reduce labour income by €817 (standard error 396), about a third of the housing benefit (€2,249) they receive, with no change in the probability of working or in unemployment. Recipients aged 25 or under are the ones who respond: they reduce labour income by €2,005 (standard error 749), about the €2,098 they receive, so the transfer replaces earnings one for one. The adjustment is on the intensive margin: the probability of having labour income (-0.7 points, standard error 4.0) and unemployment spells do not change, and social security contributions fall by €155, in line with the fall in earnings. For this group, disposable income after rent does not rise (€46, standard error 617). Given their age, a plausible reading is that the youngest recipients use the benefit to buy time, for instance to study, rather than goods.

This is an income effect on the intensive margin: nobody leaves employment or enters unemployment, and the earnings that fall are those of recipients whose jobs are more often part-time or secondary. The transfer does not create an inactivity trap over its two-year

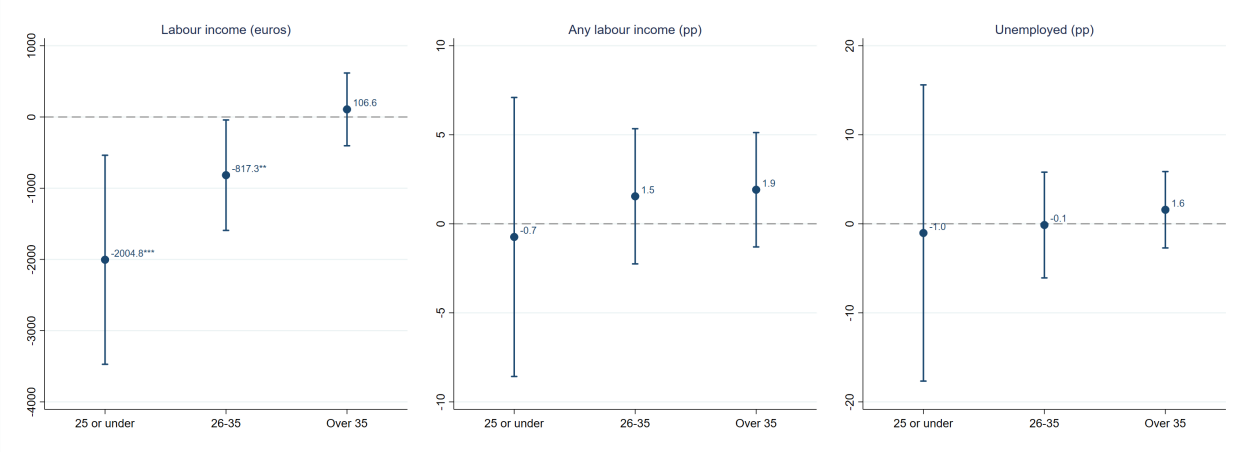


Figure 9: Labour-supply responses by age group

Notes: Effects of the benefit by age group fixed before 2022, equation (8) with age-group-by-year fixed effects. Labour income in euros per year; probability of any labour income and of an unemployment spell in percentage points. Bars are 95% confidence intervals; standard errors clustered by individual. Estimates in Table F14.

horizon. Heterogeneity by children points the same way: the labour response is concentrated among recipients without children (Appendix Table F12).

7.2.2 Saving and other margins

Appendix Table F15 reports saving and investment outcomes. Bank balances at the end of the year change by €49, not significantly, and the confidence interval excludes saving more than about a third of the gain in disposable-income. The probability of declaring any interest income rises by about 2 points, which is consistent with the transfer passing through the recipient's account during the year, but end-of-year balances are unchanged, so there is no accumulation of liquid savings. This supports the idea that a temporary transfer to liquidity-constrained households is spent, not saved.

7.3 Summary

Of each euro of benefit, 82 cents appear as disposable income, 7 cents go to rent and 17 cents are offset by lower earnings, all of them among recipients aged 35 or under. Housing consumption changes through moves for recipients of the proportional rent subsidy and not at all for recipients of the income grant, who settle and formalize their households instead. The allowance works as an income transfer: it finances non-housing consumption and, for the youngest, time. Each design does what the theory assigns to it: the subsidy moves housing, the grant moves the budget.

8 Conclusion

This paper reveals how the design of housing benefits matters to avoid unintended effects as well as for shifting households housing choices. We study Spain's 2022 rental assistance that reached under 2% of renters and was paid to the tenant under two designs: an income grant and a proportional rent subsidy. Comparing recipients with eligible non-recipients rationed out of the program, we first check whether landlords use the bilateral bargaining to raise the rent of recipients, and find that they do not, even in the configurations where landlords were better positioned to do so.

With prices fixed, housing choices respond to the design of the transfer: recipients of the proportional rent subsidy move more, recipients of the income grant do not move and formalize their households. Of each euro, 82 cents appear as disposable income after rent; the rest is offset mainly by lower labor earnings, a response on the intensive margin concentrated among recipients aged 25 or under. Liquid savings do not increase and nobody leaves employment.

The broader lesson is about what a housing allowance does when it cannot change prices. Under frictions to repricing and at a scale that does not move markets, the benefit is not captured by landlords and reaches the tenant. What it does then depends on its design. A government that wants low-income households to live in more or better housing has to lower the relative price of housing, and even then moving costs limit the response. A government that wants to lower their rent burden obtains more welfare per euro from the grant. But the grant is an income transfer like any other. Its housing label serves only to target it on tenants, whose tenure status proxies for holding no housing wealth and for low income.

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A Matched Comparison Group

Procedure. To address selection into application (Section 2.3), we build a comparison group of eligible non-recipients who resemble recipients on pre-treatment observables. Matching is performed within exact cells defined at the province level (48 cells), which balances local housing market conditions against cell sizes large enough to find matches. Within each cell, a recipient i is matched to the eligible non-recipient(s) j minimising the Mahalanobis distance (Rubin, 1980)

$$d(i,j) = \sqrt{(X_i - X_j)' S^{-1} (X_i - X_j)}, \tag{9}$$

where X is the vector of pre-treatment characteristics and S its sample covariance matrix, so that covariates measured on different scales contribute in proportion to their dispersion and correlation. X includes individual and household income, the cadastral value, surface and year of construction of the dwelling, the tenant's age, the number of filers at the dwelling, a room-rental indicator, family size, the number of children, and the contract start year, entered as a continuous variable with a separate dummy for the 44% of observations with missing contract information.[28] We implement 1:3 matching, so each treated tenant will be matched with their three closest eligible non-receivers tenants.

Estimation. On the matched samples we estimate

$$Y_{it} = \alpha_i + \lambda_{g(i),t} + \beta\, D_{it} + \varepsilon_{it}, \tag{10}$$

where $\lambda_{g(i),t}$ are matched-group-by-year fixed effects and $g(i)$ is the matched group of tenant i. These fixed effects compare each recipient only with its own matches in every year. Because every matched group contains one recipient and never-treated controls, the estimates never use already-treated tenants as controls for later-treated ones, the source of bias of two-way fixed effects estimators under staggered adoption (**?**, **?**). Inference uses a wild cluster bootstrap with 500 replications, clustered at the matched-group level.

Results. Table A1 reports the main outcomes on the 1:3 matched sample. The results are in line with the baseline estimates on the full sample of eligible tenants. The effect on rents remains small and statistically indistinguishable from zero, both for the total rent (-0.012, s.e. 0.009) and for rent per square meter (-0.015, s.e. 0.011), while disposable income rises by about 1,600 euros, close to the baseline estimate. The reduction in labour income is smaller in magnitude and not statistically significant. The only difference with respect to the baseline

[28] The distance to the rent cap is excluded because it is a direct function of the outcome.

is residential mobility: recipients in the matched sample are 4.4 percentage points more likely to move, whereas the full-sample estimate is close to zero. We do not read much into this difference. Moving is a rare event, and with matched-group-by-year fixed effects the estimate is identified from within-group comparisons: when a recipient moves, it is likely that none of the matched controls in the same group moves that year, so the coefficient is driven by a small number of moves rather than by a systematic difference in mobility[29].

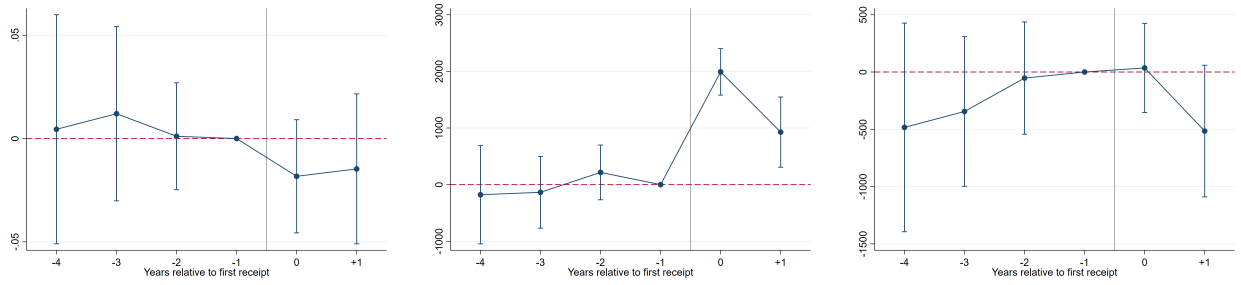
Table A1: Baseline outcomes, 1:3 matched sample

	(1) Log rent	(2) Log rent/m^2	(3) Disp. income	(4) Labour inc.	(5) Moved
Housing benefit	-0.012	-0.015	1594.000***	-81.618	0.044***
	(0.009)	(0.011)	(236.256)	(236.075)	(0.015)
Observations	15,338	15,332	15,338	15,338	14,277

Individual, municipality and matched-group-by-year fixed effects.

Cluster bootstrap standard errors (500 replications), clustered at the matched-group level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



(a) Log rent (b) Disposable income after rent (c) Labour income

Figure A1: Event studies on the 1:3 matched sample

Notes: 1:3 matched sample, Mahalanobis distance within province on pre-treatment covariates. Coefficients $\hat{\beta}_k$ with $k = -1$ as reference period, both cohorts pooled. Individual, municipality and matched-group-by-year fixed effects. Bars are 95% confidence intervals; cluster bootstrap by matched group.

[29]As in the tenant heterogeneity results, one would ideally absorb the contract cycle through year-of-entry-by-year fixed effects, since recipients could hold more recent contracts and they would be therefore more likely to be at a renewal date in 2022–23. With individual, municipality and matched-group-by-year fixed effects already in place, however, adding a further set of roughly 160 dummies would leave very little variation to identify the effect in a sample of about 15,000 observations and a few hundred moves, so we do not pursue it here.

B Robustness of the Baseline

Fixed effects. Tables B1 and B2 re-estimate the baseline under seven geographic-by-time structures, from region-by-year to municipality-by-year (and individual FE). The effect on log rent ranges from 0.4% to 0.9%, with a standard error of 0.9 to 1.0% in every column. The effect on disposable income ranges from €1,235 to €1,555, within one standard error of the baseline €1,473 in all cases; the lower values come from specifications with additive year effects, which do not absorb province-level income trends.

Table B1: Robustness to the fixed-effects structure: log rent

	(1) Region × Year	(2) Prov. + Year	(3) Prov. × Year	(4) Mun. + Year	(5) Mun. × Year	(6) Mun. + Region × Year	(7) Mun. + Prov. × Year
Housing benefit	0.008	0.007	0.009	0.007	0.004	0.007	0.008
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)
Observations	69090	69090	69086	68992	66078	68992	68988
Adj. R^2	0.835	0.834	0.835	0.834	0.837	0.834	0.835

Individual fixed effects in all columns; column headers give the geographic-by-time fixed effects. No controls. Standard errors clustered by individual.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Robustness to the fixed-effects structure: disposable income

	(1) Region × Year	(2) Prov. + Year	(3) Prov. × Year	(4) Mun. + Year	(5) Mun. × Year	(6) Mun. + Region × Year	(7) Mun. + Prov. × Year
Housing benefit	1473.831***	1258.497***	1490.228***	1235.274***	1555.355***	1444.249***	1473.352***
	(194.046)	(191.945)	(193.961)	(193.710)	(205.733)	(195.699)	(195.826)
Observations	69090	69090	69086	68992	66078	68992	68988
Adj. R^2	0.632	0.631	0.632	0.627	0.630	0.628	0.629

Individual fixed effects in all columns; column headers give the geographic-by-time fixed effects. No controls. Standard errors clustered by individual.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Controls. Table B3 adds, in turn, contemporaneous controls (children, room rental, log cadastral value and log surface) and pre-2022 covariates interacted with year (log rent, log income and age group, plus room rental). The effect on log rent is 0.8%, 0.8% and 1.1%; on disposable income €1,473, €1,324 and €1,544.

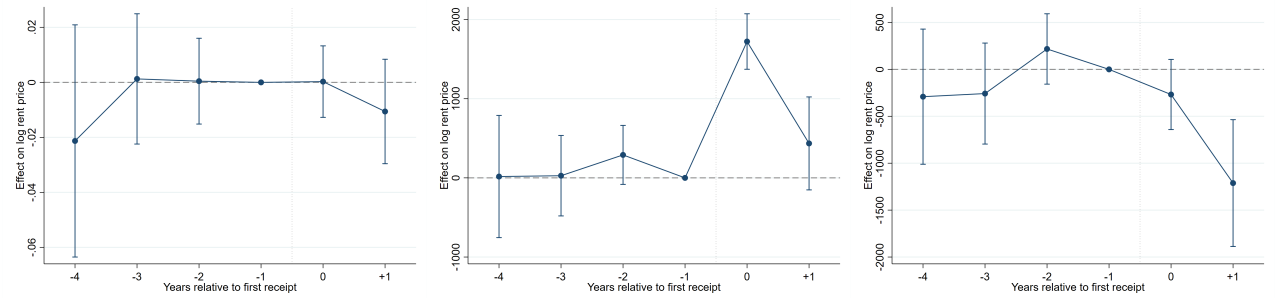
Table B3: Robustness to the control set

	Log rent			Disposable income (€)		
	(1) Baseline	(2) Contemp.	(3) Baseline×Year	(4) Baseline	(5) Contemp.	(6) Baseline×Year
Housing benefit	0.008	0.008	0.011	1473.352***	1323.688***	1544.270***
	(0.009)	(0.009)	(0.010)	(195.826)	(193.268)	(207.214)
Observations	68988	67424	57581	68988	67424	57581

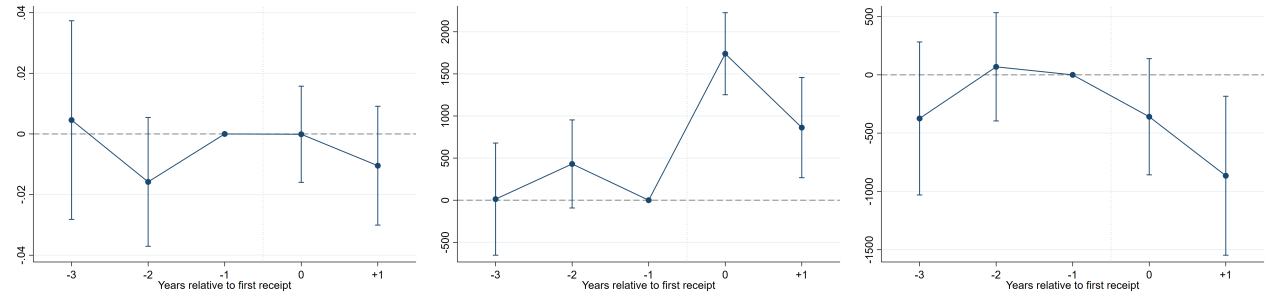
Individual, municipality and province-by-year fixed effects in all columns. Contemp.: children, room rental, log cadastral value and log surface measured in year t. Baseline×Year: pre-2022 log rent, log income and age group interacted with year dummies, plus room rental. Standard errors clustered by individual.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Heterogeneity-robust event studies. Figure B1 re-estimates the event study for log rent, disposable income and labour income with the estimator of Callaway and Sant'Anna (2021) and with that of de Chaisemartin and D'Haultfœuille (2020). The former compares each cohort with eligible tenants not yet treated, uses a common pre-treatment reference period, and conditions on covariates fixed at their last pre-2022 values; it cannot accommodate the municipality fixed effects of the baseline. The latter allows treatment to switch off, as it does when a two-year entitlement ends, estimates effects from tenants who switch into treatment relative to tenants with the same past treatment, restricts the sample to switchers observed in both post periods, and allows for province-specific trends; it admits two placebo periods. For log rent both estimators give 0.0% in the first year of receipt and −1.1% in the second, with pre-treatment coefficients within one standard error of zero. For disposable and labour income they reproduce the baseline dynamics.



(a) Log rent, Callaway-Sant'Anna (b) Disposable income, Callaway-Sant'Anna (c) Labour income, Callaway-Sant'Anna



(d) Log rent, de Chaisemartin-D'Haultfœuille (e) Disposable income, de Chaisemartin-D'Haultfœuille (f) Labour income, de Chaisemartin-D'Haultfœuille

Figure B1: Heterogeneity-robust event studies

Notes: Top row: Callaway and Sant'Anna (2021) estimator, not-yet-treated eligible tenants as controls, doubly robust inverse probability weighting, covariates fixed at the last pre-2022 observation, universal base period $k = -1$. Bottom row: de Chaisemartin and D'Haultfœuille (2020) estimator for non-absorbing treatment, switchers observed in both post periods, province-specific trends; $k = -2$ and $k = -3$ are placebos. Bars are 95% confidence intervals.

C Local Rental Markets and Market Power

Market definition. We define local rental markets by combining ZIP codes and counties ("comarcas", is a territory that groups several municipalities or towns together because they share similar physical, cultural, historical, or economic characteristics.) according to the urban or rural character of the area: in municipalities belonging to a urban area, the market is the ZIP code; elsewhere (rural areas), it is the comarca. The unit is meant to be large enough to contain several landlords and small enough to represent the choice set of a tenant looking for a dwelling. Figure C1 maps the resulting markets.

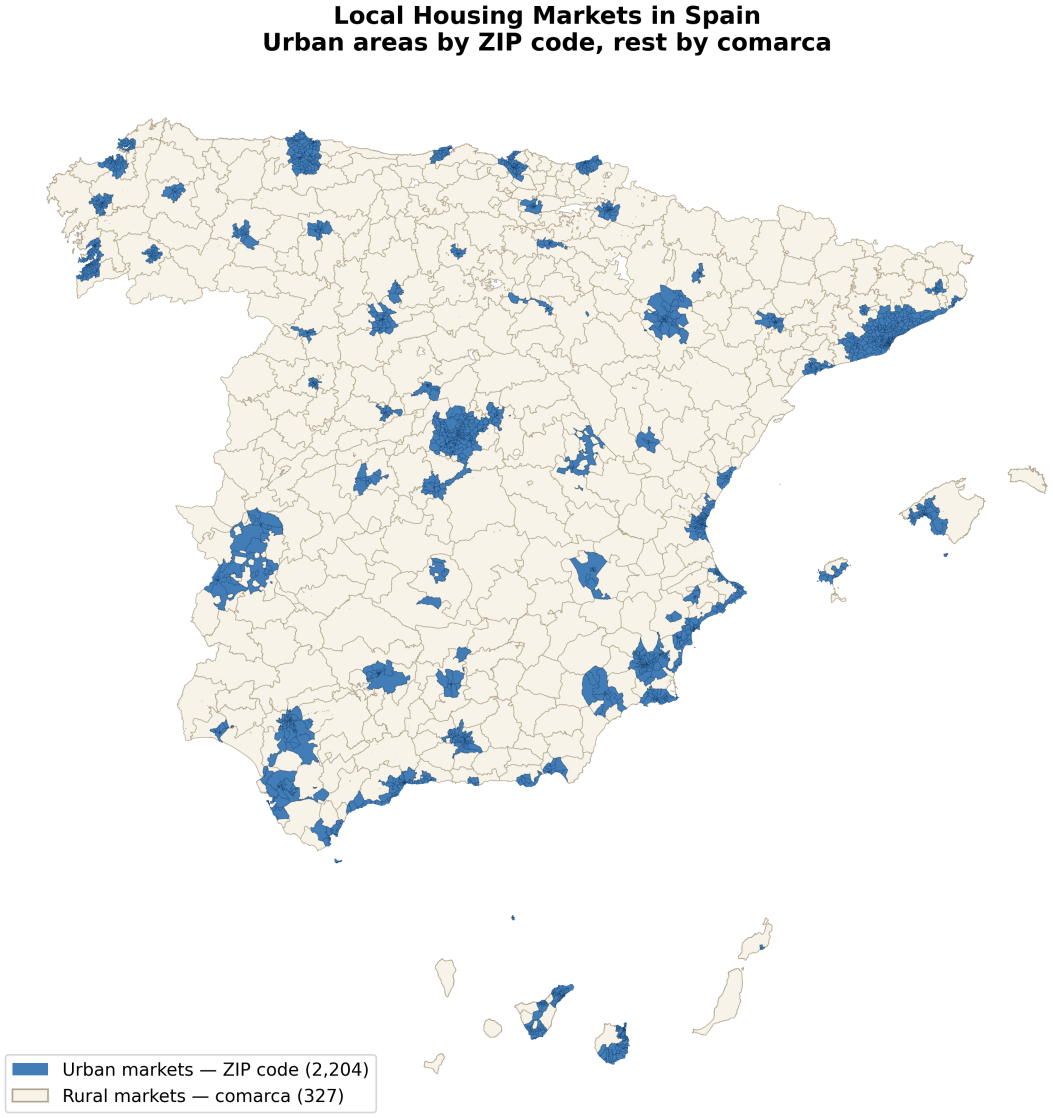


Figure C1: Local rental markets

Notes: Local rental markets used in the concentration measures: ZIP codes in municipalities belonging to a named urban area, comarcas elsewhere.

Validation: renters relocate locally. Table C1 reports the share of observed residential moves by renters whose origin and destination fall in the same geographic unit. Over 93% of moves stay within the province and 72% within the municipality, so the relevant choice set is local. Our markets are finer and retain 46% of moves, in line with Ramoutar, 2025 where she finds 50% of move occur within one mile.

Table C1: Geographic scope of renter relocations

Share of moves staying within	Mean	Obs.
Local market	0.463	38,191
ZIP code	0.436	38,191
Municipality	0.716	38,191
Comarca	0.741	38,191
Province	0.933	38,191

Residential moves by renters, 2016–2023. Each row reports the share of moves whose origin and destination fall in the same unit.

Concentration. We measure concentration with the Herfindahl–Hirschman Index. A market M (a ZIP code in urban areas, a *comarca* in rural areas) contains a set of landlord portfolios $\mathcal{L}(M)$, each portfolio L being the set of rented dwellings with a common owner. The share of landlord L in market M is s_{LM}, the number of rented dwellings in L divided by the number of rented dwellings observed in M, and

$$\text{HHI}_M = 10{,}000 \times \sum_{L \in \mathcal{L}(M)} s_{LM}^2, \tag{11}$$

so that 10,000 corresponds to a single landlord owning the whole rented stock and values close to zero to many small independent owners. We use the 2021 cross-section and exclude markets with fewer than ten rented dwellings observed. Table C2 shows that urban and rural markets are comparable in size, with a median of 108 and 107 rented units observed, and that both are unconcentrated: the median Herfindahl index is about 340 on the 0 to 10,000 scale, below 1,300 even at the 90th percentile and far below the 1,500 threshold used by competition authorities to consider a concentrated market (2,500 very high concentration), and the average landlord holds 3% of the local rented stock. Pooling both definitions gives 1,3 92 markets for our sample with a median HHI of 340 and a median of 108 rented units.

Spatial distribution. Figure C2 maps the local markets along four dimensions. Panel (a) shows concentration of the rented stock, the measure used in Table C2. Panel (b) extends the index to all residential properties, rented or not, in markets with at least five properties

Table C2: Concentration in local rental markets, 2021

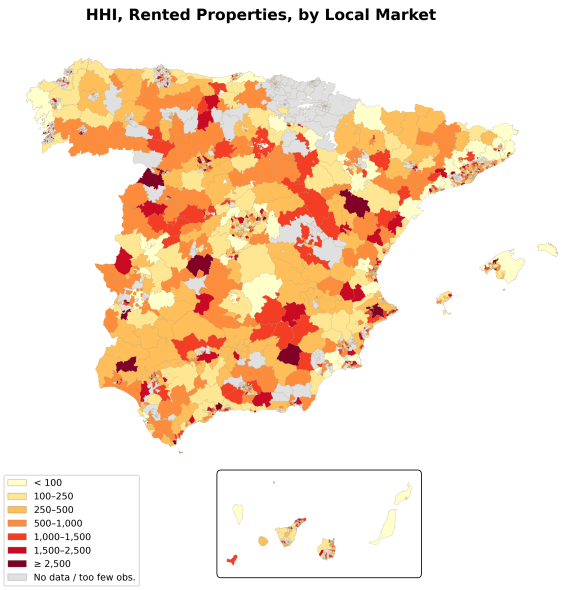
	p25	p50	p75	p90	Mean	N
Panel A: Urban markets (ZIP code)						
Herfindahl index (0–10,000)	200.8	343.9	662.5	1,276.3	579.6	1,170
Landlord share of rented stock	0.002	0.009	0.032	0.085	0.033	1,170
Rented dwellings observed	51	108	214	389	173.0	1,170
Panel B: Rural markets (comarca)						
Herfindahl index (0–10,000)	165.2	308.7	640.8	1,190.9	505.7	221
Landlord share of rented stock	0.002	0.010	0.036	0.075	0.030	221
Rented dwellings observed	46	107	249	455	205.6	221

Market definitions for 2021. Markets with fewer than ten rented dwellings are excluded.

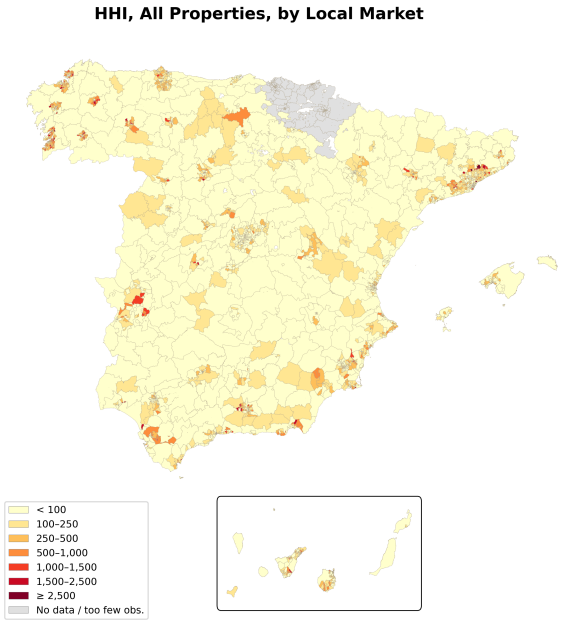
Herfindahl index computed over landlord shares of the observed rented stock.

observed; since every owner-occupied dwelling is a one-unit portfolio, this index describes the concentration of property ownership rather than of the rental market, and it is informative about the potential supply of rented dwellings a landlord competes with. Concentration is not geographically clustered under either definition: high and low values alternate within provinces, and the largest metropolitan areas, Madrid, Barcelona and Valencia, have among the lowest values, because many small landlords share each ZIP code. Panels (c) and (d) show the share of tenants in each market who are eligible for benefit and who receive it. Eligibility is widespread, above half of tenants in many markets outside the large cities, where higher rents push tenants above the cap. Receipt is sparse: in most markets fewer than 2% of tenants receive the benefit, and it exceeds 10% only in a small number of markets with few observed tenants. The program is therefore thin everywhere, including in the markets where it is most present, which is what equation (1) requires.

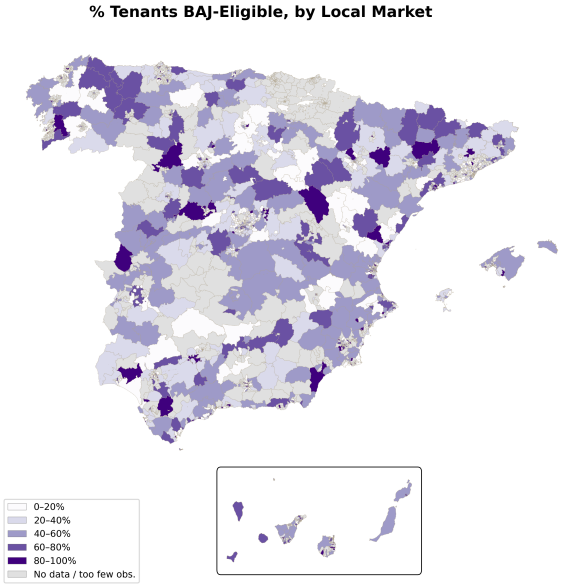
Market power and the benefit. Table C3 interacts the benefit with pre-2022 measures of local market power: an indicator for markets with a Herfindahl index above the median, indicators for markets above the 1,500 and 2,500 thresholds used by competition authorities, an indicator for landlords whose share of the local rented stock is above the median, and, as a measure of the program's local presence, an indicator for markets where the share of eligible tenants is above the median. Each specification includes individual, municipality, province-by-year and group-by-year fixed effects, so that the interaction is identified from the differential response of recipients in high-concentration markets relative to recipients elsewhere, net of any post-2022 trend specific to those markets; standard errors are clustered at the market level. None of the differentials is significant. Recipients in markets with an



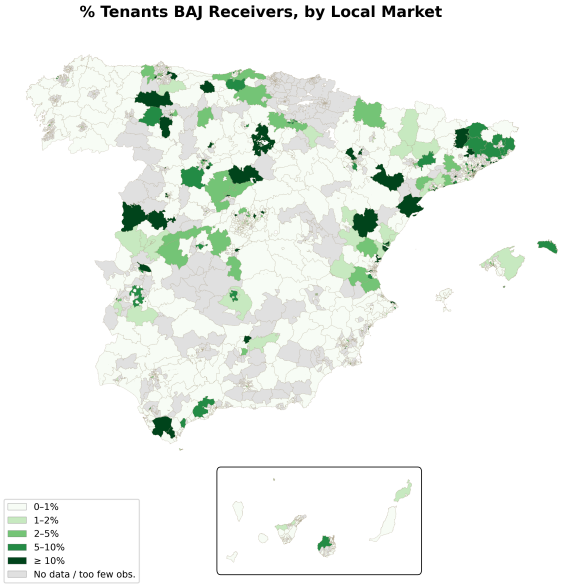
(a) Herfindahl index, rented dwellings



(b) Herfindahl index, all properties



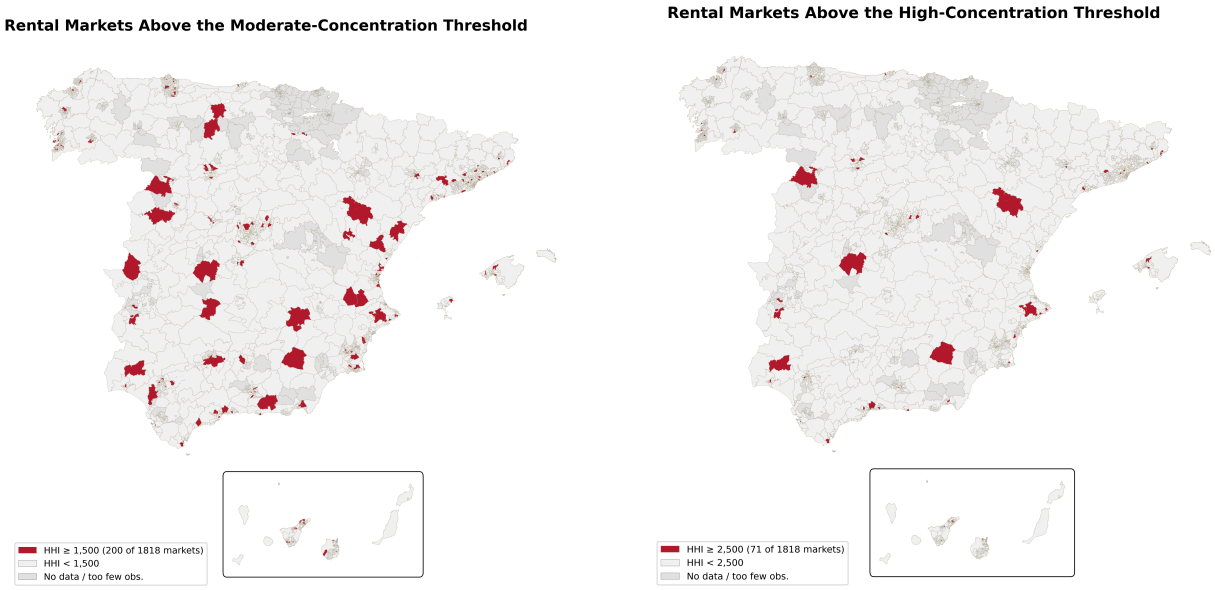
(c) Share of eligible tenants (%)



(d) Share of recipients (%)

Figure C2: Concentration, eligibility and receipt across local rental markets, 2022

Notes: Local markets as defined in this appendix. Panel (a): Herfindahl index over landlord shares of the rented dwellings observed in the market, on a 0–1 scale (multiply by 10,000 for the scale of Table C2); markets with fewer than ten rented dwellings observed in grey. Panel (b): Herfindahl index over owner shares of all residential properties observed in the market; markets with fewer than five properties observed in grey. Panel (c): tenants in the panel who meet the income and rent conditions of the young-track benefit, as a share of all tenants in the market. Panel (d): tenants receiving the benefit, as a share of all tenants in the market. Colour scales are capped at the 95th percentile.



(a) Herfindahl index above 1,500

(b) Herfindahl index above 2,500

Figure C3: Concentrated local rental markets, 2021

Notes: Local rental markets whose Herfindahl index over landlord shares of the observed rented stock exceeds the thresholds used by competition authorities to define a concentrated market (1,500) and a highly concentrated market (2,500). Markets with fewer than ten rented dwellings observed in grey.

above-median Herfindahl index pay 3.0% more than other recipients (standard error 2.9), and the differential is of the same size above the 1,500 threshold (3.4%, standard error 5.2) and smaller above 2,500 (1.7%, standard error 8.0), where few markets remain. The differential for landlords with an above-median share of the local stock is zero (−0.2%, standard error 2.8), and so is the one for markets with a high share of eligible tenants (−0.8%, standard error 2.7). The confidence intervals are wide, since concentration is low almost everywhere (Table C2), but the point estimates give no indication that landlords in more concentrated markets, or with a larger share of them, extract more of the benefit.

Table C3: Local market power and rent extraction

	(1) HHI median	(2) HHI>1,500	(3) HHI>2,500	(4) Tenant exp.	Sh
Receives benefit in year t	0.00219	0.0167	0.0171	0.0216	
	(0.0185)	(0.0137)	(0.0135)	(0.0173)	
Receipt × High HHI	0.0297				
	(0.0286)				
Receipt × HHI > 1,500		0.0341			
		(0.0524)			
Receipt × HHI > 2,500			0.0173		
			(0.0801)		
Receipt × High tenant exposure				-0.00833	
				(0.0271)	
Receipt × High share					
Observations	58196	58196	58196	58159	
Clusters	1492	1492	1492	1482	

Dependent variable: log rent. Pre-2022 local market power: HHI above the median, above 1,500 and above 2,500; share of eligible tenants in the landlord share of the local rented stock above the median. Individual, municipality, province-by-year and group-by-year fixed effects. Standard e
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D The Two Programs in the Data

The data record benefit receipt and amount but not the program. Age separates the programs only partially: tenants over 35 can receive only the general program, while tenants aged 35 or under can receive either. Three features of the amounts received, among recipient-years with a positive benefit, indicate that the age split tracks the two designs (Table D1).

First, the income grant pays a fixed €250, so annual amounts should be exact multiples of 250. Among recipients aged 35 or under, 44% of annual amounts are exact multiples of 250; among recipients over 35, 9%. Second, the ratio of the annual benefit to the annual rent has a median of 0.42 among the young, as expected for a fixed amount paid against rents below the cap, and of 0.33 among the old, with most of the mass between 0.3 and 0.4, as expected for a share of the rent reduced by partial years and by the cap. Third, a regression of the annual amount on the monthly rent gives a slope of −0.20 (standard error 0.25) among the young and of +0.27 (standard error 0.11) among the old: the amount received by older recipients increases with the rent, the amount received by younger ones does not.

The proportional rent subsidy does not, however, make the marginal price of housing lower throughout the benefit period. Among recipients over 35 observed in a full year of receipt, the amount does not vary with the rent paid in that year (slope −0.21, standard

error 0.45; 134 observations), consistent with an amount fixed at award on the rent of the contract then in force. The difference between the programs is therefore in how the amount is set when the benefit is granted, or re-granted after a move, which is the margin at which the lower relative price can affect the choice of dwelling (Section 3.4).

Table D1: Amounts received by age group

	35 or under	Over 35
Share of annual amounts that are exact multiples of €250	0.44	0.09
Median ratio of annual benefit to annual rent	0.42	0.33
Slope of annual amount on monthly rent	−0.20 (0.25)	0.27 (0.11)

Recipient-years with a positive benefit; age in the year.

Standard errors in parentheses.

Table D2: Housing and household responses by age group: robustness to the contract cycle

	Moved		Tax filers in dwelling		Joint filing (pp)		Residents (census)	
	(1) Baseline	(2) + Cycle	(3) Baseline	(4) + Cycle	(5) Baseline	(6) + Cycle	(7) Baseline	(8) + Cycle
Over 35	0.035**	0.031**	0.008	0.024	0.057	-0.317	-0.022	0.002
	(0.015)	(0.015)	(0.034)	(0.036)	(1.843)	(1.935)	(0.053)	(0.056)
Difference	-0.092***	-0.052*	-0.074	-0.081	8.257***	7.767**	-0.020	-0.050
	(0.031)	(0.029)	(0.050)	(0.055)	(2.929)	(3.216)	(0.093)	(0.105)
35 or under, total	-0.057**	-0.021	-0.066*	-0.057	8.314***	7.450***	-0.041	-0.048
	(0.028)	(0.025)	(0.038)	(0.042)	(2.309)	(2.605)	(0.077)	(0.089)
Observations	61450	51365	65996	55092	43453	36363	65996	55092

columns add contract-start-year-by-year fixed effects and are restricted to tenancies with a non-missing contract start year.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Dwelling Rents of Young Recipients

Table E1 reports the price margins of Section 6.3 by age group, with contract-cycle fixed effects. The rent of the dwelling rises by 2.1% for recipients over 35 (standard error 1.3) and falls by 2.3% for recipients aged 35 or under (1.4); the difference is −4.4 points (1.9). Rent per square meter moves in the same way. No margin accounts for the decline among the young: the probability of an update (−2.0 points, standard error 3.8) and of a new contract (−0.4, 3.3) do not change, and the renewal premium is the same as for older recipients (6.0 versus 5.3 points).

Table E1: Price margins by age group

	(1) Log rent	(2) Log rent/m^2	(3) Price update	(4) New contract	(5) Log rent at renewal
Over 35	0.021	0.015	-0.025	0.013	0.053**
	(0.013)	(0.015)	(0.024)	(0.016)	(0.022)
Difference	-0.044**	-0.050**	0.005	-0.016	0.007
	(0.019)	(0.023)	(0.044)	(0.036)	(0.038)
3 or under, total effect	-0.023	-0.035*	-0.020	-0.004	0.060*
	(0.014)	(0.018)	(0.038)	(0.033)	(0.031)
Observations	55092	54917	41119	43805	43458

Notes: Group fixed at the last pre-2022 observation. Individual, municipality, province-by-year, contract-start-year-by-year and group-by-year fixed effects; tenancies with a non-missing contract start year. Column (5): tenant-by-dwelling fixed effects; rent jump in the renewal year relative to a non-recipient who also renews. Total effect by the delta method. Standard errors clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F Additional Tables

Table F1: Sample characteristics: recipients vs. eligible non-recipients

	Control	Treated	p-value
Small LL (%)	32.26	33.84	0.038
Med-small LL (%)	40.29	39.52	0.322
Large LL (%)	27.44	26.64	0.260
Rent price (€/month)	511.46	556.74	0.000
Rent price (€/m^2)	6.76	6.32	0.000
Cadastral value	61,075.43	61,341.69	0.872
Surface(m^2)	93.62	109.16	0.000
Room rent	0.09	0.09	0.634
Observations	63,029	4,155	
Unique tenant-dwelling pairs	19,004	1,086	

Notes: Sample used in the heterogeneity regressions (eligible tenants; treated are ever-receivers). Landlord rows report the share of observations by landlord type. Means by group; p-value from a test of equality of means with heteroskedasticity-robust standard errors.

Table F2: Repricing frequency by landlord type, all tenancies

	(1) Any price change	(2) Within-contract change	(3) Contract renewal
Small landlord (1 unit)	0.427***	0.392***	0.078***
	(0.005)	(0.006)	(0.002)
Medium landlord (2–5 units)	0.504***	0.471***	0.086***
	(0.005)	(0.005)	(0.002)
Large landlord (6–50 units)	0.601***	0.570***	0.095***
	(0.007)	(0.008)	(0.002)
Differences			
Δ Medium − Small	0.077***	0.078***	0.008***
	(0.008)	(0.009)	(0.003)
Δ Large − Small	0.174***	0.178***	0.017***
	(0.011)	(0.011)	(0.004)
Observations	95448	87106	95448

Notes: All tenancies 2016–2023, not conditional on eligibility or receipt. Annual probability that the dwelling rent changes: any change, change within the same contract, contract renewal. Levels from predictive margins; differences are regression coefficients. Controls: cadastral value and surface (centred). Municipality-by-year fixed effects. Standard errors clustered by municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F3: Price margins by landlord type: full specification

	(1) Log rent	(2) Log rent/m^2	(3) Price update	(4) New contract
Housing benefit (small landlord)	0.008	0.001	-0.004	0.044
	(0.019)	(0.025)	(0.031)	(0.028)
Housing benefit × Medium	-0.005	-0.008	-0.035	-0.057
	(0.025)	(0.029)	(0.042)	(0.036)
Housing benefit × Large	0.019	0.016	-0.008	-0.037
	(0.025)	(0.030)	(0.053)	(0.039)
Group-specific effects				
Medium landlord (2–5 units)	0.003	-0.008	-0.039	-0.013
	(0.016)	(0.015)	(0.028)	(0.023)
Large landlord (6–50 units)	0.027*	0.016	-0.013	0.007
	(0.016)	(0.017)	(0.043)	(0.028)
p-value: equal effects	0.561	0.578	0.697	0.286
Observations	57423	57232	42979	41802

Notes: Small landlord is the reference category; Medium and Large rows are triple differences relative to it. Landlord-type-by-year fixed effects. Column (4): contract-start-year-by-year fixed effects. Standard errors clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F4: Price margins by landlord type: robustness to the contract cycle

	Log rent	Log rent/m^2	Price update	New contract
All landlords	0.007	0.000	−0.023	0.007
	(0.010)	(0.012)	(0.020)	(0.015)
By landlord type				
Small landlord (1 unit)	0.009	0.002	0.002	0.044
	(0.021)	(0.028)	(0.036)	(0.028)
Medium landlord (2–5 units)	0.006	−0.004	−0.050*	−0.013
	(0.017)	(0.016)	(0.030)	(0.023)
Large landlord (6–50 units)	0.017	0.005	−0.014	0.007
	(0.016)	(0.016)	(0.045)	(0.028)
Observations	52,481	52,311	39,260	41,802

Notes: Same specifications as Table F3, adding contract-start-year-by-year fixed effects; tenancies with a non-missing contract start year. All landlords: full estimation sample. By type: landlord's pre-2022 portfolio observed; total effects by the delta method. Standard errors clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F5: Within-landlord comparison: recipients versus other tenants of the same landlord

	(1) Log rent	(2) Log rent/m^2	(3) Price update
Housing benefit	0.024	-0.012	0.051
	(0.029)	(0.022)	(0.059)
Treated tenant-years	278	277	217
Observations	35177	35071	22655

Sample: landlords with at least two linked tenants in the same year. Individual and landlord-by-year fixed effects. Standard errors clustered by individual.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F6: Segment pricing: eligible versus non-eligible tenants after 2022

	(1) Log rent	(2) Log rent/m^2
Housing benefit (recipient)	-0.004	-0.011
	(0.010)	(0.011)
Post × Eligible, small landlord	0.005	0.005
	(0.005)	(0.005)
Post × Eligible, medium landlord	-0.005	-0.002
	(0.005)	(0.005)
Post × Eligible, large landlord	0.006	0.007
	(0.007)	(0.007)
Observations	109982	109705

Sample: all tenants, eligible and non-eligible. Eligibility defined by income at the last pre-2022 observation. Individual, municipality, province-by-year and landlord-type-by-year fixed effects.
Post × Eligible rows: post-2022 change in rents of eligible tenants relative to non-eligible tenants of the same landlord type (delta-method SE). Standard errors clustered by individual.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F7: Information: rent in new contracts

	2023 contracts		2022 contracts (placebo)	
	(1) Log rent	(2) Rent (€)	(3) Log rent	(4) Rent (€)
Receiver_{t-1}	0.037	27.455		
	(0.060)	(28.527)		
Receiver_t			-0.035	-14.433
			(0.080)	(41.117)
Observations	439	439	606	606
Adj. R^2	0.414	0.480	0.416	0.413

Sample: new contracts signed in the year, eligible tenants. Columns (1)–(2): contracts signed in 2023; Receiver_{t-1} equals one if the tenant received the benefit in 2022, visible in the tax return the landlord can request. Columns (3)–(4): contracts signed in 2022 (placebo); Receiver_t equals one if the tenant receives the benefit in 2022, not yet visible. Controls: age, cadastral value, room rental, surface. Municipality fixed effects. Robust standard errors.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F8: Robustness to the contract cycle

	Log rent			New contract	
	(1) Baseline	(2) + Cycle	(3) Same dwelling	(4) Baseline	(5) + Cycle
Housing benefit	0.009	0.007	-0.011	0.029*	0.007
	(0.010)	(0.010)	(0.009)	(0.016)	(0.015)
Observations	55183	55092	34869	43884	43805

Sample restricted to tenancies with a non-missing contract start year. Columns (2), (3) and (5) add contract-start-year-by-year fixed effects. Column (3): tenant-by-dwelling fixed effects, excluding years with a new contract. Standard errors clustered by individual.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F9: The renewal premium: robustness

	(1) No cycle FE	(2) + Contract cycle	(3) No imputed rents	(4) Whole dwellings	(5) Timing
Housing benefit	-0.009	-0.012	-0.013	-0.008	-0.012
	(0.008)	(0.009)	(0.011)	(0.009)	(0.009)
Renewal year	0.002	0.009**	0.008*	0.010**	0.009**
	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)
Housing benefit × renewal year	0.059***	0.054***	0.051***	0.069***	
	(0.018)	(0.018)	(0.019)	(0.020)	
Housing benefit × renewal in first year of receipt					0.052***
					(0.019)
Housing benefit × renewal in later years					0.060**
					(0.030)
Observations	49878	43458	33868	39842	43458

Dependent variable: log rent. Tenant-by-dwelling, municipality and province-by-year fixed effects; columns (2)–(5) add contract-start-year-by-year fixed effects. Column (3) drops imputed rents; column (4) tenants renting a whole dwelling at baseline. Column (5) splits the renewal year by whether it coincides with the first year of receipt. Standard errors clustered by individual.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F10: Price margins by presence of children

	(1) Log rent	(2) Log rent/m^2	(3) Price update	(4) New contract	(5) Log rent at renewal
No children	-0.002	-0.008	0.019	-0.003	0.059*
	(0.012)	(0.012)	(0.032)	(0.024)	(0.031)
Difference	0.015	0.013	-0.068*	0.015	-0.008
	(0.019)	(0.021)	(0.041)	(0.030)	(0.038)
With children, total effect	0.013	0.005	-0.049*	0.013	0.051**
	(0.015)	(0.017)	(0.025)	(0.019)	(0.022)
Observations	55092	54917	41119	43805	43458

Group fixed at the last pre-2022 observation. Individual, municipality, province-by-year, contract-start-year-by-year and group-by-year fixed effects; tenancies with a non-missing contract start year. Column (5): tenant-by-dwelling fixed effects; rent jump in the renewal year relative to a non-recipient who also renews. Total effect by the delta method. Standard errors clustered by individual.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F11: Dwellings of recipients and of movers: surface and cadastral value

	Log surface	Log cadastral value
Panel A: all recipients		
Housing benefit	−0.000	0.010
	(0.005)	(0.007)
Panel B: conditional on moving since 2022		
Moved, non-recipients	0.001	−0.011
	(0.006)	(0.008)
Recipient × moved	0.017	0.060
	(0.034)	(0.045)
Observations	67,427	68,808

Notes: Panel A: baseline specification. Panel B: moved equals one if the tenant has moved at least once since 2022; moving is itself an outcome, so the panel is descriptive. Individual, municipality and province-by-year fixed effects. Standard errors clustered by individual.

Table F12: Heterogeneity by presence of children

	(1) Benefit (€)	(2) Disp. income (€)	(3) Labour income (€)	(4) Moved	(5) Tax filers in dwelling
No children	1950.07***	1323.13***	-850.79***	0.01	-0.04
	(73.84)	(279.56)	(319.58)	(0.02)	(0.04)
Difference	-287.87***	231.84	868.47**	-0.01	0.03
	(92.82)	(387.41)	(419.47)	(0.03)	(0.05)
With children, total effect	1662.20***	1554.97***	17.68	0.00	-0.00
	(56.52)	(272.77)	(277.01)	(0.02)	(0.04)
Observations	65996	65996	65996	61450	65996

Notes: Group fixed at the last pre-2022 observation. Individual, municipality, province-by-year and group-by-year fixed effects. Total effect by the delta method. Standard errors clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F13: Where does the transfer go? Income accounting

	(1) Benefit received	(2) Disposable income	(3) Labour income	(4) Annual rent	(5) Other income
Housing benefit	1791.9***	1473.4***	-313.3	121.3	115.9
	(44.4)	(195.8)	(207.4)	(81.0)	(154.0)
Share of benefit	1.00	0.82	-0.17	0.07	0.06
Observations	68988	68988	68988	68988	68988

Notes: All outcomes in euros per year. Disposable income = benefit + labour income + other income − annual rent, so columns (1) + (3) + (5) − (4) = (2). Other income: self-employment, capital and other transfers. Share of benefit: coefficient divided by the benefit coefficient in column (1). Individual, municipality and province-by-year fixed effects. Standard errors clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F14: Labour-supply margins by age group

	(1) Benefit (€)	(2) Disp. income (€)	(3) Labour income (€)	(4) Any labour income (pp)	(5) SS contributions (€)	(6) Unemployed (pp)
Regression coefficients						
Housing benefit (over 35)	1562.3***	1709.0***	106.6	1.9	11.7	1.6
	(48.9)	(249.6)	(260.6)	(1.6)	(19.9)	(2.2)
× Aged 26–35	686.9***	-386.7	-923.9**	-0.4	-63.6**	-1.7
	(114.4)	(448.5)	(471.3)	(2.5)	(31.1)	(3.7)
× Aged 25 or under	536.1***	-1663.5**	-2111.4***	-2.7	-166.8***	-2.6
	(188.1)	(664.2)	(791.9)	(4.3)	(51.0)	(8.8)
Total effect by age group						
Over 35	1562.3***	1709.0***	106.6	1.9	11.7	1.6
	(48.9)	(249.6)	(260.6)	(1.6)	(19.9)	(2.2)
Aged 26–35	2249.1***	1322.2***	-817.3**	1.5	-51.9**	-0.1
	(103.4)	(375.6)	(395.7)	(1.9)	(24.1)	(3.0)
Aged 25 or under	2098.4***	45.5	-2004.8***	-0.7	-155.1***	-1.0
	(181.5)	(616.6)	(748.7)	(4.0)	(46.9)	(8.5)
Observations	65996	65996	65996	65996	65996	65996
Adj. R^2	0.589	0.634	0.724	0.693	0.731	0.385

Over 35 is the reference group; the × rows are differences relative to it. Total effects by the delta method. Age fixed at the last pre-2022 observation.

Euros per year, or percentage points for columns (4) and (6). Individual, municipality, province-by-year and age-group-by-year fixed effects. Standard errors clustered by individual.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F15: Savings and investment

	(1) Bank balances (€)	(2) Any interest (pp)	(3) Any dividends (pp)	(4) Any bonds (pp)
Housing benefit	49.32	1.91**	-0.36	-0.07
	(272.02)	(0.95)	(0.34)	(0.06)
Observations	68988	68988	68988	68988

Bank balances at 31 December. Any interest, dividends, bonds: positive income of that type declared in the year.

Individual, municipality and province-by-year fixed effects. Standard errors clustered by individual.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.