Insuring Landlords*

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Abstract

This paper demonstrates that unpaid rent risk makes landlords reluctant to supply housing services to fragile tenants; and that insuring owners against it improves the access of renters to high-opportunity neighborhoods. We study the implementation of *Visale*, a publicly funded rent guarantee insurance policy in France, free of charge to eligible tenants and landlords. We exploit exhaustive registry information on all French households, data on the universe of *Visale* beneficiaries and claim payouts, and quasi-experimental eligibility variation across renters. We demonstrate that the non-payment guarantee increased access to private-sector rental housing for eligible tenants. The effects are stronger for immigrants and those with low or volatile incomes, who often do not satisfy standard screening criteria for landlords. The scheme eased the spatial mobility of low-income renters towards higher-wage, higher-rent locations. It led to new household formation and some reallocation of the vacant housing stock, but may have displaced ineligible households in tighter housing markets.

JEL codes: R21, R31, G5.

Keywords: landlords, tenants, rental markets, risk, unpaid rent, guarantee.

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1 Introduction

Investors in rental properties face many concerns: unexpected vacancies, expensive maintenance, stringent rent control, or difficult refinancing conditions. Survey evidence suggests, however, that the most damaging risk factor perceived both by potential and actual landlords is the chance of a delinquent tenant, and the costs associated with collecting – or forgoing – unpaid rent.[1] The resulting financial compensation and extra screening steps that risk-averse landlords demand for bearing unpaid rent risk could account for the high cost of housing faced by lower-income communities (Desmond and Wilmers, 2019), and a lower chance of "moves-to-opportunity" for fragile and poorer tenants (Bergman et al., forthcoming), potentially as a byproduct of discrimination (Christensen and Timmins, 2023).

Nevertheless, empirical evidence on the magnitude of non-payment risk – and its consequences for renter access to housing and spatial mobility – remains scant. Unlike homeowner default on mortgages, which is readily observable, measuring non-payment hazard among renters is difficult in the absence of detailed administrative data on rent payment behavior. More importantly, estimating the *ex ante* causal effect of landlords' risk perception is challenging. Unpaid rent risk empirically correlates with other characteristics of tenants, such as citizenship or employment status, which could independently affect screening, housing affordability, and tenant mobility, through e.g. discrimination or social networks.

In this paper, we study a unique, publicly funded insurance policy specifically designed to alleviate rent delinquency risk, free of charge to landlords and renters: France's *Visale* guarantee. *Visale* aims at "de-risking" landlords' exposure to non-payment when leasing out dwellings. Using a quasi-experimental design, we show the guarantee improved the willingness of owners to rent to eligible tenants. We also demonstrate that despite its low fiscal cost, it resulted in a stark increase in spatial "moves to opportunity" of lower-income households towards high-rent, high-wage, high-density neighborhoods. In tight housing markets, it fostered a reallocation of the rental stock towards more fragile renters, potentially at the expense of ineligible households with similar housing consumption bundles.

[1] For survey evidence in the US residential sector, see Non-Payment is Top Landlord Concern, TransUnion Study Finds, March 2014, TransUnion. Similarly, surveys conducted in France (e.g. by listing website SeLoger) list non-payment of rent as the most commonly reported concern for private sector landlords.

Visale indemnifies landlords if and when pre-approved tenants fail to pay their rent on time. Its stated objective is to combat rationing in the rental market – in the context of France's strong tenant protections – and improve high-risk tenants' access to rental housing opportunities. As of end-2023, the scheme had underwritten more than 1.2 million rental contracts, covering close to a third of newly signed eligible leases annually. We assemble detailed tax registry information on all occupied housing units in France; Census micro-data; and restricted-access administrative data from the *Visale* rent guarantee insurance on beneficiaries (tenants and landlords), leases, and claim payouts. Until late 2021, *Visale* mostly targeted younger tenants – including students – below 30 years of age at the start of their lease. We take advantage of this age-based differential exposure to the policy to estimate its causal effect on tenants, landlords, and housing markets. This allows us to provide the first quasi-experimental piece of evidence quantifying how insuring landlords affects risk, returns, and tenant selection, and how it influences the access of low-income and constrained renters to high-opportunity locations.

Using the *Visale* data on beneficiaries and claim payouts, we start by providing some of the first systematic evidence on the economic magnitude of unpaid rent risk among younger and low-income individuals. Our large sample of rental contracts allows us to evidence economically significant non-payment risk among *Visale* beneficiaries, up to a cumulative 6-7% over the life of a rental contract. The average non-payment event occurs early in a lease, and represents close to five months of unpaid rent, exposing small "mom-and-pop" landlords to large financial hazards. We find some evidence for the presence of initial adverse selection of higher-risk renters into *Visale*, and for moral hazard as rents are distorted downwards to match eligibility criteria. High initial rent-to-income ratios, immigrant status, family composition, and lower-skilled occupations are all quantitatively relevant predictors of non-payment risk. Nonetheless, observable characteristics of tenants do not account for the bulk of variation in unpaid rent, which we show is largely unpredictable and idiosyncratic in nature, even using state-of-the-art prediction techniques.

Taken together, these facts suggest that the pooling of risk operated by the *Visale* guarantee may loosen the screening criteria required by risk-averse and un-diversified landlords for bearing unpaid rent hazard. We next show that the provision of insurance indeed improved access to housing opportunities in the private rental sector for targeted tenants. We employ a flexible difference-in-differences empirical design, exploiting unequal eligibility to the scheme between individuals in a narrow age window below and above the 30 years old cutoff, after the start of *Visale* in 2016 and up to its expansion to older age groups at the end of 2021. We evidence a substantial intent-to-treat effect of the policy for eligible age groups, who become more likely to rent as heads of household in the private sector after its implementation. Our results are robust to using either Census or tax registry data, and to variations in the choice of the control group. Additionally, our intent-to-treat effects are spatially concentrated in areas with high observed *Visale* market share, lending credence to the causal interpretation of our age cutoff-based empirical design.

The scheme notably increases the private rental housing share for the most fragile constituencies, including immigrants, domestic and foreign students, and lower income individuals, who all witness larger increases in housing mobility and the probability of living on their own as renters. The effects appear to operate partly through the displacement of ineligible but similar tenants of slightly higher ages with closely connected housing consumption bundles in the "housing rat race", but also through an increased supply of formerly vacant dwelling units by landlords – particularly in less tight housing markets.

We next demonstrate the presence of large "moving-to-opportunity" effects of insuring landlords. Eligible tenants are more likely to move across municipalities after the implementation of the *Visale* policy, compared to untreated age groups. More importantly, these moves are directed: we find evidence that the availability of the scheme in younger age groups improved access to high-opportunity neighbourhoods, in particular for lower-income renters. The gap in economic prosperity between the municipality of destination and the origin increases for eligible households after the start of *Visale*, through both higher rates of departure from low-income municipalities, and a higher probability of moving to a high-income location. The scheme facilitated an increasingly directed mobility towards high-rent, high-density, highly-educated and high-wage ZIP codes. The effects are stronger among low-income individuals, who become more likely to depart low-income towns. Back-of-theenvelope calculations of the implied fiscal expenditure per additional move suggests that providing *owners* with insurance against unpaid rent risk represents a moderate cost tool to facilitate moves-to-opportunity for constrained *tenants*, who would not otherwise satisfy the strict screening criteria put in place by risk-averse landlords.

Contribution to the literature A substantial body of work evaluates measures aimed at enhancing housing affordability, ranging from rent control (Diamond, McQuade, and Qian, 2019) to means-tested vouchers (Jacob and Ludwig, 2012) to the subsidized construction and operation of low- or middle-income housing (Diamond and McQuade, 2019; Levy, 2021). Measuring the supply-side response of landlords to such rental market policies is key to estimating their efficiency. The trade-off between tenant protection and landlords' reaction is also at the core of eviction regulations (Abramson, 2021) and other renter protection laws (Ambrose and Diop, 2021). We show that landlords' perception of the risk associated with a tenant plays an outsized role in modulating access to rental housing, and that targeted interventions can alleviate it at scale and low effective fiscal costs.

Our paper is tightly related to a novel literature focusing on policies designed to address renters' ability-to-pay shocks *ex post*.[2] Rental assistance (Aiken et al., 2022), eviction moratoria (An, Gabriel, and Tzur-Ilan, 2021), and right-to-counsel policies (Collinson et al., 2022) have been proposed as tools to address the *downstream* consequences of failure to pay rent. In contrast, our interest is in the effects of an *upstream* policy addressing rental delinquency risk as perceived by landlords *ex ante*. In contemporaneous work, Abramson and Van Nieuwerburgh (2024) study the theoretical properties of introducing rent guarantee insurance in a calibrated incomplete market life-cycle model. We show empirically that a rent guarantee provision targeted to landlords can alleviate *ex ante* information asymmetry and improve access to rental housing for low-income tenants in expensive locations.

Second, we focus on the impact of *Visale* on spatial mobility to opportunity. There has been renewed interest in the role of housing policies in facilitating or hindering access to high-wage and desirable neighbourhoods. Several studies (e.g. Ganong and Shoag (2017) or

[2] Due to the lack of data, very few papers have been able to study renter default risk separately from eviction. A rare exception is Agarwal, Ambrose, and Diop (2022), who show that local minimum wage increases reduce the chances of unpaid rent.

Hsieh and Moretti (2019)) document that construction regulations prevent upwardly mobile relocation to desirable and productive areas. Aliprantis, Martin, and Phillips (2022) examine how allowing section 8 voucher ceilings to rise in pricier neighborhoods may improve tenant access to high-opportunity locations. We show that a guarantee targeted at landlords to reduce the risk of tenant non-payment produces large moves-to-opportunity effects towards high-wage, high-rent areas for low-income renters. In the context of the mostly fixed stock of housing in the short-run, we show such moving-to-opportunity effects of targeted support policies are large, but can come at the expense of ineligible groups whose consumption basket overlaps with treated households. This result relates to a recent line of inquiry exploring the drawbacks of moving-to-opportunity policies, either due to the ”flight” of incumbent residents (Derenoncourt, 2022), or due to general equilibrium price adjustments (Chyn and Daruich, 2022).

Third, our findings speak to recent work on housing market discrimination, in the United States (Christensen and Timmins, 2023) and in France (Acolin, Bostic, and Painter, 2016). Reducing perceived risk for landlords specifically facilitates access to rental services for disadvantaged populations who are often under-served in the housing market, in particular immigrants and non-citizens, and students or young adults with limited credit history, by reducing the potential for statistical discrimination on the part of the landlord.

Finally, our paper adds to a burgeoning literature on the role of idiosyncratic risk in the housing market. While most of the existing literature focuses on the *price risk* experienced by either homeowners (Giacoletti, 2021) or commercial investors (Sagi, 2021), we are interested in the *rent risk* for landlords that arises from potential default on the part of the tenant. We show that non-payment behavior is largely idiosyncratic, correlated with – but not fully accounted for by – *ex ante* tenant or unit characteristics, and highly consequential for the returns achieved by small-scale landlords exhibiting limited portfolio diversification. This evidence on ”default risk” in the rental market complements studies of mortgage default for at-risk homeowners (Ganong and Noel, 2023).

2 Institutional context, policy details, and data

2.1 Institutional background

Unpaid rent risk in France According to the Survey of Income and Living Conditions (SILC), about 4% of French tenants had been unable to pay rent on time at least once in the previous year as of 2019. Four major features of France's private rental market, which ranks among the most heavily regulated in the OECD (Andrews, Sánchez, and Johansson, 2011), make it a suitable laboratory to study landlords' concerns for unpaid rent risk.

First, tenant security deposits are limited by law to one month of rent for unfurnished dwellings (two for furnished units). They are thus unlikely to cover a substantial share of non-payment costs at the end of a lease, and mostly act as insurance against small damages to the unit. Even in the case of non-payment, deposit repossession is heavily regulated.

Second, eviction cases are complex to file, and their outcomes highly uncertain. The average duration of an eviction case is upwards of one year, and industry sources estimate court costs to lie between EUR 3,000 and EUR 5,000 (more than five months of the average rent in the country), with limited prospects for recovery after a non-payment period.[3] Eviction is also entirely illegal for five months out of twelve (the so-called "winter truce", which lasts from November 1 to March 31 every year), potentially exposing landlords to long-lasting non-payment without any legal means of accessing or repossessing their units. Overall, evictions are rare: while close to 500,000 households receive a formal "demand for payment" every year (c. 4% of all renters), only 150,000 reach the court hearing stage, and 17,500 were evicted with the help of law enforcement in 2022, or 0.14% of all renters.[4]

Third, most private sector landlords in France are small, individual investors, who own fewer than two individual apartments. They often operate these rental dwellings them-

[3] Upon non-payment, a landlord sends out a formal "demand for payment". The tenant automatically benefits from a two months extension. After that, the landlord can send a second injunction, concurrently informing a judge of the non-payment. A second minimum two months period follows, when the renter can apply for emergency assistance from social services. At this stage, a court hearing can take place (a minimum of 4 months – and in practice 5 to 8 months – after the non-payment event) and a judge may nullify the rental contract. A second judge can grant additional payment delays. If payment does not resume, the landlord can request a bailiff to serve the tenant with a formal order to vacate the premises. This triggers a two months grace period, allowing the tenant to find new housing before the order is enforced. Finally, assistance to evict an unwilling tenant can be requested by the landlord from law enforcement, who have two months to respond to the request.

[4] Report from the *Cour des Comptes* budget responsibility office, "*La Prevention des Expulsions Locatives*".

selves, close to their own residence location and without resorting to property management intermediaries (Levy, 2021). Unlike large commercial investors – who can self-insure against non-payment by diversifying risk across tenants, buildings, and locations, such "mom-and-pop" landlords, who represent the bulk of rental supply, are vulnerable to idiosyncratic non-payment risk, given the significant exposure of their portfolio to a single tenant.

Fourth, due to the absence of a credit score system in France, most landlords do not receive objective information on a tenant's default risk. They must rely on stringent selection criteria, including imposing rule-of-thumb maximum rent-to-income ratios, and demanding rent payment receipts from former landlords. Qualitative evidence (e.g. interviews conducted by sociologists Bonnet and Pollard (2021)) suggests that small-scale individual landlords invest heavily in selecting tenants perceived as safe.

Unpaid rent insurance prior to the *Visale* policy To counter exposure to non-payment risk, private sector landlords historically resorted to two main coverage options. The most common is the use of individual guarantors, often parents or relatives, who provide their own tax returns and payslips as evidence of ability-to-pay, and legally commit to paying rent on behalf of the tenant, should they miss any installments. Survey data show that prior to *Visale*, 50% of private-sector landlords required renters to provide such a guarantor – and close to three quarters of those whose tenants are students.[5]

The second method is to purchase a private unpaid rent insurance guarantee (*Garantie Loyers Impayes* or *GLI*): 15% of owners subscribe to *GLI* schemes, which cost from 2.5 to 5% of annual rent. Before insuring a lease, almost all private insurers require tenants to have rent-to-income ratios below 33% and permanent labor contracts – a rare feat for younger employees in France's dual labor market.[6] The remaining 35% of landlords do not use any dedicated tool to cover unpaid rent risk, mostly relying on strict screening processes.

In 2014, to remedy the perceived inequality stemming from the central role of individual guarantors and restrictive private insurance, then president Hollande's cabinet proposed a "Universal Rent Guarantee" (*GUL* for *Garantie Universelle des Loyers*). Due to internal con-

[5]ANIL (*Agence Nationale pour l'Intermediation Locative*) survey "*Enquete sur la securisation locative*", 2018.

[6]Private rent insurance, while uncommon in the United States, exists in some European countries, including France and the UK. In the US, startups such as *theGuarantors*, *Steady*, and *RentRescue* provide similar products.

flicts in the administration about its potential costs,[7] however, it was eventually abandoned.

2.2 The *Visale* guarantee

Instead of the *GUL*, in 2016, a smaller scale public guarantee scheme, the *Garantie Visale*, was made available for free to landlords and tenants by *Action Logement*, a public-private partnership steered jointly by employer and labor unions. *Visale*[8] provides coverage against non-payment of rent, or damages to a rental unit. No deductible or coverage delay applies. The guarantee covers up to 36 months of rent, imposes a maximum rent of EUR 1,300 (1,500 in the Paris area), as well as a maximum rent-to-tenant income ratio of 50%. Students with no income are not subject to the rent-to-income condition and only face a flat rent cap of EUR 600 (800 in the Paris area).

The *Visale* guarantee is designed to encourage access to the private rental sector for low- and volatile-income tenants, including students, young adults, mobile workers, and non-citizens. The guarantee was therefore initially only made available to individuals less than 30 years of age, except students, and to mobile employees older than 30 in the first few months of their labor contract. It was later expanded to students (in 2018), and then to very low-income employees aged older than 30 after June 2021. *Visale*'s sponsor agency describes it as a form of "affirmative action" towards fragile renters who do not have access to the support of an individual guarantor.

Prior to finding a rental, a potential tenant seeks pre-approval by the *Visale* scheme (which verifies tenant eligibility). As part of their application file for a specific unit, they then present their pre-approval or "*visa*" to the landlord in lieu of an individual guarantor. If the *Visale*-sponsored tenant is selected by the landlord, specifics of the lease are reported to *Action Logement* through an online platform. These include agreed rent and utilities, address, as well as some landlord and tenant characteristics. Once *Visale* verifies that the terms respect eligibility conditions (in particular, the maximum rent and rent-to-income ratio), the

[7]Modelled after the French universal healthcare system, the *GUL* would have provided universal coverage against unpaid rent for all private rentals in the country, and was due to be financed by a proportional tax on rents. A parliamentary report (*Projet de loi pour l'accès au logement et un urbanisme rénové - Etude d'impact*) in anticipation of the law evaluated its yearly cost at c. EUR 700 million, assuming 91% coverage of 6.7 million private dwellings with a 650 EUR average monthly rent, a 2.5% default rate, an 8-months average default duration, and a 7.5% recovery rate.

[8]The acronym stands for *Visa pour le Logement et l'Emploi* or *Visa for Housing and Employment*.

contract is insured. In the event of non-payment or property damages, the owner requests payment directly from the agency. Following a landlord claim and payout, the *Visale* scheme obtains the exclusive right to collect the corresponding unpaid amounts from the tenant, and usually attempts to schedule a long-term payment plan with them. In practice, recovered amounts from tenants are about 20% of landlord payouts.

2.3 Data construction

We use three complementary sources of data to uncover the causal effect of the *Visale* scheme on the housing market, tenants, and landlords. First, we exploit the *FIDELI* detailed tax registry data covering the universe of individuals and housing units in France — approximately 67 million individuals and 36 million dwellings on average during the period. These microdata, available from 2015 to 2022, provide annual snapshots of residents as of January of year t – and are informative on mobility events during year $t-1$. They combine information on individual demographics (e.g., age, family composition), disposable income sources (e.g., wages, pensions, financial income), as well as dwelling characteristics (e.g. number of rooms), occupation status (owner-occupied, rental, second home, or vacant), and geographic coordinates. Although the data are repeated cross-sections and lack a panel structure, we can track whether a household moved across housing units since $t-1$.

The *FIDELI* tax data have some limitations. They lack observable demographics such as immigration status, citizenship, or education. They fail to account for most students who are still registered at their parents' home on tax returns. To mitigate potential errors stemming from mis-measuring students (a key target population for *Visale*), we restrict the *FIDELI* sample to individuals aged 25 and older. Lastly, the dataset only covers the period after 2015, thereby limiting the exploration of pre-policy trends.

To address the shortcomings of *FIDELI*, we also incorporate individual data from the French annual Census. This allows us to explore longer pre-policy trends (from 2013) and measure immigration and occupation. It includes students when they move out of their parents' home, allowing us to study all individuals aged 18 or more. However, the Census does not provide information on incomes or past housing situation; and its latest release stops at the start of 2020, prior to a substantial increase in *Visale* take-up in the years 2020 and 2021. Consequently, *FIDELI* and the Census are complements.

Finally, we also obtained access to restricted-access data from the *Visale* scheme itself on the beneficiaries of the guarantee: tenants, landlords, and each individual lease and non-payment claim. By construction, these data only contain information on ever-treated households. They allow us to assess their characteristics relative to the broader population, and to quantify the value of the subsidy by computing the frequency, incidence, and cost of rental non-payment. In addition, we use these data to derive a granular measure of spatial exposure to *Visale* at the municipality level by computing the total number of *Visale* contracts normalized by the total number of dwellings in the location occupied by individuals aged between 25 and 35 years old. Figure C.3 maps the spatial distribution of this indicator across 35,000 municipalities in the country. Contracts are over-represented in high-density locations, in college towns, and in cities with a younger population of renters.

Table 1 provides summary statistics on the overall population of tenants from the *FIDELI* files. Data on the specific population of users of the *Visale* scheme for whom we have data are displayed in table D.1. *Visale* tenants are lower-income, substantially younger, and more likely to be single and to be born abroad than the average renter in France, consistent with legal restrictions on eligibility and the overall policy goal of targeting more fragile renters.

3 Stylized facts on *Visale*

3.1 Policy understanding and take-up

To gauge interest in the policy among landlords and renters, we first show evidence from GoogleTrends search data that interest in the rent guarantee policy grew rapidly following its implementation and gradual take-off. In particular, we plot in figure 1 the evolution over time of (normalized) Google search interest for three tools of insurance against non-payment: *Visale*, *individual guarantor* (*garant*), and *rental deposit* (*caution*). While all three series exhibit seasonality related to the pace of activity in the rental market, which picks up around September every year, there is a sharply increasing trend of interest in the *Visale* guarantee, which overtakes the other two at the end of the sample in 2023.

In parallel, figure 2 uses *Visale* micro-data to plot the estimated take-up of the scheme over time for various groups: students; non-students below 30 years old; non-students above 30. It illustrates the rapid take-off of the policy in 2018 as knowledge of its availability became more broad-based among landlords. As of end 2023, slightly more than 1.2 million *Visale*-supported rental agreements had been covered under the policy since its implementation in 2016. Currently registered students (at the start of the rental contract) account for close to half of all contracts, and about 90% of all renters are below the age of 30 years old.

3.2 Non-payment: stylized facts

While the *Visale* beneficiaries are a selected sample, understanding the magnitude and behavior of unpaid rent risk among households treated by the policy is essential to gauge its financial relevance for landlords operating in this segment of the housing market. We take advantage of the *Visale* micro-data on beneficiaries to evidence three main facts on the incidence of non-payment risk among younger, low- and middle-income households.

Fact 1: Non-payment risk is economically significant First, non-payment risk among the treated is a low-probability but economically consequential event. About 6 to 7 percent of all *Visale* contracts experience at least one non-payment event triggering the guarantee within 36 months of the start of the rental agreement. Figure 3a displays the cumulative probability of triggering the guarantee against days since the start of the rental agreement. Most non-payment events occur within the first six months of a rental contract. Moreover, non-payment involves significant financial stakes from the perspective of landlords. The average non-payment event represents five months of unpaid rent, and EUR 2,500 in claims, as well as additional legal fees of close to one month of rent on average.

Fact 2: Non-payment risk is correlated with financial vulnerability Second, non-payment risk is correlated with tenant demographics and socioeconomic characteristics. Non-payment occurrence grows quickly with initial rent-to-income ratios. Figure 3b shows that the incidence curve of non-payment for households with lower initial-rent-to-income ratios lies below the incidence curve for high rent-to-income households, suggesting that non-payment occurs more frequently and earlier in the life cycle of a rental contract for highly burdened households.[9] Non-payment hazard is also highly correlated with other predictors, including immigrant status. It occurs more often and earlier for non-EU foreign citizens than for either French or EU citizens (as displayed in figure C.5).

Table 2 provides regression evidence on the correlation between (the binary occurrence of any) non-payment and various characteristics. EU citizens, students, and tenants in short-term furnished rentals are less likely to experience non-payment, and conversely for individuals with low or missing incomes, high rent-to-income ratios, or non-EU foreigners.

In addition, appendix A provides evidence that the insurance scheme displays modest amount of adverse selection of higher-risk renters into using the scheme, and moral hazard as landlords and tenants distort reported rents and reported rent-to-income ratios in order to meet the eligibility criteria and be sponsored by the *Visale* scheme.

Fact 3: Non-payment risk is mostly idiosyncratic Third, in spite of these correlations, non-payment remains a largely idiosyncratic event, and prediction methods using available information have limited explanatory power for the likelihood of its occurrence. Even when multiple demographic and unit-specific features are taken into account, as shown in the bottom line of Table 2, the coefficient of determination is extremely low, suggesting substantial unexplained variance in the binary outcome of default. In the last column of table 2, which includes province-by-year fixed effects to proxy for local business cycles and time-invariant regional characteristics, in addition to a host of observable demographics, the explanatory power remains low, with an R-square of less than three percent.

Even when exploiting non-linear and non-parametric prediction techniques, such as non-linear logit or random forests, we find that the realization of tenant default displays limited predictability *ex ante*, when using only baseline characteristics available to landlords at the origination of the lease. Following (Fuster et al., 2022), we train three models (linear logit, non-linear logit, and random forest) on a training sample (70% of the sample), and assess the accuracy of the models on the rest of the test data (30%). Details about the imputation and estimation procedure, and the results of each model, are provided in appendix B.

[9]Figure C.4 displays the steeply increasing relationship in the *Visale* micro-data between the probability of unpaid rent triggering the guarantee and the initial rent-to-income ratio of the tenant at the start of the rental agreement, in the pooled data for all renters with strictly positive income.

Random forest estimation of default risk performs better than non-linear logit, which does better itself than the linear logit. However, even for random forests, the AUC for the sample with non-missing incomes is still below 0.7, a poor prediction performance relative, for example, to those obtained by Fuster et al. (2022) using credit scores and additional demographics when trying to predict homeowner default on mortgages. We conclude that even non-linear techniques have limited predictive ability for future default on rent payment by tenants, especially in the French context with limited information on credit worthiness, suggesting the highly idiosyncratic nature of rent non-payment risk.

Taken together, these facts suggest that non-payment constitutes a major financial risk to landlords, especially for those operating at the lower end of the renter income and vulnerability spectrum; and that landlords' exposure to this mostly idiosyncratic source of rental income volatility might be alleviated by insurance tools designed to pool risk across leases.

4 Empirical strategy

We aim at establishing the causal intent-to-treat effects of being eligible to the *Visale* scheme on access to housing in the private rental sector; housing conditions and affordability; and spatial mobility to high-opportunity areas for constrained renters.

As described in section 2, our main empirical strategy exploits variation in exposure to *Visale* between people younger than 30 and those slightly older. While both groups are likely to experience similar overall housing outcome dynamics in the absence of the policy, the fact that *Visale* almost exclusively targeted renters below the age of thirty (until its expansion to broader age groups after 2022) implies that we can estimate its causal effects by measuring the differential post-policy evolution of outcomes in the younger, eligible age group. Figure C.1 supports our empirical design and identification assumption, by showing that, consistent with differential *de jure* eligibility to the scheme, actual users of the *Visale* scheme over the period 2016-2023 are *de facto* heavily over-represented in the below-30 age group, with a sharp and discontinuous drop in the number of users at the exact age of 31. Close to 90% of all users of the *Visale* scheme during the period were below the age of 30.[10]

[10]Given the sharp discontinuity in the proportion of users of the scheme at the exact age of 30, a fuzzy regression discontinuity (RD) design would seem feasible. However, one should note that the exact age cutoff

Our main estimating equation is the following difference-in-differences specification:

$$Y_{i,t} = \alpha_{a(i)} + \delta_t + \beta \mathbb{1}_{a(i) \leq 30} \times \mathbb{1}_{t \geq 2016} + X_{it} + \epsilon_{it} \tag{1}$$

β is the coefficient of interest, capturing the differential outcome for individuals aged 30 or less, after the implementation of the policy in 2016. The baseline specification includes a full set of year fixed effects to account for common trends in housing markets; age fixed effects to purge our data from permanent differences in housing market status across age groups; as well as a potentially empty set of individual-level controls X_{it} designed to take into account potential composition effects that may correlate with non-policy determinants of access to housing in the private rental sector across age bins and over time.

Our estimating strategy aims at establishing two distinct facts. First, we study the intent-to-treat *causal* effect of the scheme on access to housing and the decision to move: the main outcomes of interest are a dummy variable equal to one when the individual is a single person renting in the private sector; and a dummy equal to one when the individual moved into a rental in the last year. Second, we study the *selection* effect on the scheme on *who* is likely to move and *where*, among the treated. There, we condition on the mobility event dummy being one, and study outcomes related to the dwelling of current residence (whether the dwelling was vacant in the prior year); to the income of the individual; and to the municipalities of current and past residence (average incomes, average rents, population density, and the presence of a university) in order to estimate selection effects on renters who are induced to move by *Visale*.

The parallel trends assumption underlying the interpretation of β is that housing outcomes would have evolved similarly for older and younger households around the cutoff, before and after 2016, if not for the implementation of the rent guarantee scheme. We provide several robustness checks and separate pieces of evidence that this assumption holds.

First, we examine a narrow window around the cutoff age (from 18 to 40 in the Census data; from 26 to 35 in the *FIDELI* data, as explained in section 2.3). We also demonstrate in an

condition only applies to renters as of the *start date* of their rental contract. Since in both the Census or *FIDELI* tax registry data, we observe current renters who may already be several months or years into their rental contract, we cannot avail ourselves of such an RD design. We rely instead on a difference-in-differences empirical strategy, comparing groups above and below the age of thirty within a narrow age band.

event study specification the absence of differential pre-policy trends between the younger and older age groups in the period immediately preceding the start of the *Visale* guarantee (from 2013 to 2016), using the following estimating equation:

$$Y_{i,t} = \alpha_{a(i)} + \delta_t + \sum_{k=2016-T}^{2016+T} \beta_k \mathbb{1}_{a(i)\leq 30} \times \mathbb{1}_{t=k} + X_{it} + \epsilon_{it} \tag{2}$$

We finally show that treatment effect heterogeneity aligns well with the differential actual take-up of the *Visale* guarantee across demographic groups and locations. In particular, we study the interaction of the policy with housing vulnerability, by limiting the Census sample to immigrants, who are less likely to have local contacts who can act as trustworthy private guarantors. We show that the estimated effects on access to rental dwellings in the private sector are strongest among this more fragile renter group, suggesting that the guarantee's main impact is indeed on tenants with no alternative security to provide to landlords. Second, to probe the robustness of our identification assumption, we exploit the detailed location information available in the *Visale* data to split *communes* (equivalent to US ZIP codes) into groups of varying *Visale* contract intensity.[11] We then re-run our main regression of interest (regressing the probability of being a private sector renter) separately across groups to demonstrate that the effects on access are concentrated in high policy take-up locations.

5 Empirical results

5.1 Access to private rental housing

Graphical evidence The key objective of the *Visale* scheme is to improve access to rental housing for treated individuals who would otherwise be excluded due to limited resources, excessive rent-to-income ratios, or the absence of a private guarantor to vouch for their ability to pay rent. If *Visale* accomplishes this objective, we should observe a larger increase in the likelihood of becoming a renter in the private sector (as head of household) among individuals aged less than thirty (relative to those slightly above the maximum age of eligibility),

[11] Intensity is defined as the ratio of total contracts from 2016 to 2023 over the number of rental units in the ZIP code in 2016

after the policy was made available in 2016.

As a preliminary visualization of our main empirical strategy, we plot in figure 4 the observed share of renters in the private sector in Census data, binned by age, for renters aged 18 to 43, in January 2016 (the last Census round prior to the implementation of the policy) and in January 2020 (the last year of data available, prior to the expansion of the *Visale* guarantee to a broader target group including households above age 30). The figure evidences that while the share of private sector renters appears roughly unchanged from 2016 to 2020 for the ”control” group of households aged more than 30, it substantially increases among the younger, potentially treated households.

Baseline specification We then turn to the estimation of our difference-in-differences and event-study intent-to-treat specifications (equations 1 and 2). Using *FIDELI* data to maximize the number of post-policy years (until January 2022), figure 5a plots the full event-study specification, focusing on the share of individuals (in age bins from 26 to 35) who are private sector renters and single male or females.[12] The coefficients displayed are the estimated interaction terms between year fixed effects and an indicator for age bins below 30, up to the beginning of year 2022 in the *FIDELI* data.

The event-study coefficients display no evidence of differential trends between younger and older age groups prior to the implementation of the policy. They then exhibit a sharp differential increase in the predicted share of single private sector renters among the younger age group (entitled to the *Visale* scheme after 2017). The implied causal increase in the share of private sector renters among individuals aged 30 or less, after the implementation of the policy in 2016, also demonstrates a steady rise in the differential effect up to 2022, consistent with the gradual increase in take-up of the guarantee among eligible private sector renters documented directly in the *Visale* data (figure 2).

Robustness Our identification strategy, which compares the housing situation of individuals aged less than thirty to those just above the age threshold, could potentially be threatened by the presence of other housing market shocks affecting households below the age of thirty

[12]We restrict the treatment group to these households as, unlike couples with potentially different ages, we know for sure that they are eligible for the policy.

differently from older households, independent from their eligibility to the *Visale* scheme.

To thwart this concern, we exploit the detailed location information available in the *Visale* data to split municipalities[13] in the administrative micro-data into three groups: the set of municipalities which never received any *Visale* contract from 2016 to 2023; and the two sets of below and above median *Visale* contract intensity, defined as the ratio of total contracts from 2016 to 2023 over the number of rental units in the ZIP code in 2016. We then re-run our main regression of interest (regressing the probability of being a private sector renter) separately for each of these three groups.

As shown graphically in figure 5b, in towns that received no *Visale* renter throughout the period, we find no differential trend, either before or after the implementation of the policy, in the private renter share of households aged 26-30 relative to those aged 31-35. Importantly, we find that the positive treatment effect of being aged less than thirty, post-2016, on the share of renters in the private sector, is stronger in towns with a larger *Visale* presence (the top half of *Visale* intensity), consistent with the differential rise in the private renter share being driven by the causal effect of the scheme.

Table D.3 summarizes our results, suggesting that the share of single individuals living in the private rental sector increases by about 1.2 percentage point in the below-30 age group after the implementation of the policy in the upper-half of *Visale* intensity municipalities, and by close to 0.9 percentage points overall nationwide. Taken together, these estimates are akin to a triple-differences strategy. While there is no quasi-experimental random source of variation in the use of *Visale* contracts across municipalities, the absence of a differential post-policy increase in the rental share for younger age groups in towns with no *Visale* presence rules out any alternative unobserved housing market shock increasing the younger group's private rental share everywhere equally.

Heterogeneity We exploit additional information available in the Census data to determine whether the policy had heterogeneous effects by immigration status. Table D.1 already suggests that *Visale* users are substantially more likely than the aggregate population of renters in France to be non-EU citizens. Unlike the tax registry information, the Census data allows us to directly observe the immigration status of households.

[13] French *communes* are roughly equivalent to US ZIP codes.

Panel C.10b plots the intent-to-treat event study results solely among immigrants. The rise in the private rental share is substantially and significantly more pronounced (at about 2 percentage points) among immigrants than among the population as a whole (panel C.10a, at about 0.7 p.p. in 2020), in line with the hypothesis that *Visale* is particularly beneficial to potential tenants with limited local networks and ability to use traditional rental protection schemes such as private guarantors.

Summary Overall, we find an economically large, statistically significant, and persistent increase in the access of *Visale*-eligible households (singles below the age of 30) to housing opportunities in the private rental sector. Our results are entirely driven by the effect observed in towns with high *ex post Visale* penetration, lending credibility to our age-cutoff-based empirical design. They also exhibit predictable heterogeneity by immigrant status. This is consistent with the claim that *Visale*'s safety might provide a differential boost to individuals – such as immigrants – with the least resources to demonstrate ability to pay in a rental market plagued by asymmetric information.

5.2 Spatial patterns and Moves To Opportunity

Descriptive evidence Access to rental housing is especially constrained in high-rent, high-density locations with strong employment opportunities, higher wages, and high-quality schools and learning institutions. In the *Visale* individual data, we can directly compute measures of "moves to opportunity" for the treated group, since we obtained both the (pre-move) address at the time of applying for a certification by the scheme, and the (post-move) address of the new unit after signing a rental contract sponsored by *Visale*. As shown in figure C.11, a majority of the cross-towns moves observed in the *Visale* micro-data involves an increase in average municipality rent per square meter, suggesting that moves among *Visale* renters tend to be upwardly mobile towards higher-amenity or higher-wage locations.[14]

Causal evidence on mobility We next provide direct causal evidence that the policy increases access of targeted lower-income renters to high-rent, high-density municipalities.

[14] In unreported results available upon request, we also find that most *Visale* renters tend to move towards higher-average wage and income municipalities than their original municipality of residence.

As a first step, we run the same intent-to-treat event-study specification as before, where the outcome variable is now a dummy variable for renters who moved into a new municipality from year $t-1$ to t.[15] We observe in figure 6 that renters aged 30 or less have a higher probability of moving to another town than those aged above 30 after the implementation of the policy, relative to before. The overall effect is a large 0.3 percentage point increase in mobility rates for renters aged below 30 after the policy is implemented, as summarized in column (1) of table D.2. This implies that eligibility for the non-payment guarantee increased the share of movers among the treated, an effect that we interpret as evidence for initially constrained tenants being able to move into a preferable location.

To probe the robustness of the result, we run the same heterogeneous sample strategies across municipalities with no, low, or high *Visale* penetration, where the outcome variable is now cross-town mobility for renters. Similar to the findings on access to rental housing, the mobility results are entirely driven by municipalities with positive and high *Visale* penetration. The estimated effects on mobility increase monotonically with *Visale* intensity (columns (2) to (4) of table D.2). A placebo test on the differential moving rates for younger renters into municipalities with zero *Visale* contracts yields a precisely estimated zero coefficient (see figure 6b, and column (2) of table D.2).

Mover selection We next turn to the investigation of the characteristics of movers, and the municipalities into which these new renters move. To increase power and focus on the directed-ness of spatial mobility events, we restrict our sample to the subset of single renters in the *FIDELI* data who did move across municipalities in the last year.

We first find that the effect on mobility is concentrated among lower-income individuals. To demonstrate this selection effect, we run a new event study within the subset of renters who moved, and use as the outcome variable the individual income of movers. Figure 7 and column (1) of table 5 show that the income of moving renters decreased among younger individuals relative to the control group after the implementation of the policy. This selection suggests that the mobility effects of *Visale* are concentrated among the population of lower-income tenants, who are likely to be most constrained by the stringent rent-to-income

[15] We also run the same specification separately across groups of municipalities with different degrees of *Visale* penetration.

criteria imposed by landlords.

Moving to opportunity Among the subsample of single renter-movers, we next compute a measure of origin-destination ”gaps” in economic characteristic. We define them as the difference in the average of a given characteristic between the destination and origin municipalities: e.g. the density gap is the density of the destination municipality minus the density in the origin municipality. We use these gaps as a quantifiable proxy of directed mobility for movers of various age groups, before and after the implementation of the rent insurance guarantee. We run the same event-study as before in the subsample of renter-movers, comparing the differential evolution of mobility gaps in the treatment and control groups, before and after the roll-out of the *Visale* rent guarantee.

We find empirical evidence that, after the policy was implemented in 2016, movers in the targeted age group tend to have larger increases in local rents and local municipality density than movers in the untargeted group. Figure 8 displays the corresponding estimated coefficients in an event-study specification. As before, the event study coefficients display no pre-policy trends before the implementation of *Visale*. After the policy is rolled-out, we consistently find that younger renters move to municipalities with higher rents, higher density, higher overall living standards, and higher wages. Density gaps between destination and origin increase by about 400 people per square kilometer. Rent gaps increase by about 0.2 EUR per square meter, close to 2 percent of the average in the country. Origin-destination changes in average wages and living standards also increase, respectively by EUR 200 and EUR 700 by the end of the period. Interestingly, we also find that single renters in the younger age group move to places which are relatively more likely (by about two percentage points) to host higher education institutions, suggesting that *Visale* improves access not only to more urban and higher income, but also to higher educated locations, despite the fact that we focus on renters aged above 25 and are mostly not of college-age.

Figure C.12 decomposes how the evolution of these ”mobility jumps” is driven by both the characteristics of the origin and destination municipalities. It generally appears that most of the effect on gaps is driven by the characteristics of the destination location. The characteristics of municipalities of origin do not drastically change after the implementation of the policy, with the exception of a decrease in living standards; whereas the wages, density, and rents of destination municipalities all increase significantly. These results imply that moving renters in the treated group come from similar (but slightly lower income) municipalities as before relative to moving renters in the untreated, higher age groups, but that the *Visale* policy enabled eligible tenants to move to higher opportunity areas where rents, urbanization, wages, and the likelihood of the presence of a university are all significantly higher.

Taken together, the previous two sets of results imply a rise in moves-to-opportunity for the treated group, and a higher likelihood of moving for lower-income households among those aged less than 30. Combining these effects directly, we compute for each individual mover, in each year, the *ratio* of their own household income to average household income in their town of residence. As shown in Figure C.13a, we find that this ratio decreases in the treated group after the implementation of the policy in 2016, relative to the control group of single renters above age 30.

Summary To summarize, we find large effects of the *Visale* policy on spatial mobility. Our results imply that (1) the program increases the share of renters who had the opportunity to move; (2) conditional on moving, it facilitates access to rental housing in high-opportunity locations where tenants were formerly constrained; (3) tenants benefiting from the policy come from more disadvantaged backgrounds than before, both at the individual and local level, resulting in an increase in lower-income individuals coming from low-income towns and sorting into higher-opportunity destination locations.

5.3 Displacement and substitution

Vacancies We test whether a higher exposure to *Visale* led landlords to increase the rental housing supply at the extensive margin, by renting out dwellings which were previously vacant. To do so, for each year, we restrict the sample to renters who moved into a new dwelling from $t-1$ to t, and we measure the probability that these movers occupy a dwelling that was vacant the year before. Our coefficient of interest is the interaction between mover age being less than thirty, and an indicator for the post-policy implementation period. More specifically, we run the following difference-in-differences specification for renter i at time t

in the sample of movers

$$Vacant^{t-1}_{i,t} = \alpha_{a(i)} + \delta_t + \beta \mathbb{1}_{a(i)\leq 30} \times \mathbb{1}_{t\geq 2016} + X_{it} + \epsilon_{it}$$

For each year, $Vacant^{t-1}_{i,t}$ is an indicator variable equal to 1 if the dwelling occupied by i at time t was vacant the year before. A positive coefficient β would suggest that the policy led to a direct reallocation of the vacant housing stock towards eligible renters. Figure 9a shows the event-study coefficients. As all coefficients are non-significantly different from zero, there does not seem to be a clear effect on vacancies.

Figure 9b however investigates heterogeneity in the vacant stock reallocation by average rent per meter squared in the municipality. We find a higher probability of occupying a dwelling that was vacant the year before in low rent areas. These results suggest that if *Visale* had any effect on a reallocation of the vacant housing stock towards rental markets, it was mainly located in markets characterized by low rents and thus lower tightness.

Displacement We then test whether the policy operates by substituting eligible tenants for ineligible renters. In particular, in the spirit of Lalive, Landais, and Zweimüller (2015), we are interested in the ”market externalities” of the rent guarantee program on untreated households, who could be out-competed in the rental market by eligible renters now benefiting from a free and safe insurance policy against non-payment risk. To empirically examine this displacement effect, we consider two different approaches.

First, we measure how the probability of moving to a new municipality changed depending on penetration of the *Visale* scheme in the municipality, before and after 2016, and for each age bin. Running the regression for each age category separately enables us to measure how the probability of moving to a high *Visale* area evolved for each age bin before and after the policy. For each age ranging from 26 to 60, we run the following specification:

$$Moving^{a}_{m,t} = \alpha + \delta_t + \beta High\_Visale_m \times \mathbb{1}_{t\geq 2016} + X_m + \varepsilon_{m,t}$$

$Moving^{a}_{m,t}$ is the share of single private sector renters of age a moving in municipality m at time t. $High\_Visale_m$ is an indicator variable equal to 1 if the municipality is above the 90$^{\text{th}}$ percentile of exposure to *Visale*, measured as the ratio of *Visale* contracts to the population aged between 25 and 35 in the municipality. We expect the cutoff at 30 years old to be fuzzy, as some *Visale* beneficiaries are above 30 (Figure C.1).[16]

As the share of *Visale* contracts in a location may be endogenous to other unobservable local characteristics affecting the probability of various age bins moving to a new municipality, we also instrument for our indicator of exposure to *Visale* with the share of renters in the municipality aged below 30 before the policy, in 2015. We think of this instrumental variable as an index for exposure to *Visale ex-ante*, which should not directly affect the probability of moving after the implementation of the policy.

Figure 10 displays the age-specific difference-in-differences coefficients for the ordinary least squares and instrumental variable specifications. In both specifications, coefficients are positive and significant for individuals aged below 30, consistent with our previous results. As we expected, the effect is fuzzy around the 30 years old threshold as some higher-than-30 age groups still benefit from *Visale*. However, above 33 years old, individuals seem less likely to move to municipalities with high-*Visale* penetration after the implementation of the policy, relative to before. We take this result as suggestive evidence that the *Visale* policy led to displacement of individuals who were not the target of the policy.

In a second, distinct approach, we assume that categories of households who are most likely to be displaced or "out-competed" by beneficiaries of the policy are those looking for rental housing in similar market segments to single renters aged less than 30. For example, single renters aged above 30 and looking for a studio are more likely to be out-competed by single renters below 30 than parents with children looking for a single-family home. We construct a measure $Overlap_i$ measuring how similar the housing consumption of category i is to that of the treated group.[17]

[16]For such individuals, our estimates would capture a mix of the potential displacement effect and some part of the fuzzy treatment effect of *Visale*.

[17]We construct our index of overlap by considering 4 characteristics of dwellings: total area, construction year, number of rooms, and a dummy for apartments (relative to single-family homes). We next split the population into 20 household categories (listed in Table D.4). For each category, we measure ventiles of total area and construction year; quartiles of number of rooms; and the share of apartments. We then measure how these statistics differ in absolute value from the average in the treated group (single renters aged below 30), and average these gaps (across quantiles, then across characteristics) to compute an index of distance to the reference group for each category. We normalize the final index so that the most distant group from singles aged below 30 is attributed an index value of zero, and the index for the treated group is 1. The index is computed using pre-policy year 2016, so that it measures the degree of market overlap between household categories *ex ante*.

With this in mind, our goal is to measure how the probability of moving to a new municipality for a given group is affected by the extent to which their market sub-segment overlaps with singles aged below 30. We then run the following regression, considering only untreated individuals, where $Overlap_i$ is our measure of overlap (a proxy of the potential for displacement)

$$Y_{i,t} = \alpha + \beta Overlap_i \times \mathbb{1}_{t \geq 2016} + \delta_t + X_i + \varepsilon_{i,t}$$

$Y_{i,t}$ is the private renter share (or the share of renters moving to a new municipality) for category i at time t. Since $Overlap_i$ is constructed to increase with consumption similarity to the treated group, we expect β to be negative in case of displacement caused by *Visale*.

Table D.4 displays the value of the index for each household category in decreasing order. The third column also shows whether each group should be considered as eligible to *Visale*.[18] Table 6 displays the regression results. The outcome in the first column is the private renter share. We notice that the extent of overlap of housing consumption with the treated group does not appear to significantly affect the share of renters within a category after the policy. In column 2, however, we show that the share of renters moving to a new municipality decreased after the implementation of *Visale* for households with a higher *Overlap* value (those competing in more similar markets to the treated group). These results suggest that the policy did not differentially affect the probability of becoming a renter for untreated groups with a consumption basket more similar to the treated households, but did decrease their probability of moving to new municipalities. In columns 3 and 4, we run triple difference regressions by adding indicator variables for moving to an area with *Visale* penetration above the median (column 3) and above the 90^{th} percentile (column 4). For column 4, the associated coefficient is negative and significant, suggesting a stronger level of displacement in highly-treated areas. These results suggest that the *Visale* policy indeed generated some substitution by lowering access to areas with high *Visale* penetration for untreated households with the most overlap with the treatment group's housing consumption basket.

[18] For couples close to the 30 years old, one member of the couple might be less than 30, which makes the treatment unclear. Thus, we restrict the untreated group to couples without children aged above 40, couples with children and other households.

6 Discussion

We studied a large-scale public insurance policy targeting non-payment risk in the private rental sector. We exploit detailed micro data on all individuals in France, and a quasi-experimental strategy relying on an age cutoff for renters' eligibility. We provide direct evidence that offering *landlords* insurance against unpaid rent risk improves the access of targeted *renters* to rental housing, and facilitates mobility towards high-opportunity neighborhoods. The effects are especially strong for immigrants, and for low-income or fragile younger tenants who would not otherwise satisfy the traditional screening criteria imposed by landlords to hedge against non-payment risk.

Overall, our results evidence a novel source of mobility constraints for households with limited resources: "risk-based rationing" in private rental markets. Since rental markets operate under a posted price mechanism, private landlords do not offer higher prices to risky tenants, but simply appear to shut them down from housing opportunities. In turn, pooling unpaid rent risk to remedy this rationing triggers moves-to-opportunity for lower-income communities, potentially at the expense of untreated households.

Our findings demonstrate the role that landlords play in *ex ante* restricting access to housing in expensive locations for the more fragile tenants. Given the strongly increasing profile of risk against rent-to-income ratios that we document, risk-averse landlords with limited diversification are highly reluctant to lease out dwellings to tenants with limited resources or volatile incomes. This rationing of *renters* plays a role similar to the down payment constraints that hinder access to high-opportunity locations for low-income or minority *home-owners*, as documented by Gupta, Hansman, and Mabille (2023).

The policy tools to remedy these two sets of constraints, however, appear drastically different. While down payment assistance designed to increase home-ownership for low-income constituencies is costly, because it requires helping all households *ex ante*, providing insurance is relatively inexpensive since it only involves *ex post* compensation. A public guarantee pooling what we evidenced to be a large idiosyncratic component of non-payment risk may thus constitute an important and low-fiscal cost tool to encourage targeted moves to opportunity.

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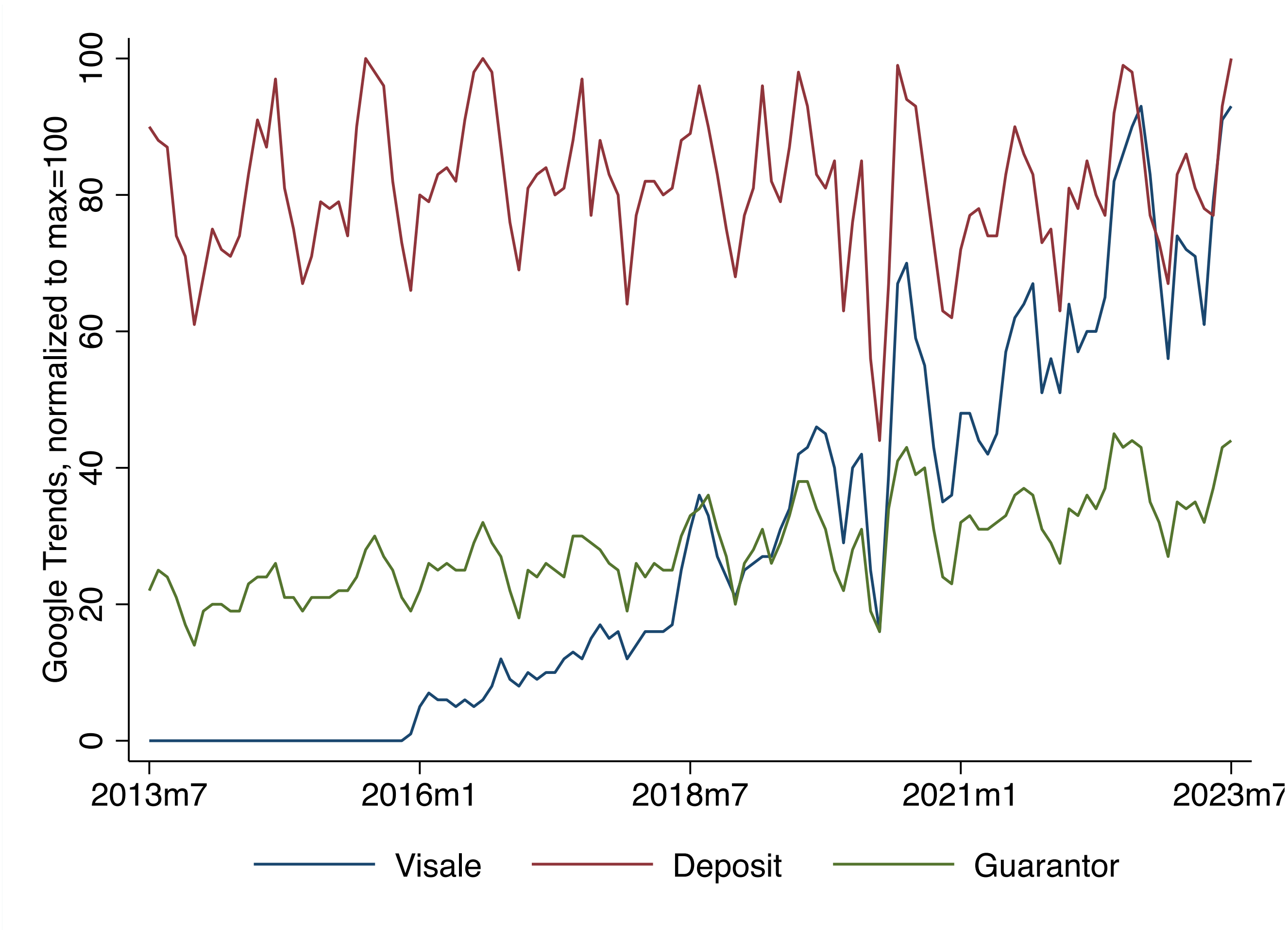
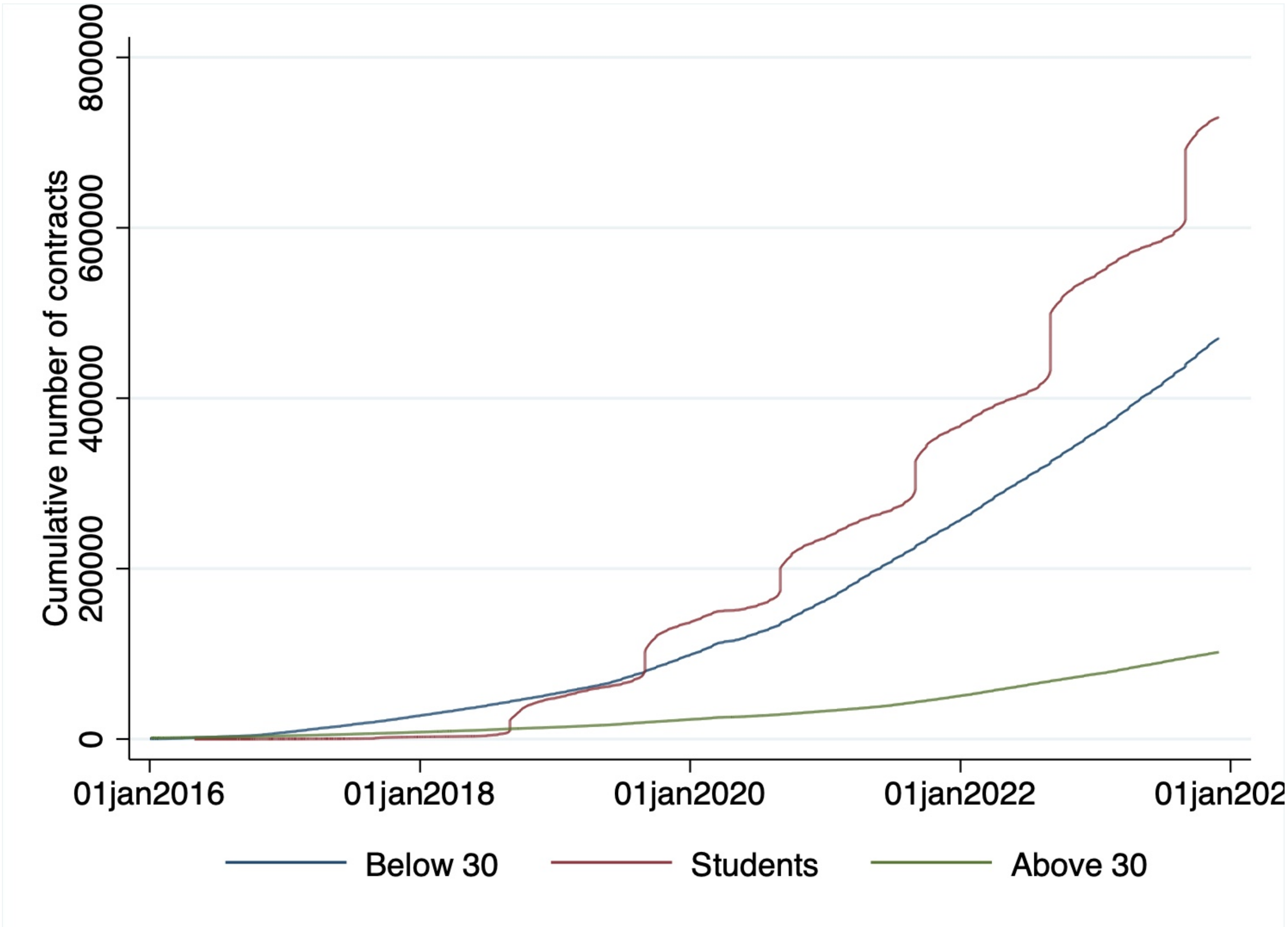
Main figures

Figure 1: **Google Searches, 2013-2023**



Notes. The figure displays GoogleTrends index of Google search interest for the terms ”Visale”, ”deposit” (*caution*) and guarantor (*garant*) over time since 2013, normalized to a maximum value of 100.

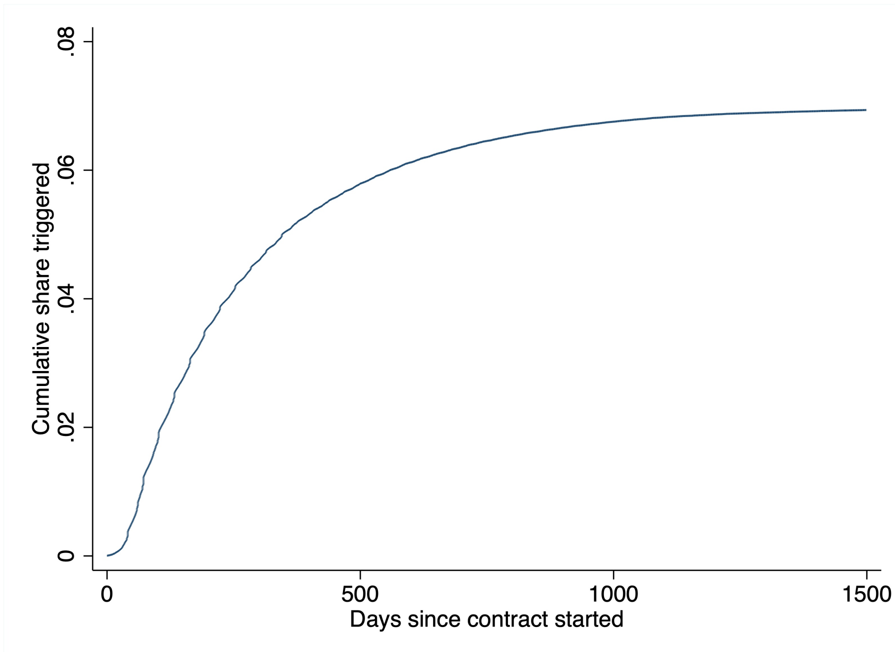
Figure 2: **Visale guarantee take-up, 2016-2023**



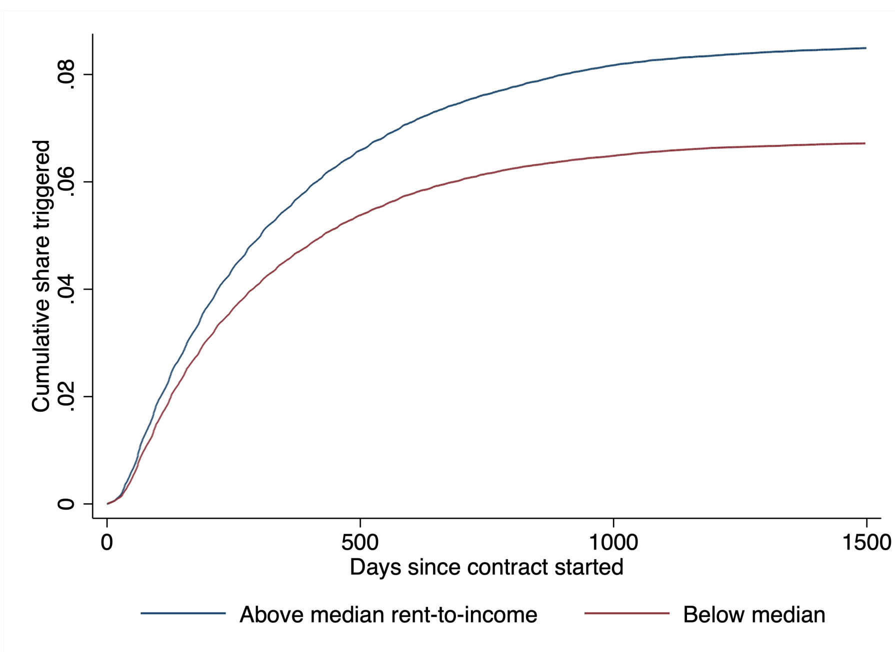
Notes. The figure displays *Visale* take-up over time for various groups (non-students aged less than 30, students, non-students aged more than 30). Students' take-up of the *Visale* scheme displays substantial seasonality, consistent with their housing mobility patterns.

Figure 3: **Unpaid rent risk time profile**

(a) **Overall**

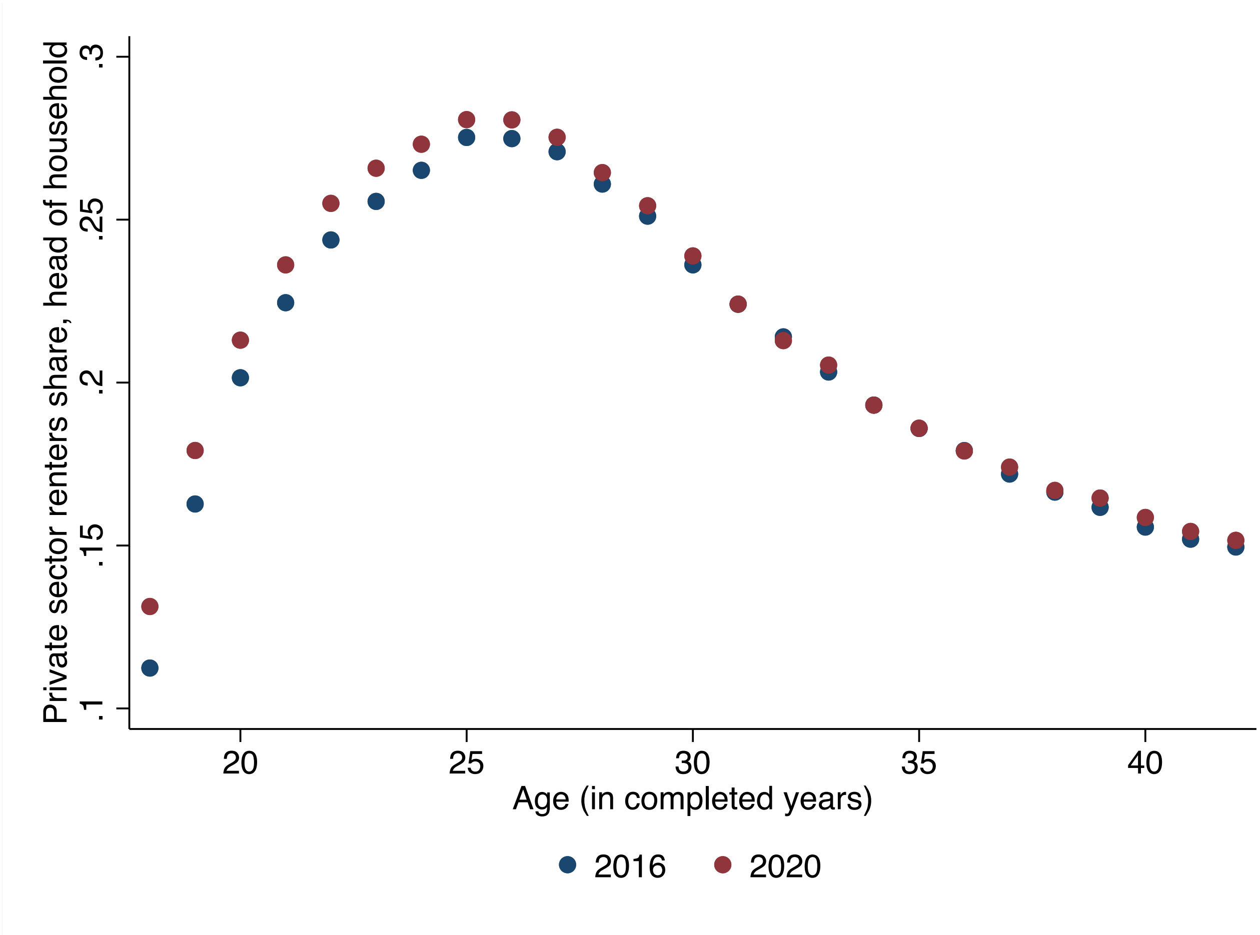


(b) **Split by initial rent-to-income ratio**



Notes. The figure displays the cumulative probability that a *Visale* contract experiences any trigger of the guarantee after a non-payment event against time elapsed (in days) since the start of the rental agreement. Panel 3b splits the contracts (among households with a non-missing income) between above- and below-median rent-to-income ratio at the start of the rental agreement.

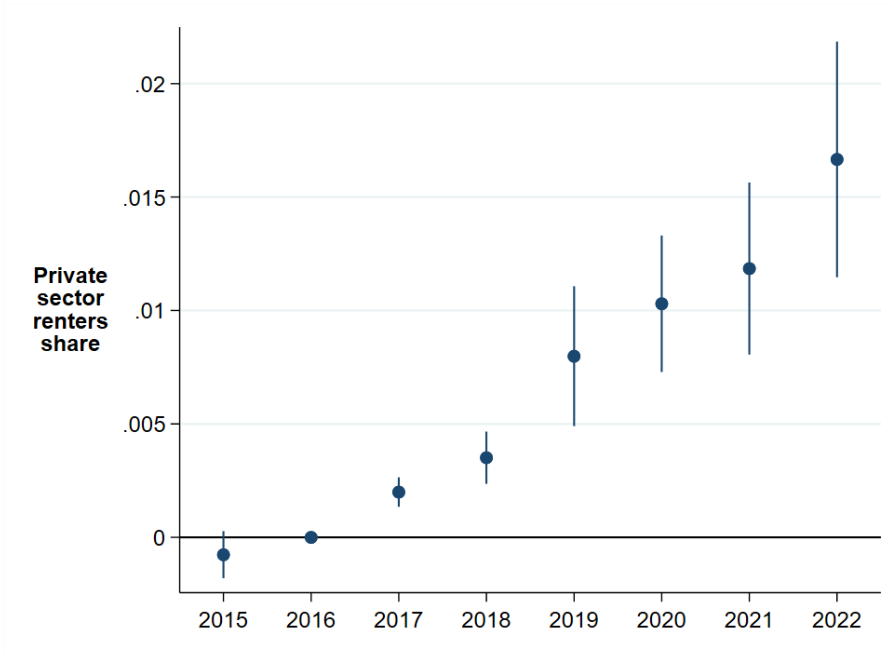
Figure 4: **Private renter share by age, Census**



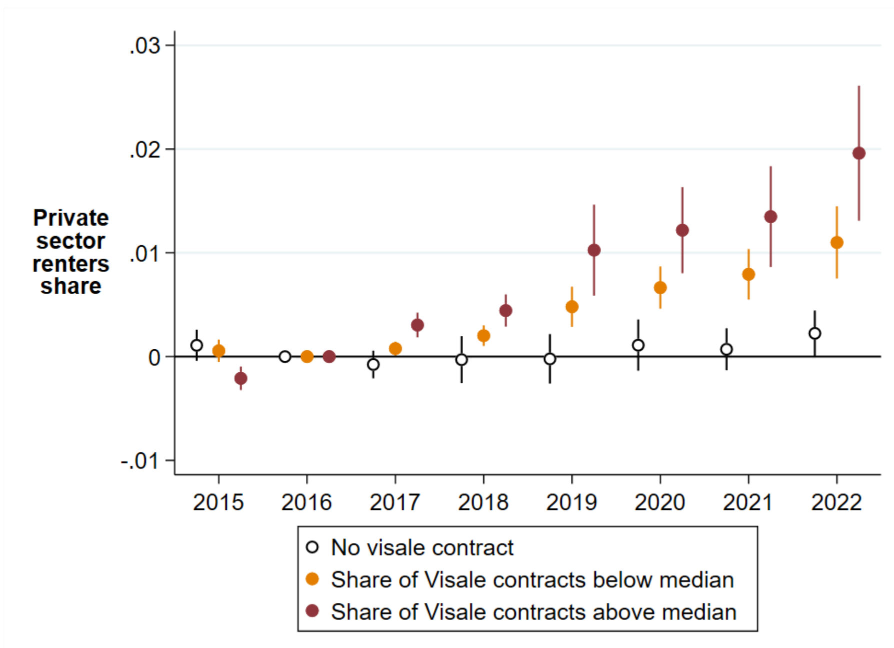
Notes. The figure displays the share of individuals who live in a dwelling characterized as belonging private rental sector and are the person of reference of their household. The x-axis corresponds to the age bin of the individual in the Census, from 18 to 43. The blue dots correspond to the last Census round prior to the implementation of *Visale*, while the red dots correspond to the latest available Census round in 2020.

Figure 5: **Baseline effect of *Visale* on renter share**

(a) **Private renter share**



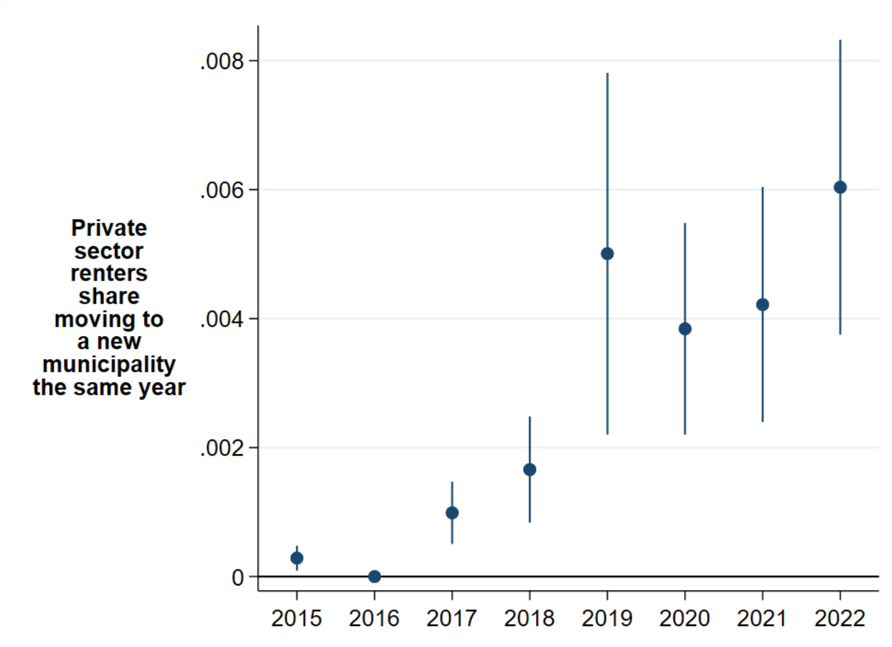
(b) **Heterogeneous effects by exposure to Visale**



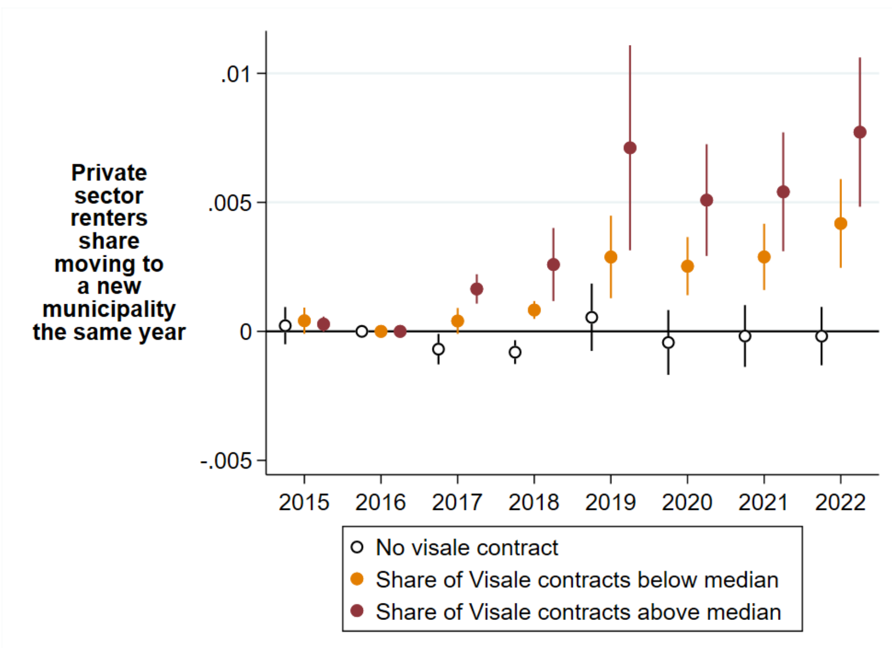
Notes. The figures plot the coefficients on the interaction of being aged less than 31 with year fixed-effects. The sample is restricted to cohorts aged 26 to 35. The outcome variable is an indicator variable for being a single male or female private renter. Panel B plots the coefficients separately for the samples of people living in municipalities with no, low, or high Visale contract intensity. Standard errors are clustered at the age bin level.

Figure 6: **Baseline effect of *Visale* on mobility**

(a) **Private renter share moving to a new municipality**

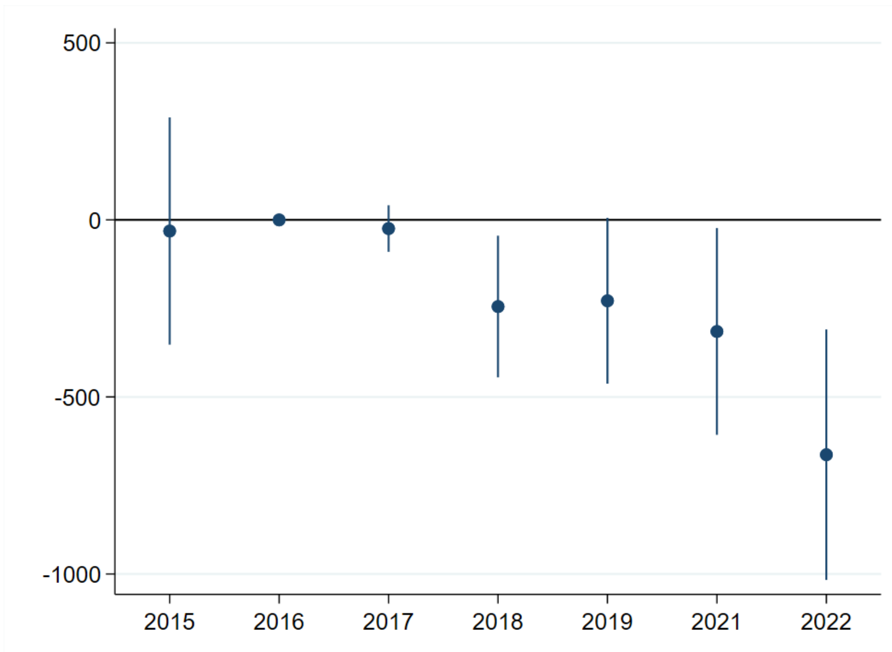


(b) **Heterogeneous effects by exposure to *Visale***



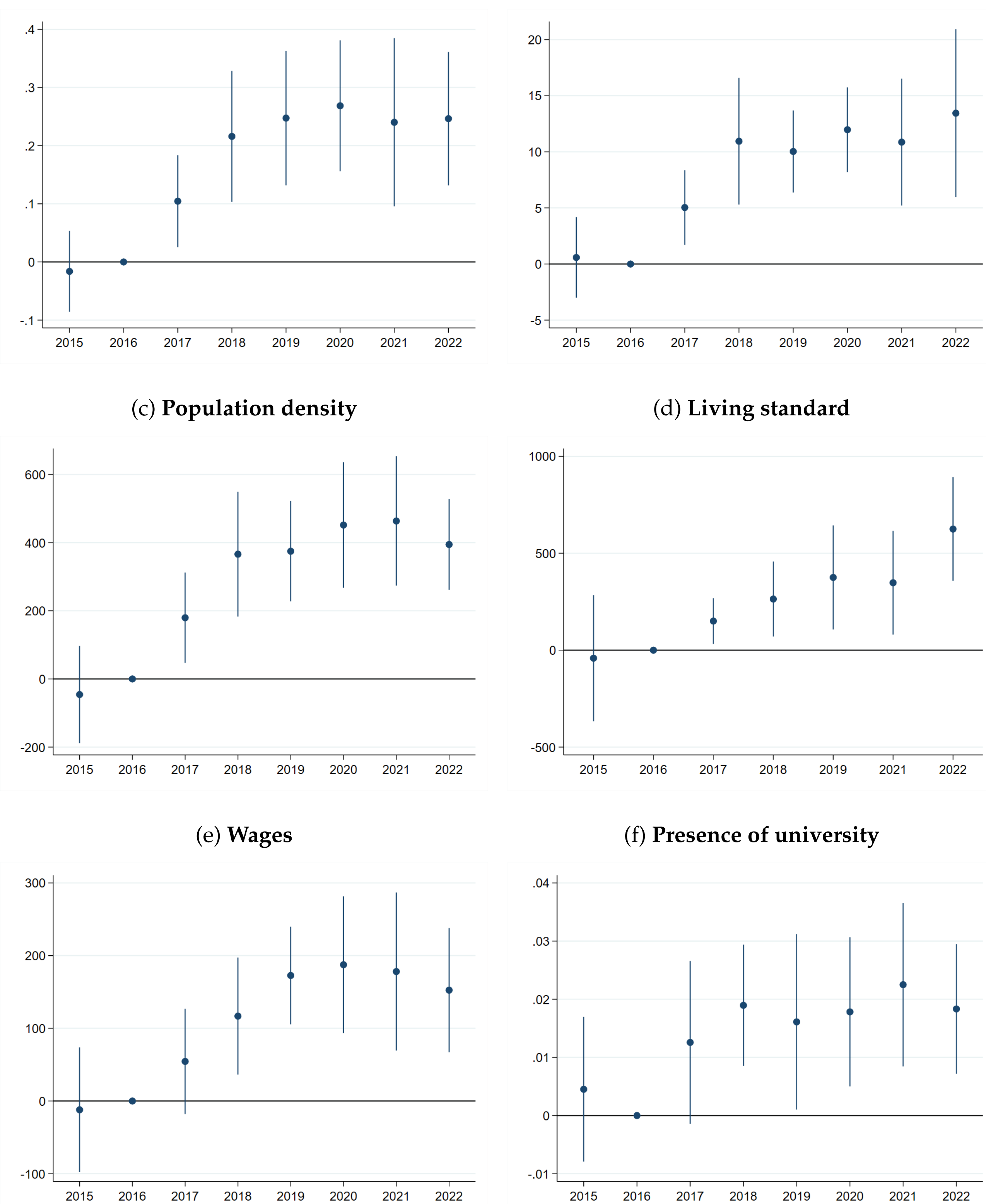
Notes. The figures plot the coefficients on the interaction of being aged less than 31 with year fixed-effects. The sample is restricted to cohorts aged 26 to 35. The outcome variable is an indicator variable for being a single male or female private renter and moving to a new municipality the same year. The top panel plots the yearly interaction coefficients for the full sample, while the bottom panel plots the yearly interaction coefficients separately for the samples of people living in municipalities with no, low, or high *Visale* contract intensity. Standard errors are clustered at the age bin level.

Figure 7: **Effect on selection of movers by income**



Notes. The figures plot the coefficients on the interaction of being aged 30 or less with year fixed-effects, in a regression limited to the sample of private sector renters who are single males or females and changed municipality over the course of the last year. The outcome variable is disposable income. The sample is restricted to cohorts aged 26 to 35. Year 2020 is missing because incomes were not released that year due to the Covid-19 pandemic. Standard errors are clustered at the age bin level.

Figure 8: **Effects on moves to opportunity**



(a) **Rent per meter squared**

(b) **Total rent**

(c) **Population density**

(d) **Living standard**

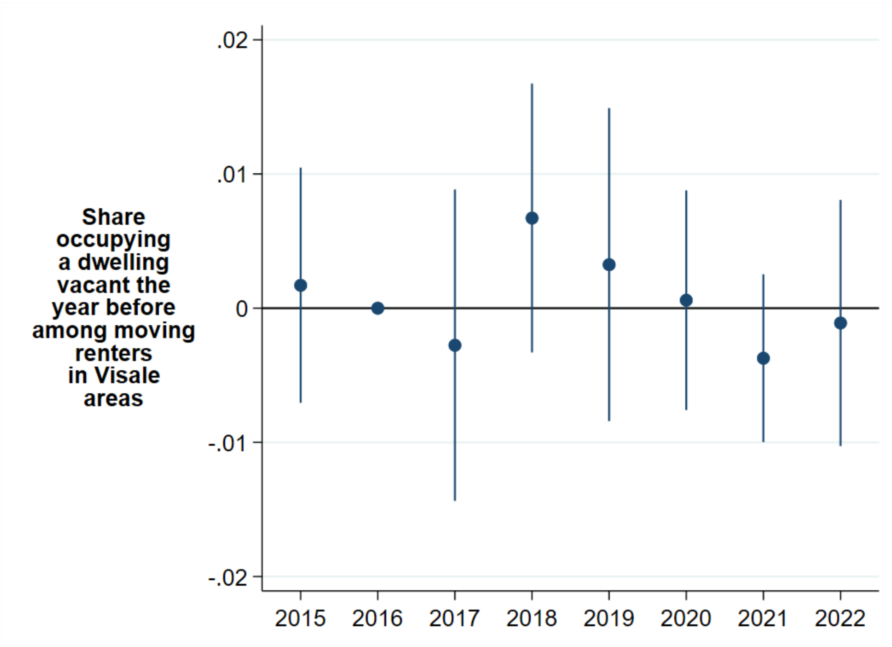
(e) **Wages**

(f) **Presence of university**

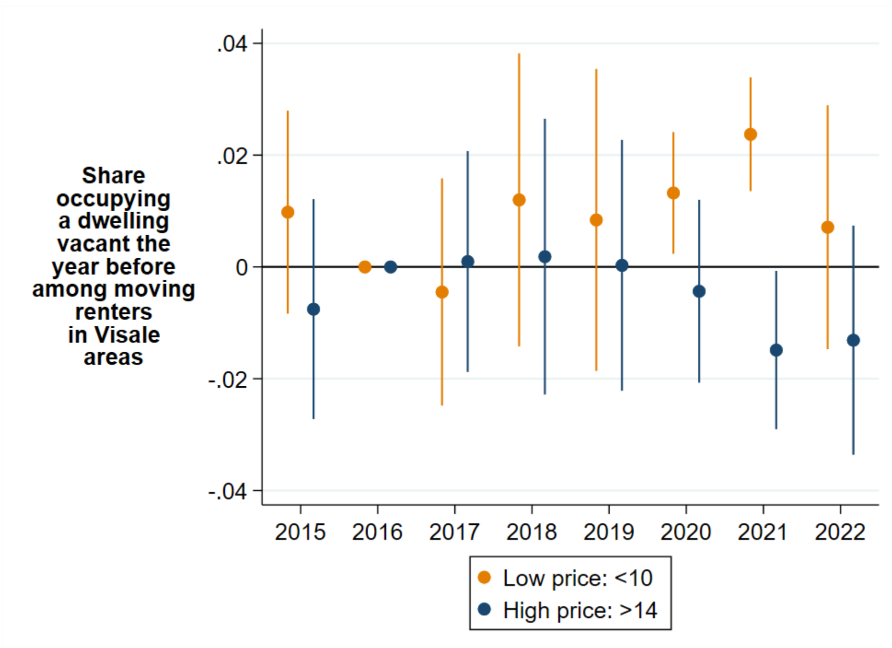
Notes. The figures plot the coefficients on the interaction of being aged 30 or less with year fixed-effects, in a regression limited to the sample of private sector renters who are single males or females and changed municipality over the course of the last year. The sample is restricted to cohorts aged 26 to 35. The outcome variable corresponds to the gap, for the corresponding variable, between the municipality of destination and the municipality of origin. Standard errors are clustered at the age bin level.

Figure 9: **Effect on vacancies**

(a) **Average effect**



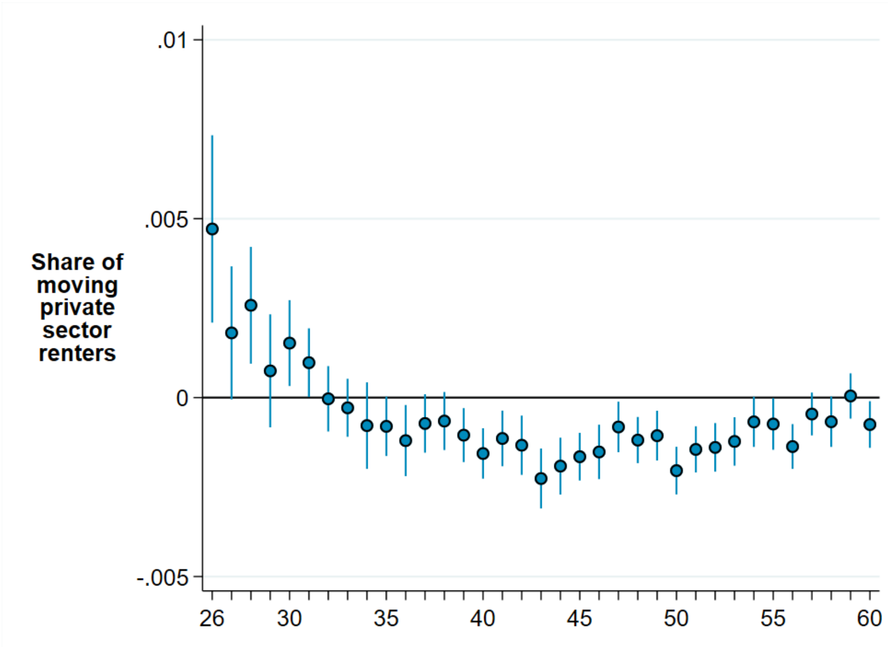
(b) **Heterogeneity by rent per meter squared**



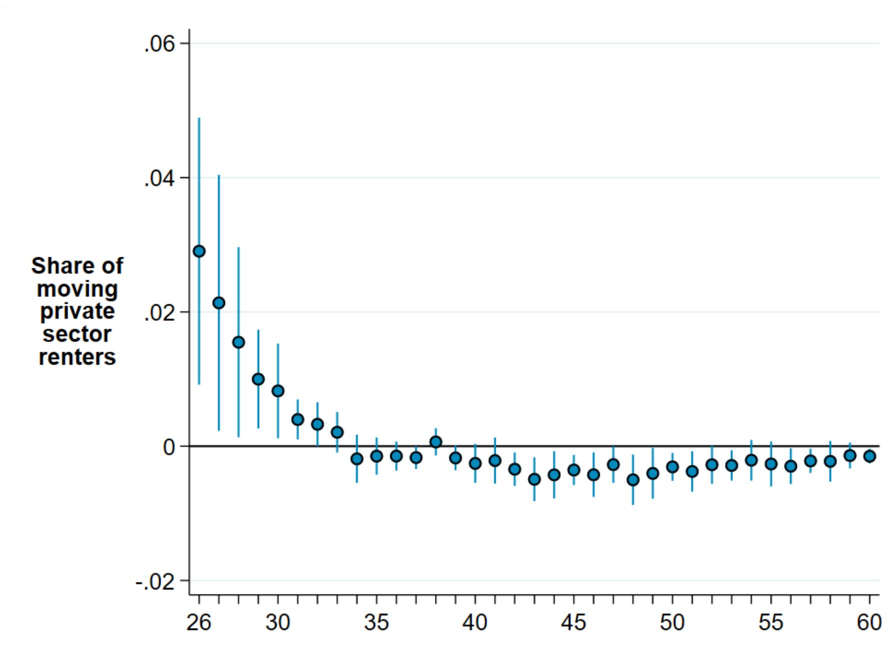
Notes. Panel A plots the coefficients of the interaction of being aged less than 31 with year fixed-effects for the sample of single renters living in municipalities with *Visale* penetration above the median. The outcome variable is an indicator variable for having moved to a dwelling that was vacant the year before. Panel B replicates the same regression for two subgroups: municipalities with rent per meter squared below 10 euros and those above 14 euros. Standard errors are clustered at the age bin level.

Figure 10: **Effect on displacement**

(a) **OLS specification**



(b) **IV specification**



Notes. For both panels, the outcome variable is an indicator variable for being a single male or female private renter moving to municipality m at time t. Panel A plots the interaction coefficients of a post 2016 variable and an indicator variable for the municipality m being above the median in *Visale* penetration. Panel B replicates the same regression instrumenting *Visale* penetration by the share of renters below aged below 31 in the municipality before 2017. Standard errors are clustered at the municipality level.

Main tables

Table 1: Summary statistics

	Population aged between 26 and 35	**Tenants**	**Tenants aged below 30**
Average disposable income	36909	30564	29341
25th percentile	19543	16685	16403
50th percentile	34655	28131	27228
75th percentile	48858	41284	39774
Average age	31	30	28
Share of single males/females without child	18%	30%	34%
Share of single males/females with child(ren)	8%	8%	6%
Share of couples without child	18%	26%	31%
Share of couples with child(ren)	45%	29%	22%
Number of individuals	7570460	2707388	1473835

Notes. Summary statistics for the full population, tenants, and tenants below 30 are from the *FIDELI* data.

Table 2: Non-payment events and Landlord and Unit Characteristics

Dependent variable: Insurance claim	Estimated coefficients (s.e.)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Net monthly income < 5th percentile	0.0538***					0.00863***	0.00512*
	(0.00276)					(0.00296)	(0.00295)
Net monthly income $\geq$ 5th percentile	-0.0341***					-0.0840***	-0.0676***
	(0.00160)					(0.00196)	(0.00197)
Net monthly income < 5th percentile $\times$ Rent-to-income Ratio	-7.76e-05*					-6.22e-05	-1.49e-05
	(4.71e-05)					(4.72e-05)	(4.94e-05)
Net monthly income $\geq$ 5th percentile $\times$ Rent-to-income Ratio	0.132***					0.144***	0.126***
	(0.00452)					(0.00456)	(0.00460)
Student		-0.0238***				-0.0500***	-0.0441***
		(0.000484)				(0.00119)	(0.00118)
Rent per sqm			-0.000762***			-0.000475***	6.44e-05**
			(2.34e-05)			(2.33e-05)	(2.59e-05)
Furnished apartment				-0.0191***		-0.00888***	-0.00461***
				(0.000504)		(0.000534)	(0.000543)
Non-EU foreigner					0.0115***	0.0231***	0.0213***
					(0.000489)	(0.000489)	(0.000494)
EU foreigner					-0.0204***	-0.0107***	-0.00727***
					(0.000763)	(0.000768)	(0.000786)
Constant	0.0513***	0.0709***	0.0707***	0.0688***	0.0529***	0.105***	0.0855***
	(0.000257)	(0.000411)	(0.000534)	(0.000435)	(0.000282)	(0.00127)	(0.00127)
Department FE							✓
Year of beginning of lease FE							✓
Department FE $\times$ Year of beginning of lease FE							✓
Observations	1,070,534	1,070,534	1,070,532	1,070,534	1,070,502	1,070,500	1,054,365
R-squared	0.002	0.002	0.001	0.002	0.001	0.008	0.028

Notes: The table presents the estimated coefficients from regressing an indicator for the binary event of an insurance claim occurring on a series of landlord and tenant characteristics. Contracts are limited to those started between 2016 and 2023, inclusive, and signed by a tenant without roommates, which make up about 87% of all contracts. Tenants fall into three mutually exclusive categories "No monthly income", "Net monthly income < 5th percentile", and "Net monthly income $\geq$ 5th percentile". In the regressions, the group with no monthly income is the benchmark category. Robust standard errors are included in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Private renter shares

	Private sector renters share				
	All municipalities	Municipalities no Visale contracts	Municipalities share Visale below median	Municipalities share Visale above median	Triple Difference
	(1)	(2)	(3)	(4)	
Post*Treated	0.009***	-0.000	0.005***	0.012***	
	(0.001)	(0.001)	(0.001)	(0.002)	
					0.006***
					(0.001)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	62058699	4432693	28815232	28810774	62058680

*Signif. Codes: ***: 0.01, **:0.05, *:0.1*
Standard errors clustered by age

Notes. Columns 1 to 4 display the double difference coefficients, successively for: all municipalities (column 1), municipalities with no Visale contract (column 2), with the share of Visale contracts being below the median (column 3) and above the median (column 4). Column 5 displays the triple difference coefficient: the interaction between being aged more than 30 years old, after 2016 and living in a municipality where the share of Visale contracts is above the median. The outcome variable is an indicator for being a single male or female renter in the private sector. The sample is restricted to individuals aged between 26 and 35 years old. The unit of observation is at the individual level.

Table 4: Average gaps for movers

	Average gap at the municipality level for movers					
	Rent per meter squared	Total rent	Population density	Living standard	Wages	Presence of a university
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treated	0.195***	9.482***	351.076***	387.704***	116.649***	0.013***
	(0.039)	(1.616)	(61.116)	(80.486)	(28.305)	(0.003)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1219515	1219515	1219515	1030446	1219515	1219515

*Signif. Codes: ***: 0.01, **:0.05, *:0.1*
Standard errors clustered by age

Notes. The table plots the coefficients on the interaction of being aged 30 or less with a post-*Visale* time period indicator, in a regression limited to the sample of private sector renters who are single males or females and changed municipality over the course of the last year. The outcome variable in each column is the gap in the corresponding variable between the municipality of destination and the municipality of origin. The sample is restricted to cohorts aged 26 to 35. In column 4, year 2020 is missing because incomes were not released that year due to the Covid-19 pandemic. The unit of observation is at the individual level.

Table 5: Income of movers

	Individual income	Individual income over municipality income	Individual income over total rent
	(1)	(2)	(3)
Post*Treated	-305.100***	-0.007***	-0.600***
	(80.020)	(0.001)	(0.070)
Year fixed effects	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes
Observations	1030446	1030446	1030446

*Signif. Codes: ***: 0.01, **:0.05, *:0.1*
Standard errors clustered by age

Notes. The table plots the coefficients on the interaction of being aged 30 or less with a post-*Visale* time period indicator, in a regression limited to the sample of private sector renters who are single males or females and changed municipality over the course of the last year. The outcome variable is the individual income of the household (1), or the ratio of individual incomes to municipality-level average incomes (2) or average rents (3). The sample is restricted to cohorts aged 26 to 35. Year 2020 is missing because incomes were not released that year due to the Covid-19 pandemic. The unit of observation is at the individual level.

Table 6: Effect on displacement - overlap index

	Private renter share	Private renter share moving to a new municipality		
	(1)	(2)	(3)	(4)
Index*Post	-0.018	-0.007***	-0.007***	-0.006***
	(0.011)	(0.001)	(0.002)	(0.001)
Index*Post*Visale above median			0.001	
			(0.002)	
Index*Post*Visale above 90th percentile				-0.004***
				(0.001)
Year fixed effects	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes
Observations	3877620	3877620	3877620	3877620

*Signif. Codes: ***: 0.01, **:0.05, *:0.1*
Standard errors clustered by household category

Notes. In columns 1 and 2, the Table displays the regression coefficients of the interaction between an indicator variable for years post 2016 and the value of the overlap index. The outcome is the share of private renters in column 1 and the share of renters moving to a new municipality in column 2. Columns 3 and 4 correspond to a triple difference specification where we add a coefficient for being in a high *Visale* penetration area, defined as above the median (column 3) and above the 90th percentile (column 4) in the share of *Visale* contracts with respect to the total population. The unit of observation is year $\times$ municipality $\times$ household category as defined in Table D.4.

Appendices

A Asymmetric information in rental insurance

We provide suggestive evidence that the non-payment guarantee displays modest amounts of moral hazard and adverse selection, common in insurance markets. If either higher-risk than average tenants select into availing themselves of the insurance scheme, or if coverage by the scheme modifies behavior by tenants or landlords, then the fiscal cost of the scheme may be high relative to a baseline case of no selection and no moral hazard effects.

Adverse selection We provide two suggestive pieces of evidence suggesting that the scheme may face a form of adverse selection. First, as mentioned *supra*, non-payment rates under *Visale* hover close to 7%, versus 4 to 5% in aggregate in the French rental sector, suggesting a slightly higher risk profile of *Visale*-sponsored tenants (although this could be entirely attributable to observable characteristics). Since the scheme is free, insured and non-insured but eligible households face the same "price" for the scheme, and higher hazard rates for the insured population suggests a "positive correlation" between insurance status and claim probability, a test that Einav and Finkelstein (2011) propose for adverse selection.
Second, non-payment claim rates decline steadily over time, as the scheme becomes more prevalent in France. Figure C.6 displays the time profile of non-payment events by year of contract start, and shows that the likelihood of non-payment after any elapsed time was higher in earlier phases of the scheme. Figure C.7 shows that the monthly rate at which currently active contracts exhibit a trigger event has been steadily declining since 2016. We take this decline as suggestive evidence of adverse selection into the *Visale* guarantee initially. Indeed, if the highest-risk renters were initially the most likely to seek or receive information on – and take advantage of – the relatively un-publicized free insurance scheme, its gradual progress to cover a increasingly large share of the eligible population leads to a decline in the hazard rate and average cost per contract.

Moral hazard Additionally, we find some evidence that households distort behavior (a form of moral hazard) in response to the presence of the *Visale* guarantee. We show that landlords distort posted rents to match *Visale* eligibility criteria described above for both maximum rents and maximum rent-to-income ratios. First, we provide evidence of exact bunching at the maximum 50% rent-to-income ratio of the *Visale* scheme, implying that landlords and tenants target the maximum rent covered under the scheme. Figure C.8a shows that the distribution of rent-to-income ratios for single renters with strictly positive income exhibits sharp bunching at the 50% mark.[19] This suggests that either tenants are distorting their directed search process towards units with posted rents strictly below the maximum rent allowed by the *Visale* scheme for their stated income; landlords are distorting rent downwards to ensure eligibility to the scheme once they have found a tenant; or incomes and/or rents are mis-reported to the scheme to make sure a given lease contract is insured. Consistent with the hypothesis that some landlords are distorting rent downwards to make sure the contract is covered under *Visale*, figure C.8b shows that rents (inclusive of utilities) are discretely lower immediately below the threshold.

Students (who do not have taxable income) are not subject to the rent-to-income ceiling, but rather to a flat maximum utilities-inclusive rent standard (of EUR 600 outside the Paris region, and EUR 800 inside). Figure C.9a shows that in their case, the distribution of rents for students exhibits bunching at the EUR 600 mark, likely driven by the additional benefit of eligibility to the insurance scheme. Moreover, figure C.9b shows that utilities are discretely lower exactly at the EUR 600 mark, suggesting that landlords and tenants misreport or distort the level of utilities downwards in order to match the maximum rent eligibility criterion to the *Visale* guarantee.

[19]The presence of some households above the maximum rent-to-income ratio or above the maximum rent in the data may indicate a fuzzy application of the maximum rent burden criterion; or mis-measurement of rents and/or incomes in the *Visale* data.

B Predicting renter default with non-parametric models

We run prediction models on two datasets in this exercise: one with imputed income values for tenants with missing income, and one with only tenants with the income field populated. We then split the sample into training and test sets (70% and 30% of the entire sample, respectively). We train linear logit, non-linear logit, and random forest models on the training data, and assess the accuracy of the models on the test data. Following **fuster˙predictably˙2022**, we compute three statistics (1) AUC, (2) average precision score, and (3) goodness of fit R^2.

The set of predictors include those in table B.1.

Table B.1: Variables used for training

Linear logit	Non-linear logit
Tenant's [imputed] income (linear)	Tenant's [imputed] income (€500 bins from 0-€5000) *(A separate bin for income >€5000)*
Rent-to-income (linear)	Rent-to-income (20-point bins from 0-1) *(A separate bin for ratio >1)*
A dummy for missing income (only when the imputed data is used)	
Common variables	
Rent per Sqm (Linear)	
Tenant is student (dummy)	
No co-tenants (dummy)	
Age of tenant at beginning of contract (Linear)	
Furnished apartment (dummy)	
Non-EU foreigner (dummy)	
EU foreigner (dummy)	
Department (97 dummies)	
Year beginning contract (8 dummies)	

Income imputation Since incomes are missing for many individuals (notably students) in the sample, income is imputed using an iterative imputation approach (regularized linear regression). Imputed values make up about 63% percent of the entire sample. In the classification problem, we use two samples, one with both imputed and non-missing income and the other with only non-missing incomes. In the non-linear logistic model, tenant's income is split up into €500 bins between 0-€5000, while income beyond that is lumped together in one bin. Similarly, rent-to-income is split into five 20-point bins between [0,1] and a separate bin for ratios greater than 1.

Tuning hyper-parameters using cross-validation To find the best set of hyper-parameters used in the random forest estimation, we tune two parameters: the minimum number of observations required to split at a node and the minimum number of observations on each leaf. We also set the number of trees used in the model at 500. Lastly, following **fuster˙predictably˙2022**, we do not impose any restriction on the maximum depth of each tree. Given this tuning setup, we define the grid as follows: for the minimum number of observations required for a split, increments of 50 from 2 to 500 (i.e., 2, 50, 100,..., 500) and for the minimum number of data points on each leaf, increments of 25 from 1 to 250 (i.e., 1, 25, 50,..., 250). Therefore, in total, 11×11 sets of parameters are cross-validated.

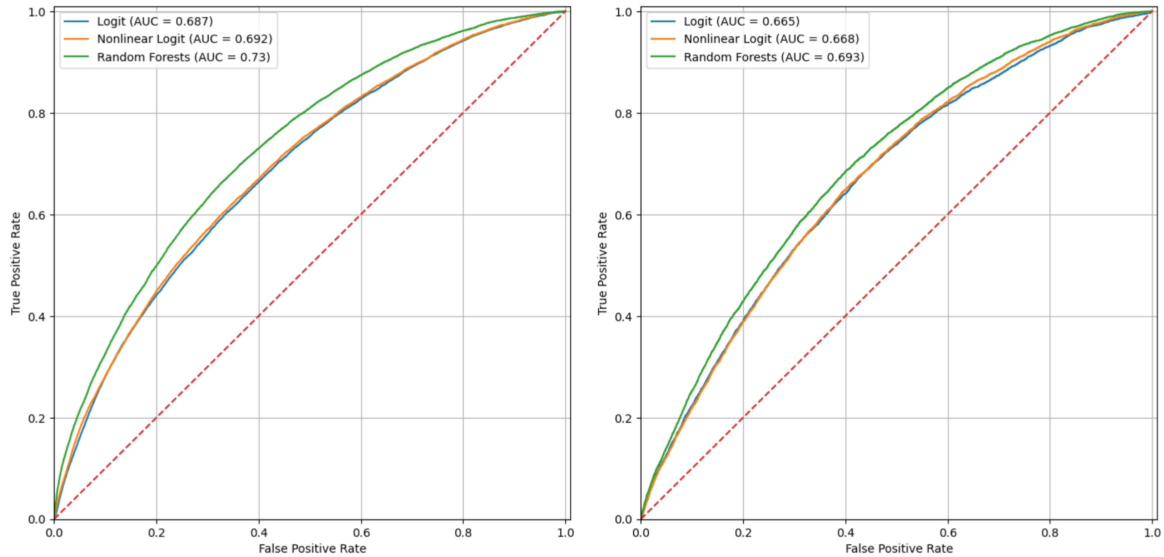
We then cross-validate the choice of parameters using a 5-fold cross-validation[20] on the data set with imputed income. The best minimum number of observations required to split is 50, and the best minimum number of observations required on each leaf is 25, regardless of the scoring metric used (average precision score or ROC-AUC). We then use this set of hyper-parameters for the main prediction exercise.

Model predictive performance We compute the AUC of the receiver-operator curve (ROC) of each model as a measure of its predictive accuracy. The ROC curve displays the trade-off between the true positive and false positive rates using different probability thresholds for classifying default predictions. The area under the ROC curve (AUC) summarizes the ability of the model to assign a higher probability to a real ”default” event without increasing the probability for ”no default” events across probability thresholds.

Figure B.1 and tables B.2 and B.3 (respectively for the sample with income imputation and the one with non-missing incomes) summarize the performance assessment for each of the models used. Random forest performs better than non-linear logit, which does better than linear logit. This progression is evidenced through higher AUC, precision score, and goodness of fit.

[20] As discussed in **fuster˙predictably˙2022**, the choice of the number of folds involves a trade-off between computational speed and variance. A larger number of folds implies less variance in our estimates of model fit. Our random forest model is computationally costly with more than a million observations and the number of trees set at 500. Therefore, we use $K = 5$ folds in the cross-validation exercise ($K = 3$ in **fuster˙predictably˙2022**).

Figure B.1: ROC curve for all approaches



Note: The figure plots the ROC curve for each statistical learning approach. The left-hand side plot displays the results when each learning model is used on the entire sample with imputed income, while the right-hand side one displays the results for the sample with only non-missing income. For random forest, I set the minimum number of observations required to split an a node at 50, and the number of observations on each leaf to be 25. I also set the number of trees used in the model at 500.

Table B.2: Comparative performance (imputed income $N = 1,211,996$)

	AUC	Precision score	R^2
Logit	0.687	0.0847	0.0193
Non-linear Logit	0.692	0.0897	0.0218
random forest	0.730	0.119	0.0372

Note: This table summarizes the performance assessment for each of the learning model used on the entire sample with imputed tenant's income. For random forest, I set the minimum number of observations required to split an a node at 50, and the number of observations on each leaf to be 25. I also set the number of trees used in the model at 500. AUC is the area under the ROC curve, and a higher AUC signals better performance. Precision corresponds to the probability of a classification being positive given a positive prediction $Pr(y = 1|\hat{y} = 1)$. The *average* precision score calculates the mean of precision, weighted by the corresponding trade-off in recall. R^2 is calculated as one minus the sum of squared residuals under the model, scaled by the sum of squared residuals from using the simple mean.

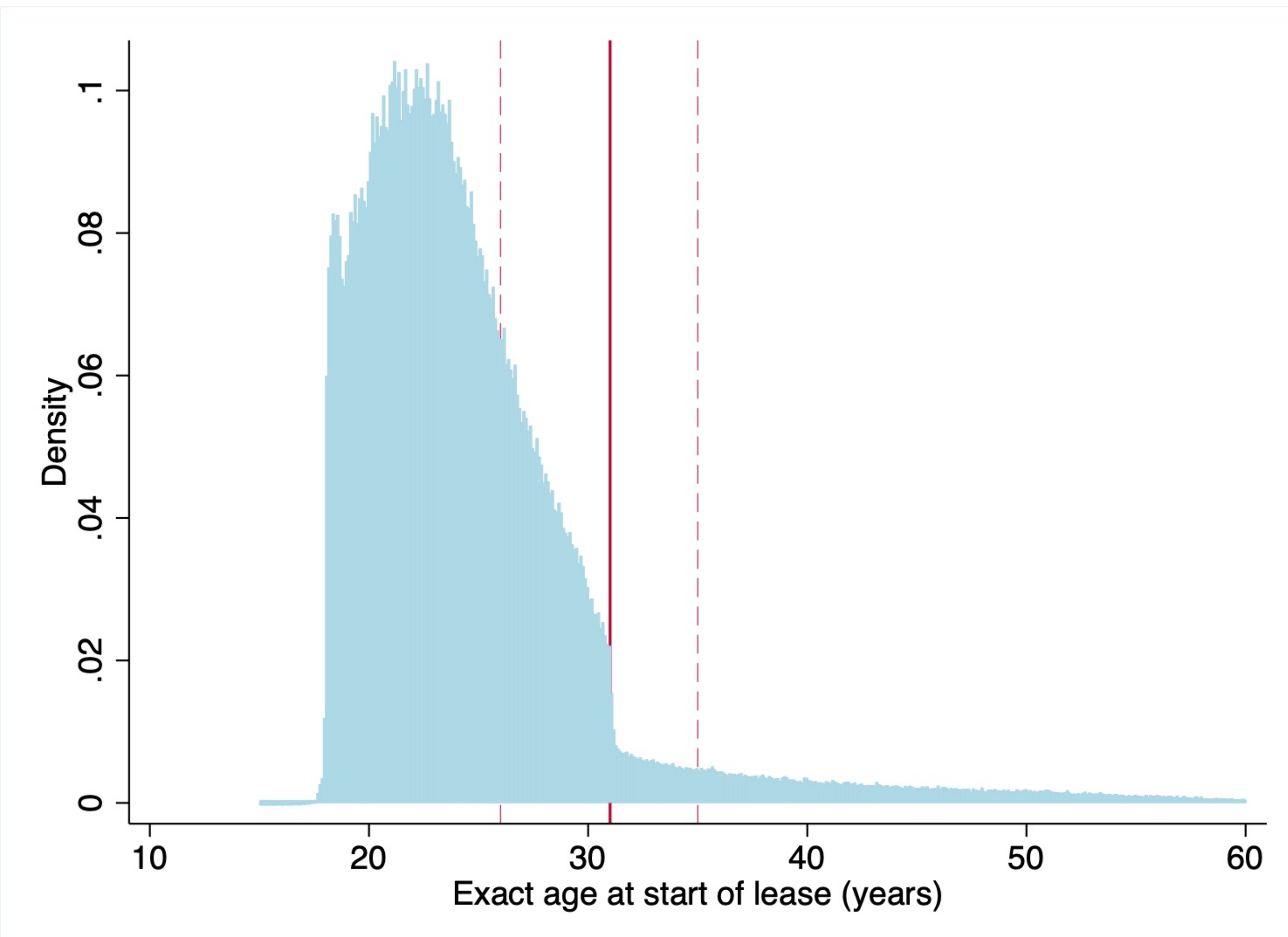
Table B.3: Comparative performance (non-missing income only $N = 447,881$)

	AUC	Precision score	R^2
Logit	0.665	0.0755	0.0139
Non-linear Logit	0.668	0.0751	0.0140
random forest	0.693	0.0866	0.0197

Note: This table summarizes the performance assessment for each of the learning model used on the sample with only tenants with non-zero income. For random forest, I set the minimum number of observations required to split an a node at 50, and the number of observations on each leaf to be 25. I also set the number of trees used in the model at 500. AUC is the area under the ROC curve, and a higher AUC signals better performance. Precision corresponds to the probability of a classification being positive given a positive prediction $Pr(y = 1|\hat{y} = 1)$. The *average* precision score calculates the mean of precision, weighted by the corresponding trade-off in recall. R^2 is calculated as one minus the sum of squared residuals under the model, scaled by the sum of squared residuals from using the simple mean.

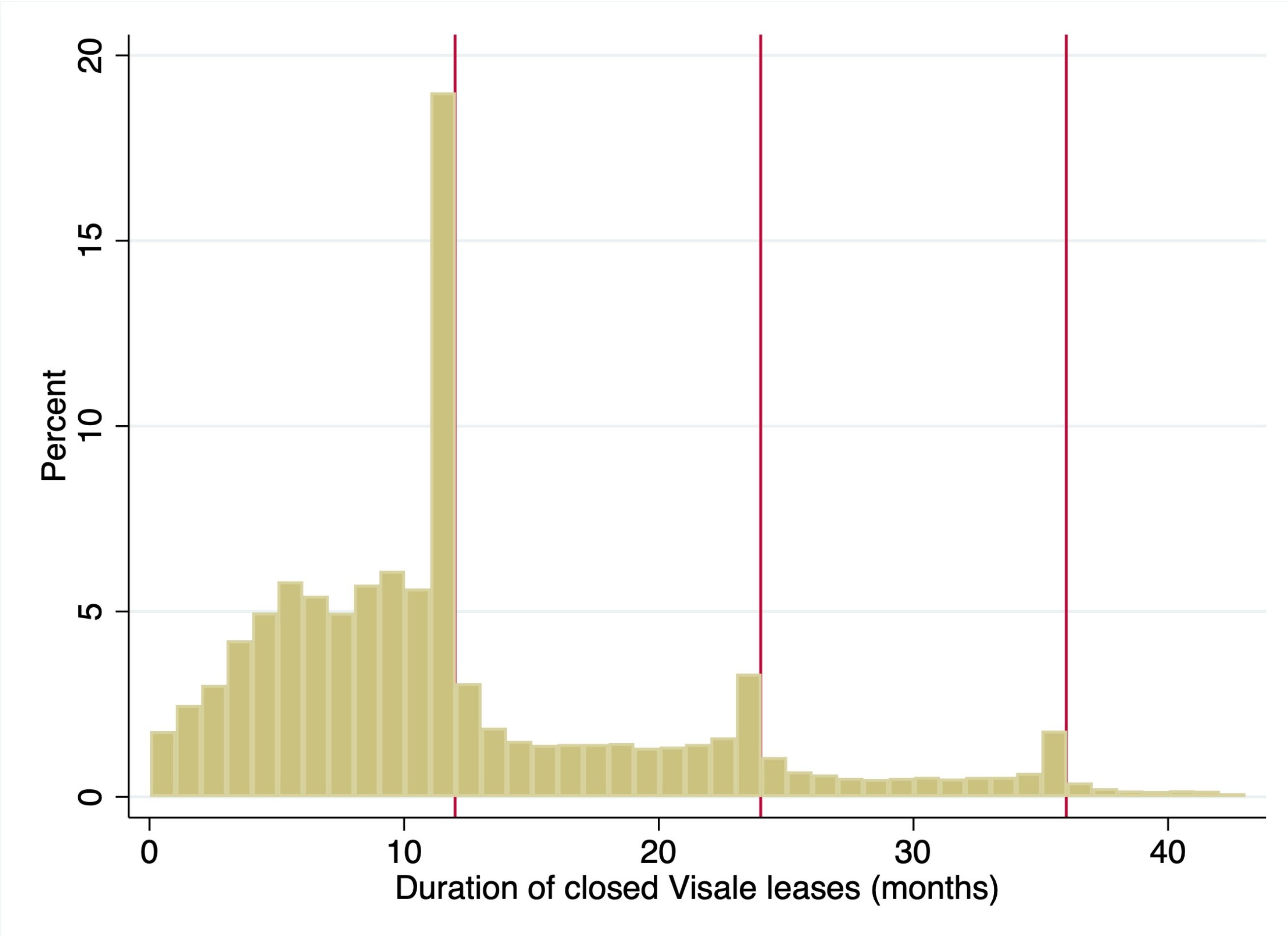
C Additional figures

Figure C.1: **Visale beneficiaries: age distribution**



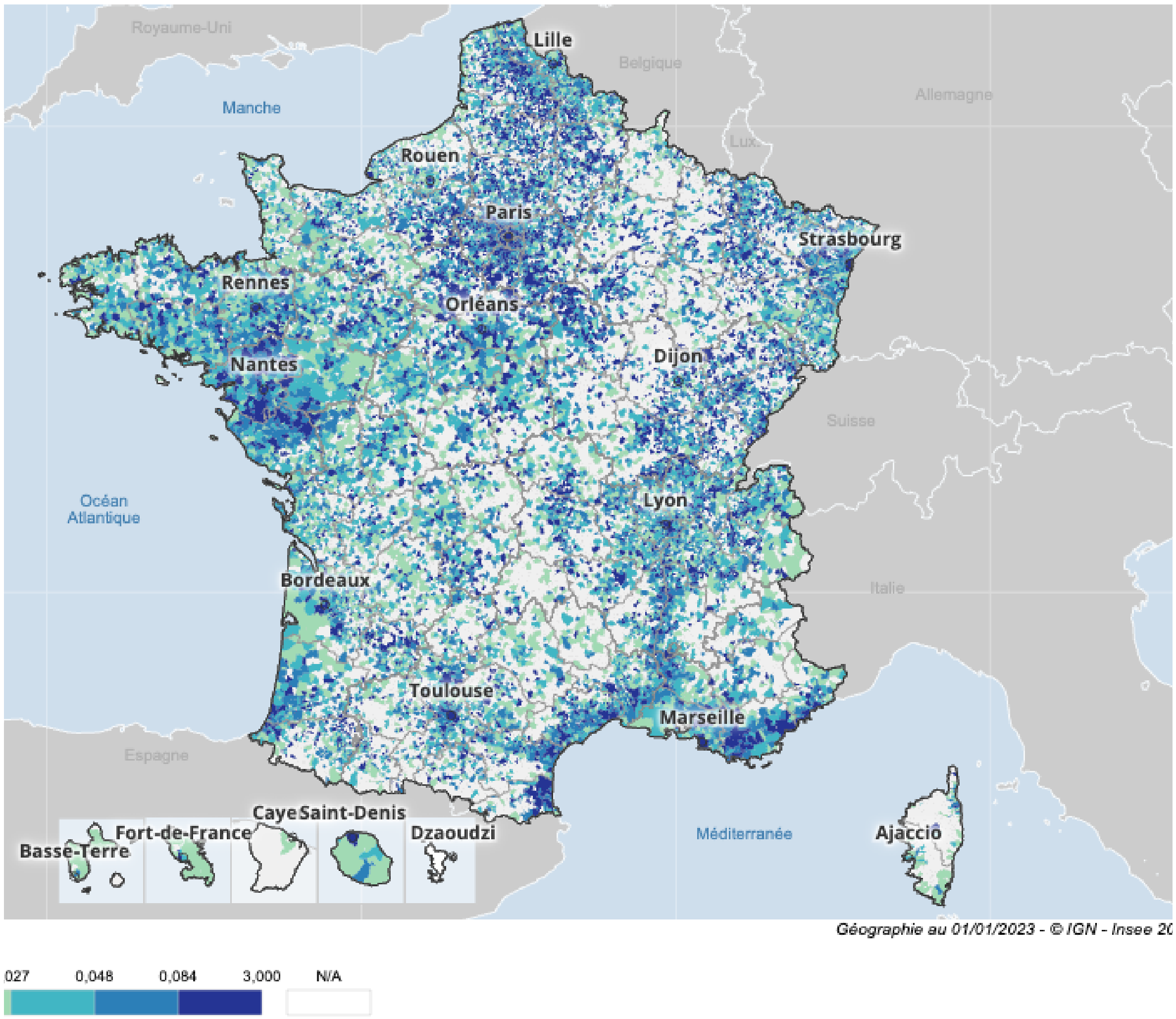
Notes. The figure displays the distribution of exact ages at the start of their lease for all *Visale* beneficiaries from 2016 to 2023, computed as the difference (in days) between the date of birth and the start of the lease. The vertical line marks the renter's 31st birthday. The vertical dashed lines at 25 and 35 correspond to the window used for most event studies in section 5.

Figure C.2: **Visale beneficiaries: duration of (finished) leases**



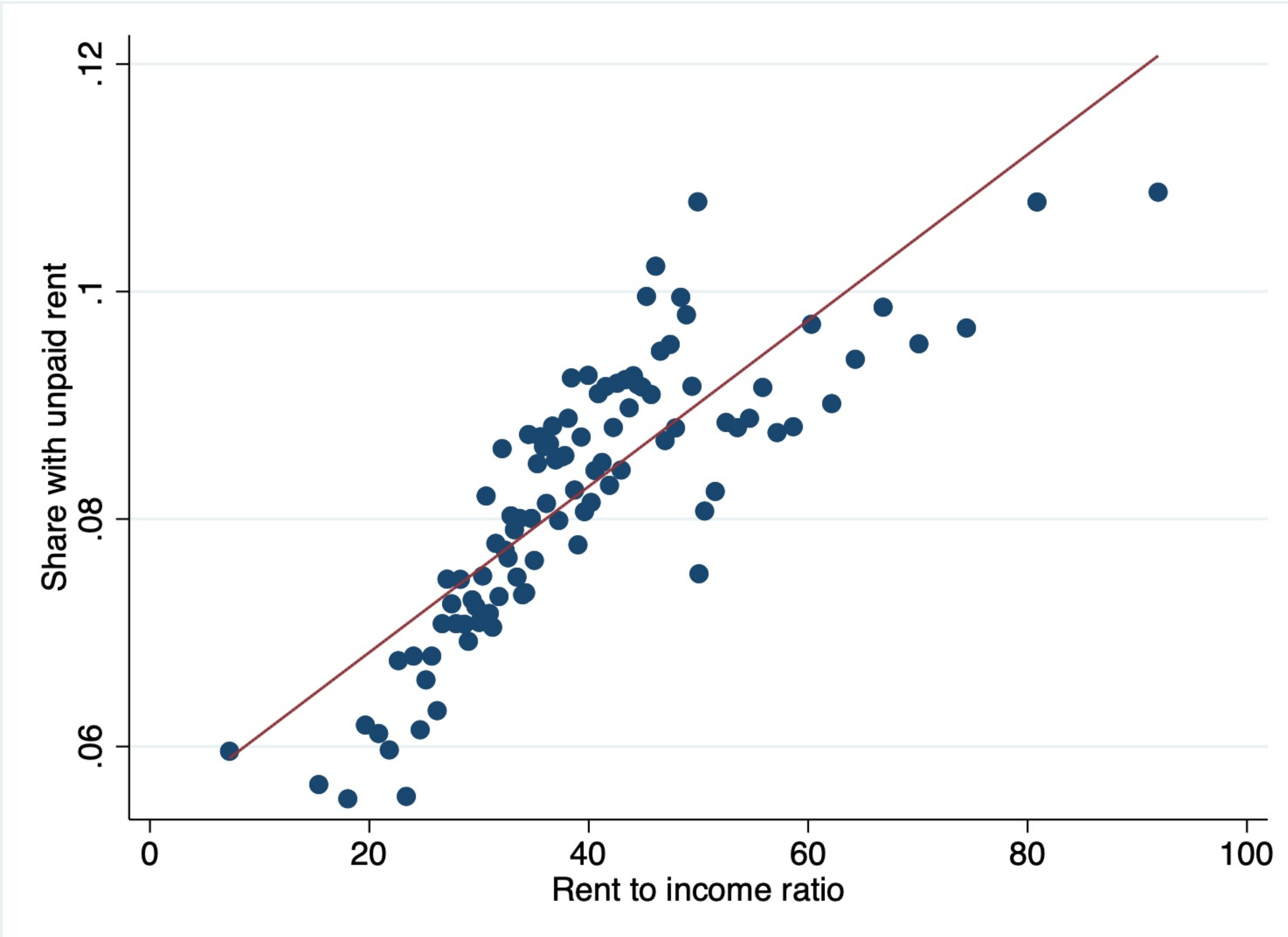
Notes. The figure displays the distribution of lease duration for all *Visale* closed leases from 2016 to 2023, computed as the difference (in days) between the start and end of the lease. The vertical lines mark the one year, two years, and three years durations, which are common in rental contracts.

Figure C.3: **Visale beneficiaries: treatment intensity by ZIP code**



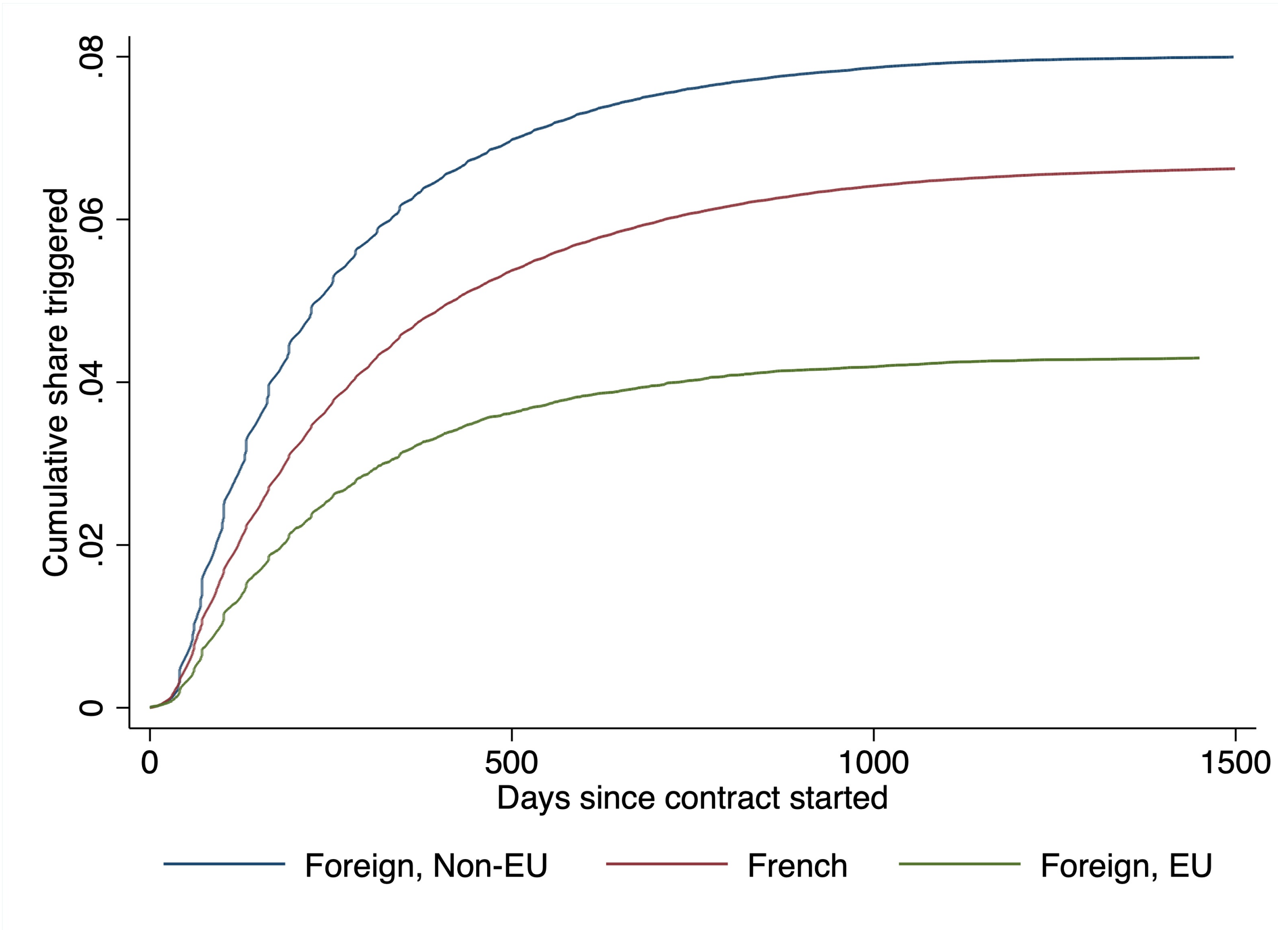
Notes. The figure displays the total number of Visale contracts signed for units in a ZIP code over the period 2016-2023, normalized by the total number of rental units in the municipality. White areas indicate ZIP codes with no *Visale* contracts.

Figure C.4: **Unpaid rent versus effort rate in the *Visale* data**



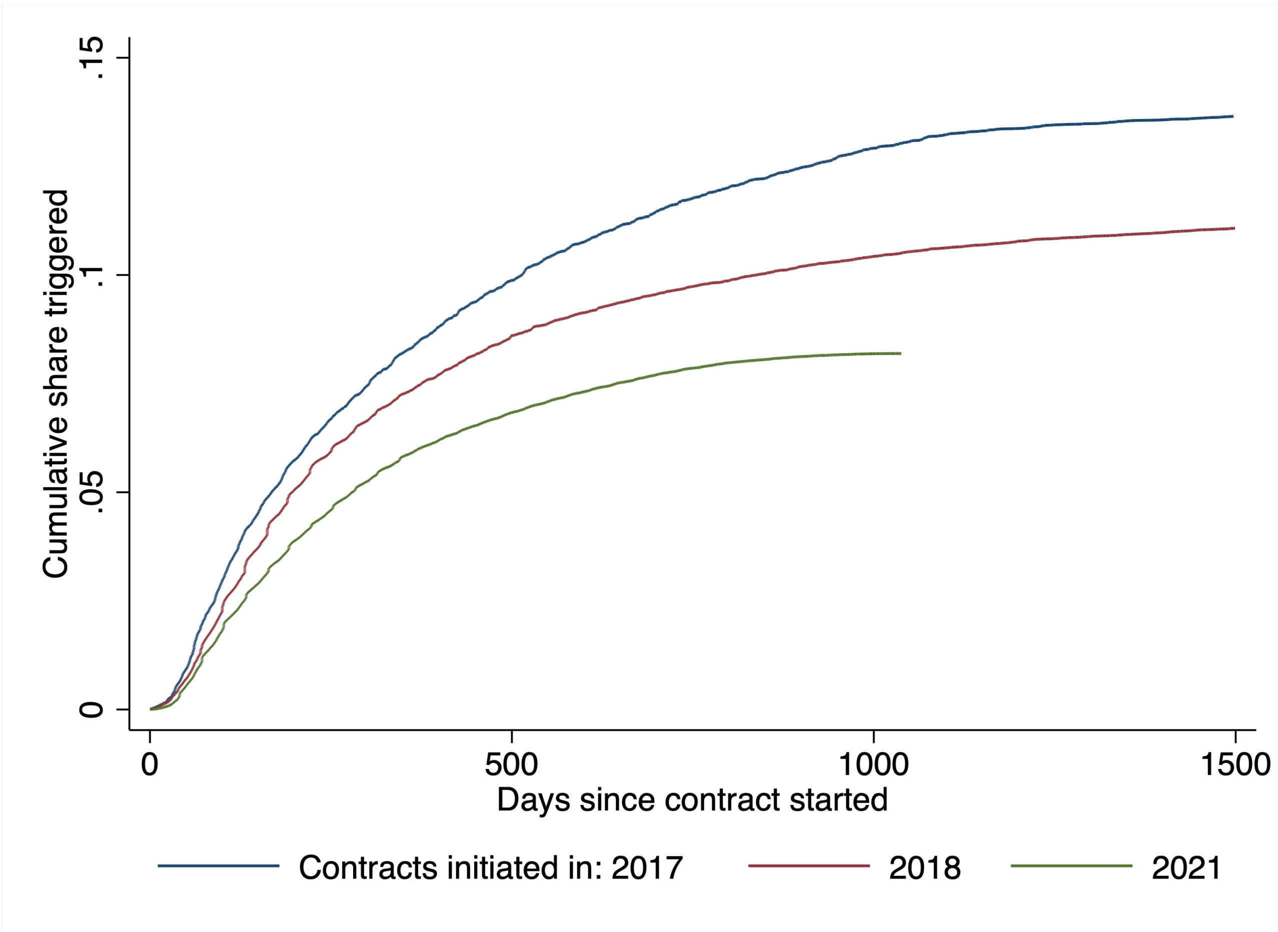
Notes. The figure displays a binned scatter plot of the probability that a *Visale* contract experiences any trigger of the guarantee after non-payment against the initial rent-to-income ratio at the start of the rental agreement. The data only incorporates contracts where tenants have strictly positive income.

Figure C.5: **Unpaid rent over time *Visale* data: by citizenship**



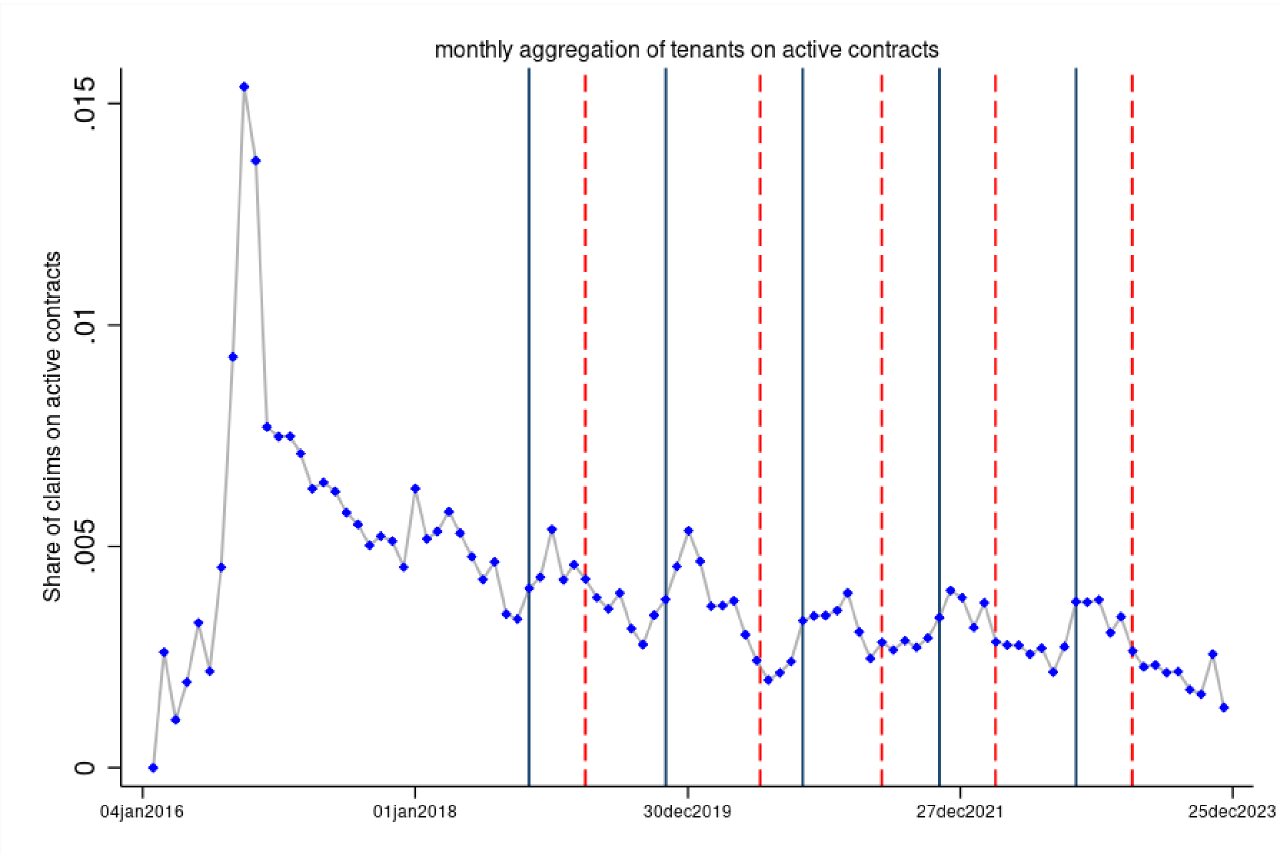
Notes. The figure displays the cumulative probability that a *Visale* contract experiences any trigger of the guarantee after a non-payment event against time elapsed (in days) since the start of the rental agreement. It splits the contracts between foreign citizens from countries within the European Union, foreign citizens from outside the EU, and French citizens.

Figure C.6: **Unpaid rent over time *Visale* data: by start year**



Notes. The figure displays the cumulative probability that a *Visale* contract experiences any trigger of the guarantee after a non-payment event against time elapsed (in days) since the start of the rental agreement. It splits the contracts by year when they started.

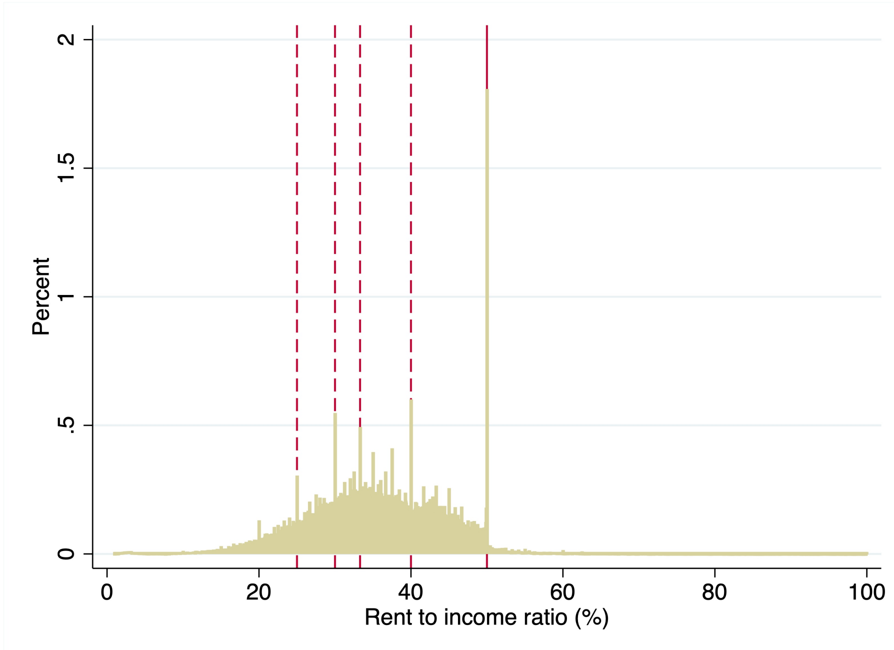
Figure C.7: **Unpaid rent over time *Visale* data: monthly share**



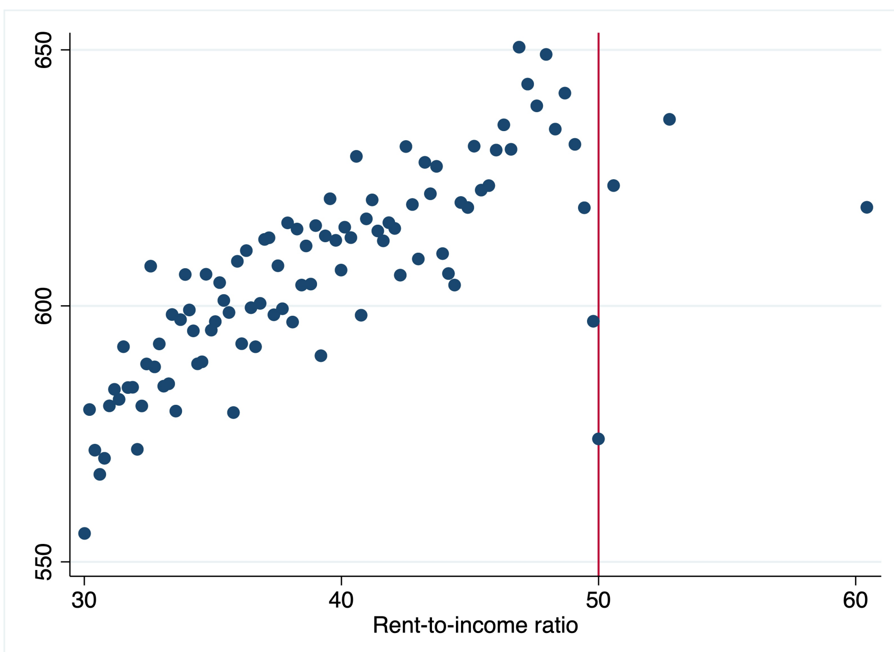
Notes. The figure displays the monthly rate at which currently active *Visale* contracts experience a trigger of the guarantee after a non-payment event, from 2016 to 2023.

Figure C.8: **Rent-to-income in the *Visale* data**

(a) **Rent-to-income distribution**



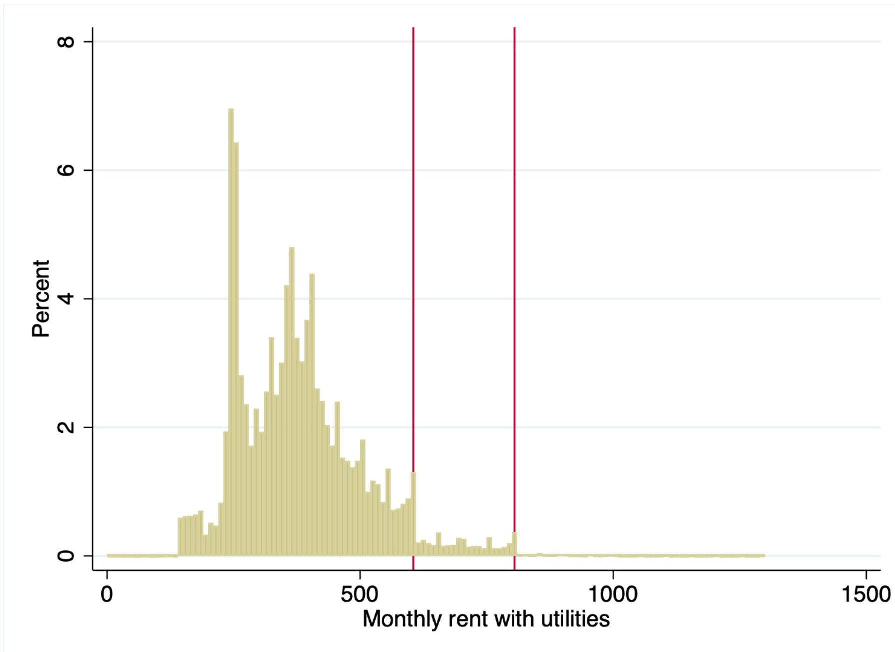
(b) **Rents versus rent-to-income**



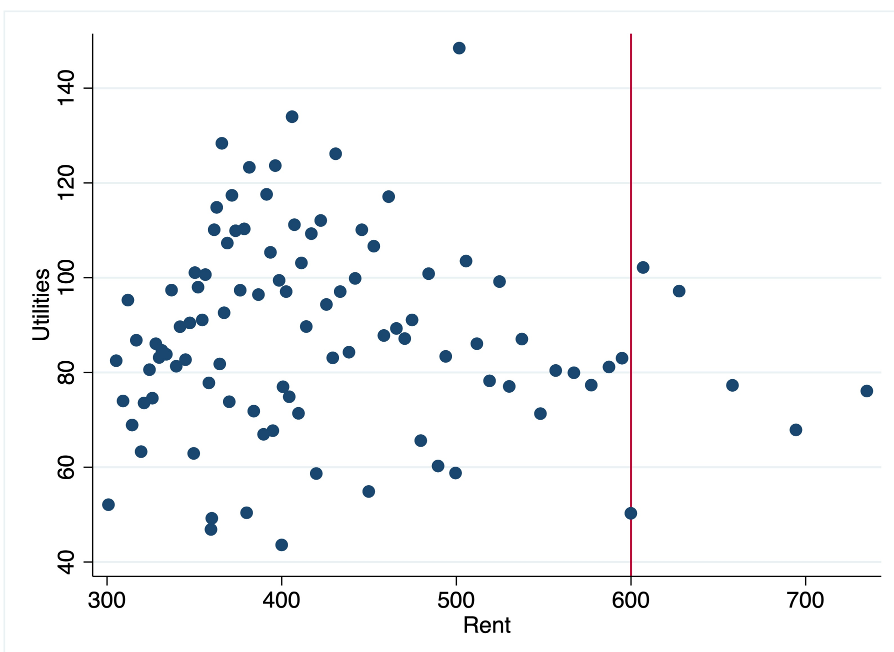
Notes. Panel C.8a displays the distribution of rents (monthly and inclusive of utilities) relative to incomes in the *Visale* contracts for single tenants with strictly positive income. The solid vertical line indicates the flat maximum rent-to-income ceiling of 50% under which a rental contract is eligible to *Visale*. The dashed vertical lines indicate other standard rent burden ratios commonly required by landlords in France (25%, 30%, 33.3%, 40%). Figure C.8b displays the average amount of rents (inclusive of utilities) by bins of rent-to-income ratios in the *Visale* contracts for single renters with strictly positive income.

Figure C.9: **Rents for students in the *Visale* data**

(a) **Rent distribution**



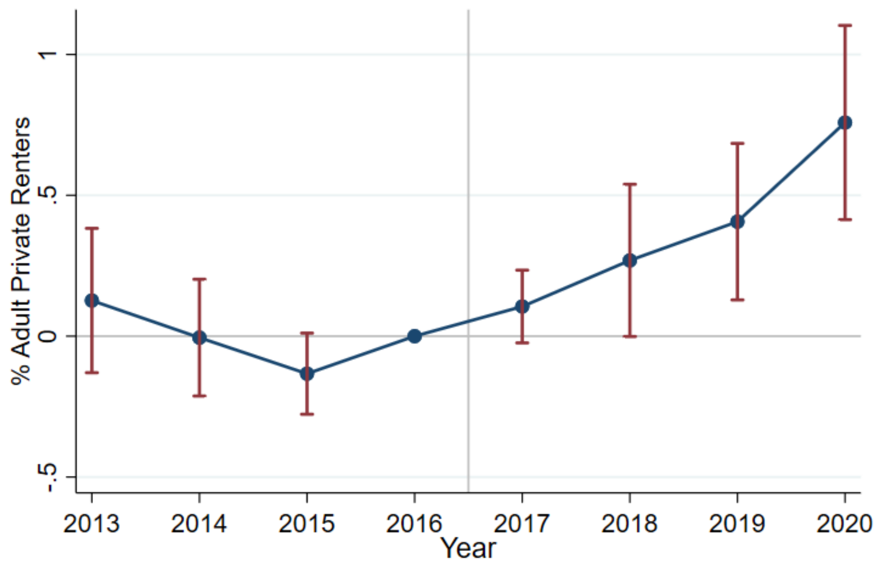
(b) **Utilities versus rents**



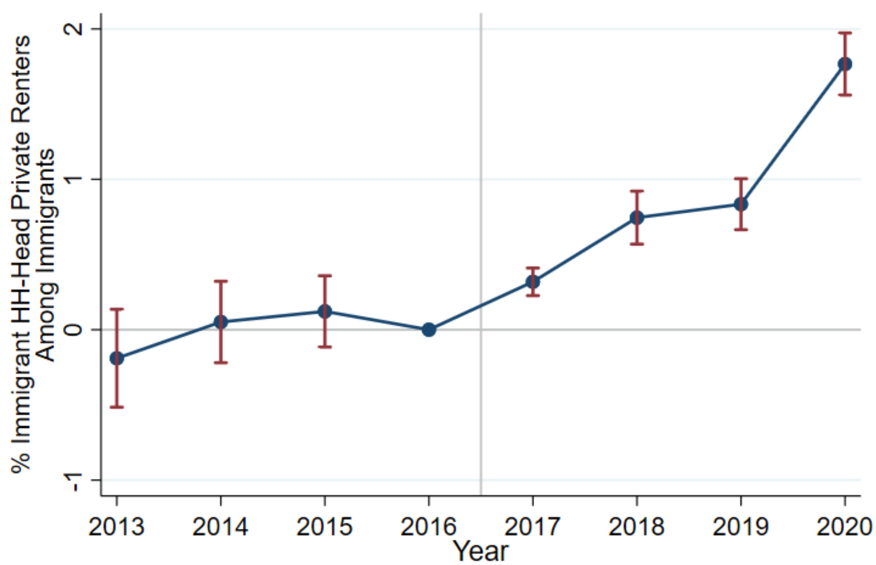
Notes. Panel C.9a displays the distribution of rents (monthly and inclusive of utilities) in the *Visale* contracts for students only. The two vertical lines indicate the flat maximum rent level under which a student-tenant's rental contract is eligible to *Visale*, inside the Paris region (EUR 800) and outside the Paris region (EUR 600). Panel C.9b displays the average amount of utilities across the distribution of rents (monthly and inclusive of utilities) in the *Visale* contracts for students only.

Figure C.10: **Private renter share, by age, overall and immigrants**

(a) **Overall**

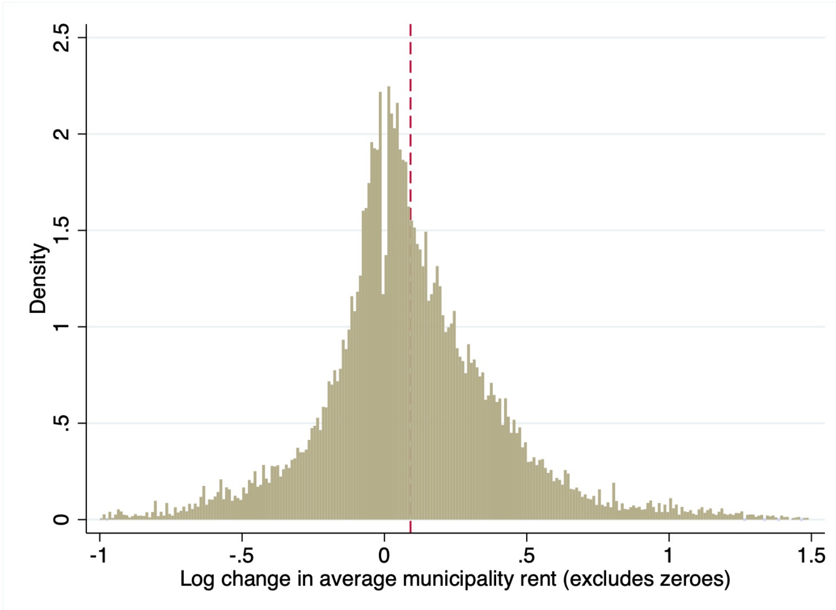


(b) **Immigrants**



Notes. The figure displays the coefficients on the interaction between a dummy for age being below 30 and year fixed effects, in a regression where the outcome is the share of individuals in an age bin who live in the private rental sector and are the person of reference of their household. The x-axis corresponds to age of the individual in the Census. Panel C.10a is for the overall population, while panel C.10b focuses on the sub-samples of households where the head of household is an immigrant.

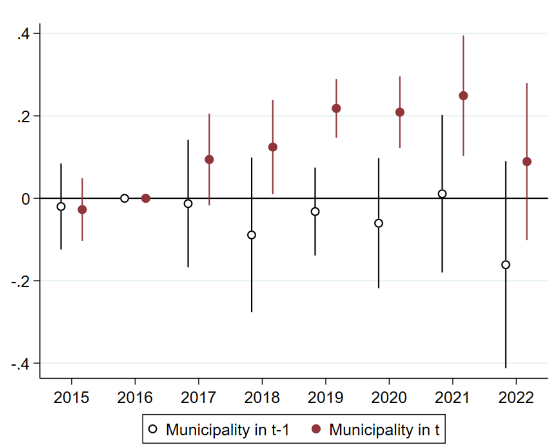
Figure C.11: **Visale movers: change in municipality-level renters**



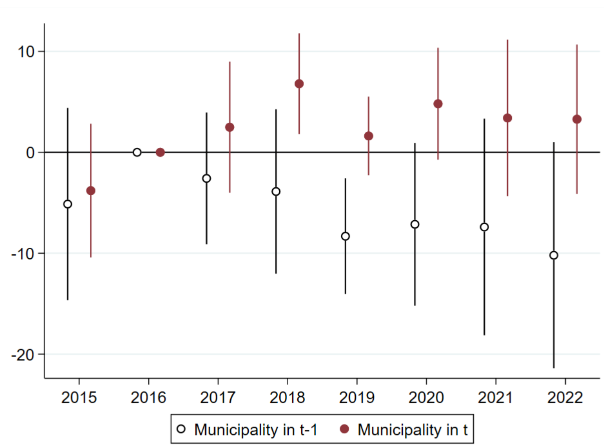
Notes. The figure displays the distribution of the change in average rent between the municipality of residence of origin and the destination municipality, for *Visale* beneficiaries from 2016 to 2023. The figure excludes zeros (which correspond to individuals who do not change municipalities when moving into a new housing unit under the *Visale* scheme). The dashed vertical line is the unweighted mean.

Figure C.12: **Destination and origin municipality characteristics for moving renters**

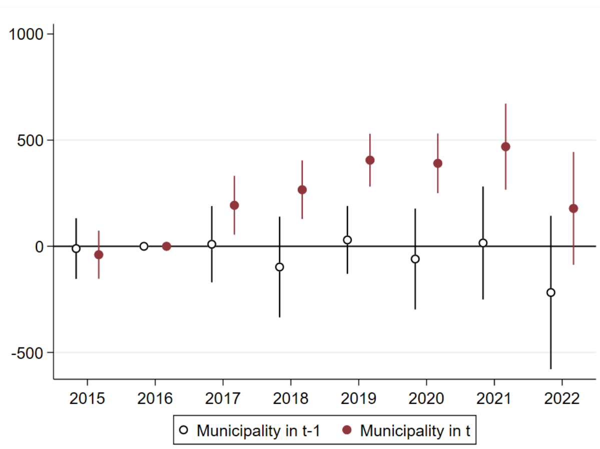
(a) **Rent per meter squared**



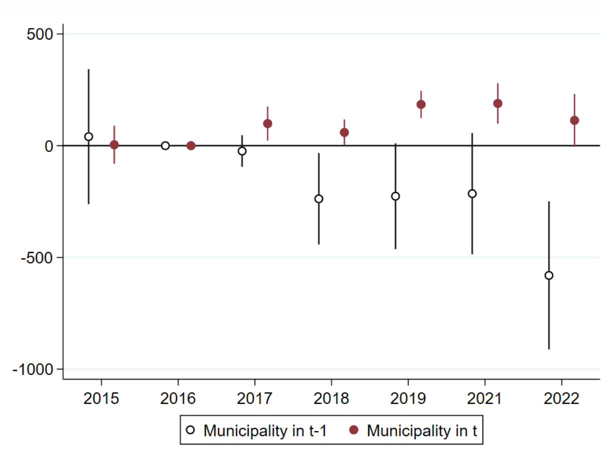
(b) **Total rent**



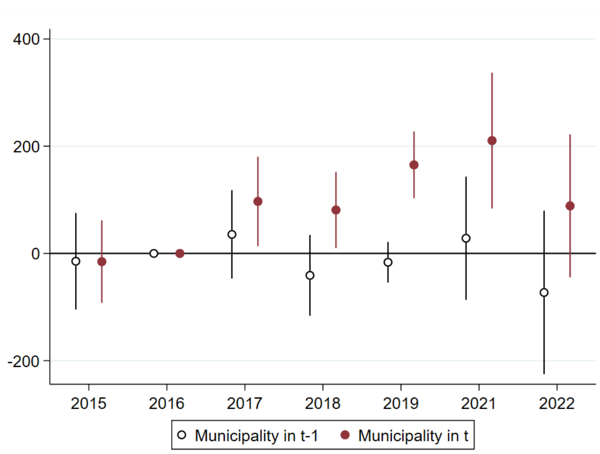
(c) **Population density**



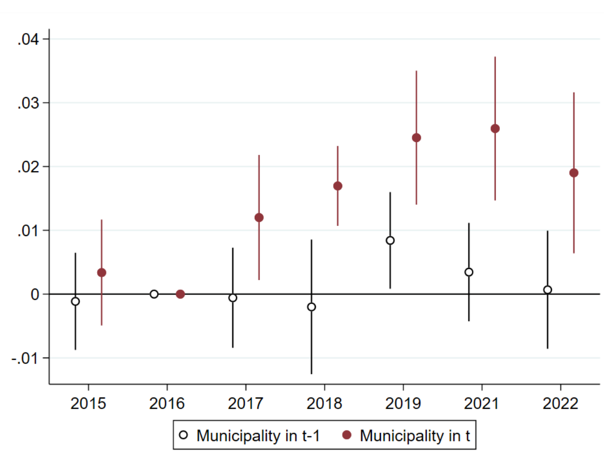
(d) **Living standard**



(e) **Wages**



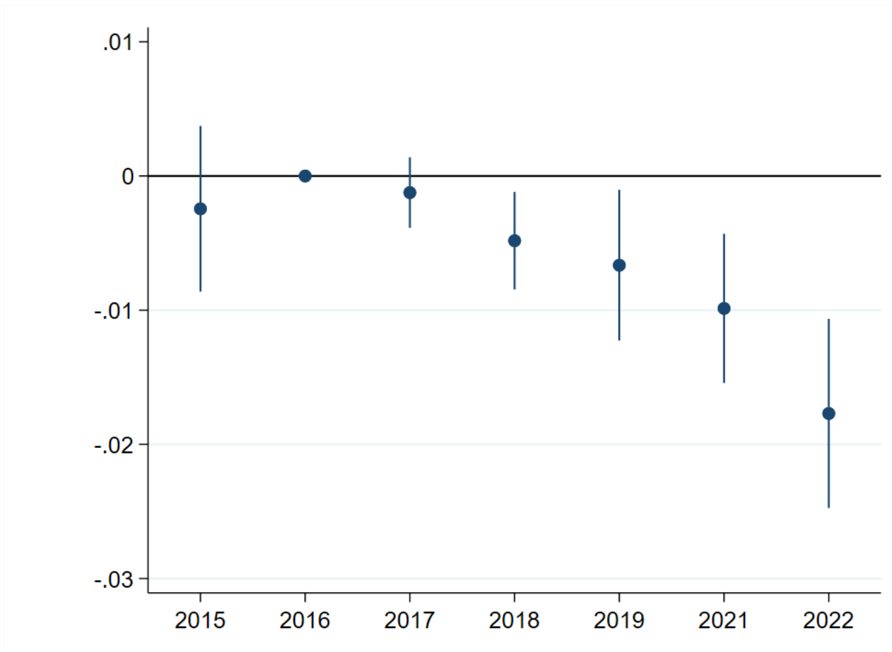
(f) **Presence of university**



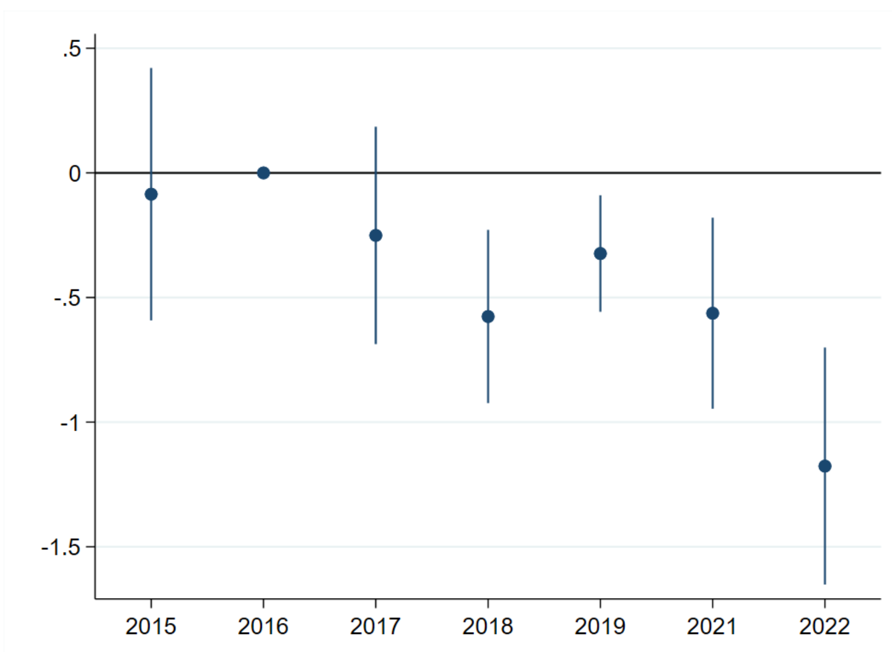
Notes. The figures plot the coefficients on the interaction of being aged less than 31 with year fixed-effects, in a regression limited to the sample of private sector renters who are single males or females and changed municipality over the course of the last year. The sample is restricted to cohorts aged 26 to 35. The outcome variable either corresponds to the municipality characteristic before (in white) or after the move (in red).

Figure C.13: **Effect on selection of movers by income**

(a) **Individual income to average municipality living standards ratio**



(b) **Individual income to average municipality rent ratio**



Notes. The figures plot the coefficients on the interaction of being aged 30 or less with year fixed-effects, in a regression limited to the sample of private sector renters who are single males or females and changed municipality over the course of the last year. The sample is restricted to cohorts aged 26 to 35. Year 2020 is missing because incomes were not released that year due to the Covid19 pandemic.

D Additional tables

Table D.1: Summary statistics for *Visale* tenants

Variables	N	mean	median	25th percentile	75th percentile
Panel A: All tenants					
Tenant's age at rental	1,229,472	24.78	23.46	20.95	26.68
Number of people in household		1.617	1	1	2
Renters with zero income**		0.633	1	0	1
Net monthly income (€)*					
All tenants		637.9	0.0	0.0	1350
Conditional on having non-zero income	451,772	1,736	1,530	1,293	1,972
Gross monthly rent (€)*		480.3	437.0	340.0	580.0
Rent-to-income ratio***	451,772	0.576	0.378	0.308	0.466
Students		0.567	1	0	1
Apartment size (sq. m.)*		34.31	23.00	17.00	46.35
Non-EU foreigners		0.318	0	0	1
EU foreigners		0.055	0	0	0
Panel B: Tenants without roommates					
Tenant's age at rental	1,070,534	24.40	23.15	20.73	26.21
Number of people in household		1.482	1	1	1
Renters with zero income**		0.687	1	0	1
Net monthly income (€)*					
All tenants		546.4	0.0	0.0	1230
Conditional on having non-zero income	335,561	1,743	1,526	1,290	1,987
Gross monthly rent (€)*		440.4	410.0	324.0	530.0
Rent-to-income ratio***	335,561	0.492	0.354	0.294	0.425
Students		0.635	1	0	1
Apartment size (sq. m.)*		29.69	20.00	16.00	38.00
Non-EU foreigners		0.353	0	0	1
EU foreigners		0.058	0	0	0

Note: The table plots the summary statistics for tenants and contracts in the *Visale* data. Contracts are limited to those started between 2016 and 2023, inclusive. *Gross monthly rent, net monthly income, and apartment size are winsorized at the 99.9th and 0.1th percentiles. **The zero income values are not affected by this winsorization because income is non-negative and zero values make up more than 0.1% of all incomes in *Visale*. ***Rent-to-income ratio is computed using the winsorized rent and income.

Table D.2: Private renters moving to a new municipality

	Private sector renters share moving to a new municipality				
	All municipalities	Municipalities no Visale contracts	Municipalities share Visale below median	Municipalities share Visale above median	Triple Difference
	(1)	(2)	(3)	(4)	
Post*Treated	0.003***	-0.000	0.002***	0.005***	
	(0.001)	(0.000)	(0.000)	(0.001)	
					0.003***
					(0.001)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	62058699	4432693	28815232	28810774	62058680

*Signif. Codes: ***: 0.01, **:0.05, *:0.1*
Standard errors clustered by age

Notes. Columns 1 to 4 display the double difference coefficients, successively for: all municipalities (column 1), municipalities with no Visale contract (column 2), with the share of Visale contracts being below the median (column 3) and above the median (column 4). Column 5 displays the triple difference coefficient: the interaction between being aged more than 30 years old, after 2016 and living in a municipality where the share of Visale contracts is above the median. The outcome variable is an indicator for being a single male or female renter in the private sector moving to a new municipality. The sample is restricted to individuals aged between 26 and 35 years old. The unit of observation is at the individual level.

Table D.3: Marginal effects on private renter shares by neighborhood

	Private sector renters share				
	Municipality income percentiles				
	1-100	1-75	76-100	1-50	51-100
	(1)	(2)	(3)	(4)	(5)
Post * treated	0.009***	0.005***	0.004***	0.003***	0.006***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Share single renters among pop aged below 30 before 2017	.13	.09	.04	.06	.07
Effect in %	7.1	5.99	10.23	4.92	9.04
	Municipality average rent per meter squared percentiles				
	1-100	1-75	76-100	1-50	51-100
	(1)	(2)	(3)	(4)	(5)
Post * treated	0.009***	0.005***	0.004***	0.001***	0.008***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Share single renters among pop aged below 30 before 2017	.13	.09	.04	.04	.09
Effect in %	7.1	5.55	11.13	2.32	11.26

*Signif. Codes: ***: 0.01, **:0.05, *:0.1*
Standard errors clustered by age

Notes. In both panels, column 1 displays the double difference coefficients for the full sample. In panel A column 2, the outcome variable is equal to one if the respondent is single renter in the private sector living in a municipality in the bottom 75% of the income distribution. Columns 3 to 5 reproduce the regressions for successively respondents in the top 25%, the bottom 50% and the top 50%. Panel B reproduces the same regressions but by municipality average rent per meter squared. Marginal effects as a share of the average baseline population are displayed at the bottom of each table. The sample is restricted to individuals aged between 26 and 35 years old. The unit of observation is the individual level.

Table D.4: Overlap index by household category

Household category	**Index value**	**Eligibility to *Visale***
Single below 30	1	Eligible
Single between 30-35	.85	Not eligible
Single between 35-40	.77	Not eligible
Single between 40-45	.72	Not eligible
Couple no child below 30	.61	Fuzzily treated
Single above 45	.55	Not eligible
Couple no child between 30-35	.54	Fuzzily treated
Couple no child between 35-40	.5	Fuzzily treated
Couple no child between 40-45	.42	Not eligible
Couple children below 30	.26	Not eligible
Other between 30-35	.23	Not eligible
Other between 35-40	.22	Not eligible
Other below 30	.21	Not eligible
Other between 40-45	.2	Not eligible
Other above 45	.18	Not eligible
Couple children between 30-35	.12	Not eligible
Couple children between 35-40	.06	Not eligible
Couple no child above 45	.05	Not eligible
Couple children between 40-45	.02	Not eligible
Couple children above 45	0	Not eligible

Notes. The table displays the values of the overlap index between each household category and the treated group (renters aged below 30). The last column shows their eligibility to *Visale*; couples with no children are considered as fuzzily treated by *Visale*, as one of the two member of the couple may be aged below 30 and benefit from the policy, making his/her spouse also benefit even if they are older than 30. More details on how the index is constructed in Section 5.