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Incidence, Allocation, and Efficiency Costs of Tenancy Rent Control

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Abstract

Tenancy rent control limits rent increases for sitting tenants while allowing market resets at vacancy. When demand grows or household composition differs across segments, spillovers raise rents in the unregulated market. We study its general equilibrium effects in Switzerland, where a nationwide regime meets large spatial variation. Linking administrative records on all households from 2010–2022 to detailed unit data and market rents, we estimate a structural sorting model with heterogeneous preferences, correcting for selection and price endogeneity. Counterfactual simulations show unregulated rents would be 8–21 percent lower, with the largest drops in supply-inelastic cities. Older, lower-income, and less educated households gain most, while newcomers face higher entry rents. The policy reduces mobility and induces space overconsumption, generating efficiency losses.

Keywords: Rent Control, Residential Mobility, Inequality.
JEL-codes: H7, H72, R23, R31, R38.

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1. Introduction

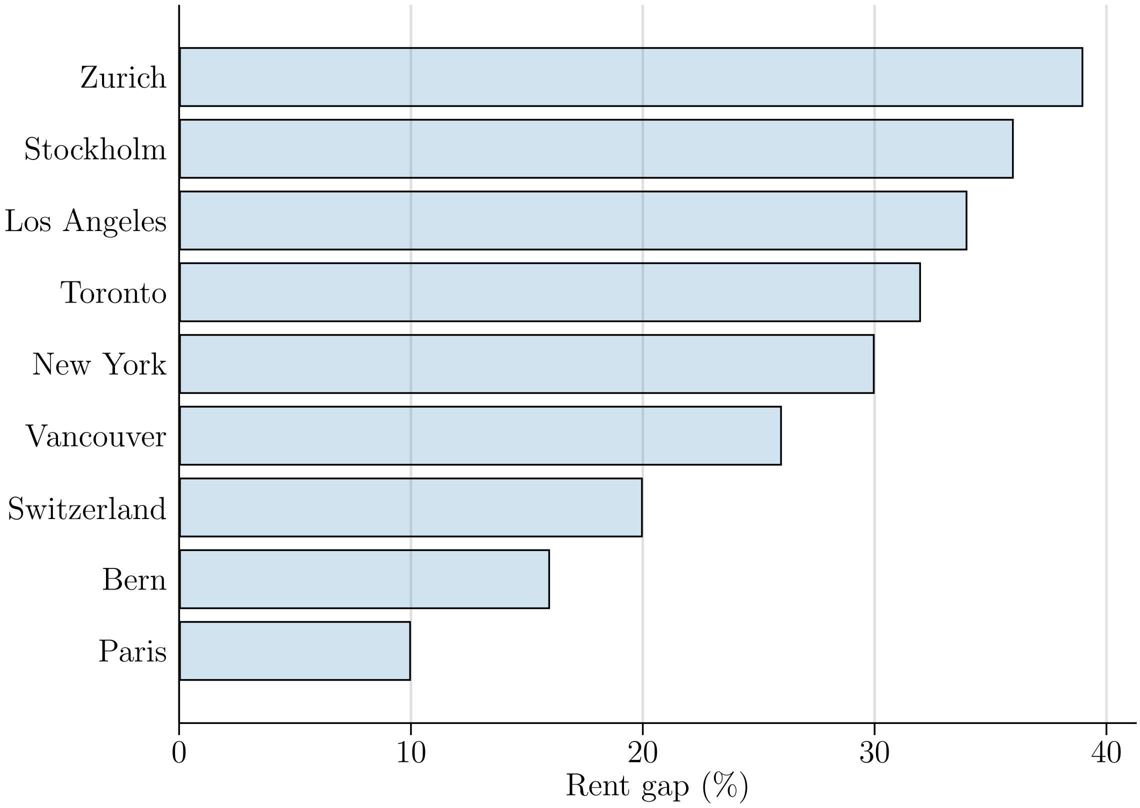
Housing affordability and availability are persistent concerns worldwide. In response to sharply rising rents, many jurisdictions have adopted or expanded rent control. Although politically popular and often motivated by distributional goals, rent control can distort price signals (Olsen, 1972; Sims, 2007; Monras and Garcia-Montalvo, 2023) and lead to inefficient allocation of housing (Suen, 1989; Glaeser and Luttmer, 2003; Bulow and Klemperer, 2012; Favilukis et al., 2023; Diamond et al., 2019a). A common form is *tenancy rent control*[1] which limits rent increases for sitting tenants, often tying them to inflation, while allowing market resets at vacancy. This widespread regulation applies in roughly 55% of OECD countries (OECD, 2024). Although often perceived as a milder intervention, a defining feature is the gap it creates between incumbent and market rents. Quantifying its distributional consequences and the scale of the resulting misallocation is challenging. It requires detailed microdata and a structural framework to recover the full counterfactual reallocation: how heterogeneous households would choose locations and units without regulation. These policies generate price spillovers between regulated and unregulated segments and alter the composition of households across neighborhoods and market tiers. Because such effects shape equity and efficiency outcomes, understanding them is central to informed housing policy debates.

This paper develops a framework to evaluate the general-equilibrium effects of rent regulation and to quantify its incidence across households, locations, and unit types. We implement the framework in a representative setting, combining a structural demand model with comprehensive household–unit microdata covering the entire rental market of Switzerland.

Switzerland is an ideal setting for our analysis. Over the past decade, strong housing demand has generated sizable and geographically heterogeneous gaps between incumbent and market rents. As As Figure 1 shows, these gaps are large in many growing cities worldwide: Zurich stands out with a gap of almost 40%, while the national average is about 20%, comparable to estimates for Vancouver. Comparable measures are not systematically available in most countries, and nationwide microdata that permit decomposing these gaps across households and locations are exceptional. Our data make this possible. We move beyond averages to examine the full distribution of gaps. Figure 2 shows that the gaps widen mechanically with tenancy duration (Panel A). The descriptive patterns reveal strong heterogeneity. Older households (Panel B) and lower-

[1] This form of rent control policy is also termed *third generation rent control* (Arnott, 2003).

Figure 1: Gap between private-sector rent-controlled (sitting-tenant) and market rents (new leases)



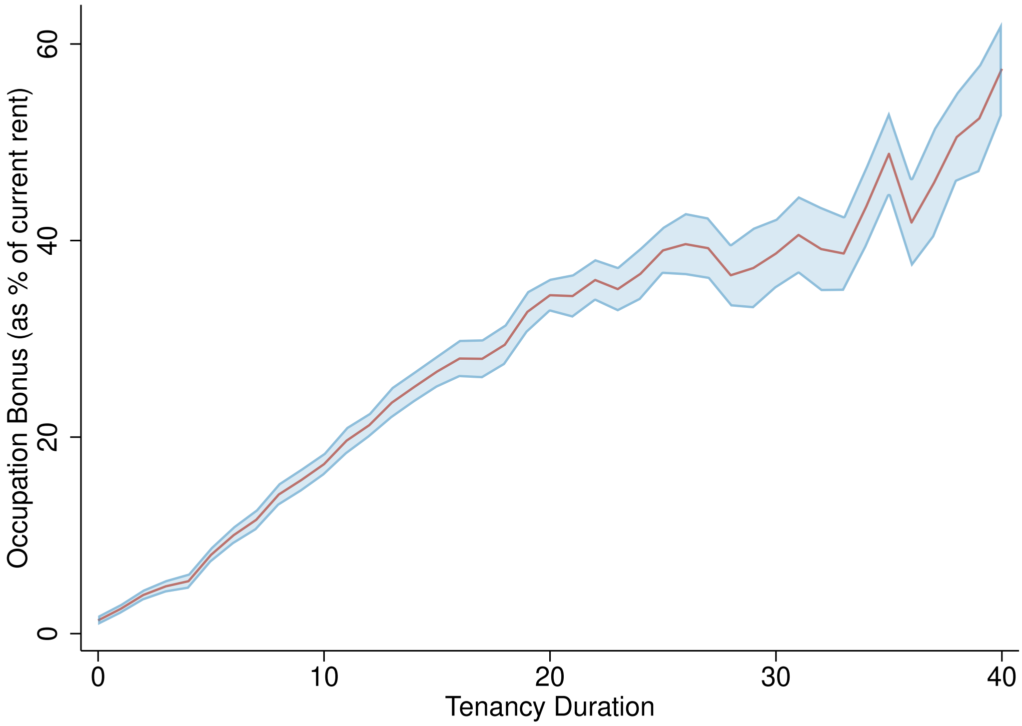
Notes: Sources: Stockholm: Donner and Kopsch (2023); Los Angeles: Diamond et al. (2019a); Toronto and Vancouver: Canada Mortgage and Housing Corporation (2022); New York: NYC Department of Housing Preservation and Development (2021); Ireland: Residential Tenancies Board (2023); Paris: Observatoire des Loyers de l'Agglomération Parisienne (OLAP) (2022); Switzerland: own calculations. See Appendix A for details on the methodologies of each source.

income households (Panel C) pay rents far below current market levels. These sizable gaps make it essential to understand both the distributional and efficiency effects of tenancy rent control. Crucially, such an assessment must account for spillover effects from controlled to uncontrolled markets (George Fallis, 1984; Early, 2000; Autor et al., 2014; Hahn et al., 2024). Incorporating these spillovers, this paper uses rich microdata and a novel empirical approach to estimate the full incidence of tenancy rent regulation.

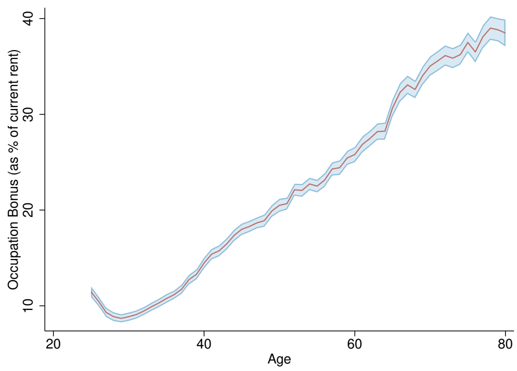
We study tenancy rent control under Switzerland's stable federal regime. It allows free negotiation of initial rents but caps within-tenancy increases to mortgage rate pass-through, partial inflation adjustments, and value-enhancing investments. These rules shield sitting tenants from market pressures while newcomers pay market rents, creating

Figure 2: Gap between rent-controlled and market rents across household characteristics

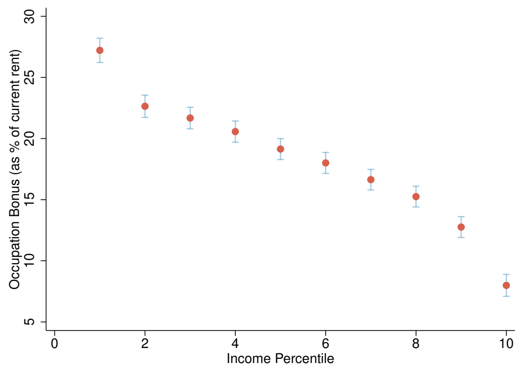
Panel A: Gap over Tenancy Duration



Panel B: Gap over Age



Panel C: Gap over Income Percentile



Notes: Data from the *Structural Survey* (see Section 4). Observed rents are available for all households. We estimate a hedonic rent model using the subsample of households that moved within the survey year ("movers") and use the estimated coefficients to impute market rents for units occupied by households that did not move ("non-movers"). Panels A–C report the difference between imputed market rent and actual rent paid, with 95% bootstrap confidence intervals. See Appendix B for details on methodology.

a persistent rent gap. Because the regulation applies nationwide, yet market conditions vary sharply across space, the setting provides rich variation for identifying general-equilibrium effects.

Our empirical analysis combines administrative microdata on the universe of Swiss households from 2010–2022 with detailed housing unit characteristics and precise geolocation. We link the Population and Households Statistics to annual income records from the social security registry, structural dwelling attributes from the Federal Register of Buildings and Dwellings, and self-reported rents from the Structural Survey. This linkage allows us to observe household demographics, incomes, and exact rental payments for the regulated sector, and to impute market rents for these units.

We build a structural residential choice model following McFadden (1978), Bayer et al. (2003), and Bayer et al. (2007). Households choose among a finite set of available units within their labor market region. Indirect utility depends on housing attributes, price, and a flexible measure of distance from the current residence, which interacts with life-cycle indicators such as having children or being retired. We allow for rich heterogeneity in tastes and price sensitivity across households. To avoid bias from regulated "stayers," we estimate preferences using only movers, correcting for selection with a Heckman-style first stage. Using a Bartik shift–share instrument based on initial municipal employment shares and subsequent sectoral shocks, we address price endogeneity from unobserved quality.

The estimated model yields household-level demand elasticities and willingness-to-pay measures for each unit. We then simulate the counterfactual without tenancy control by matching households to units through a Hungarian auction algorithm, which adjusts prices until markets clear. This procedure accounts for demand spillovers from regulated to unregulated segments, a key channel in the incidence of rent control. Comparing observed and counterfactual allocations gives the implicit subsidies accruing to incumbents, the corresponding burdens on newcomers, and the aggregate deadweight loss from misallocation. We decompose these outcomes by income, age, education, and geography, and study how regulation affects mobility and housing consumption.

Our findings reveal sizable and uneven effects of tenancy rent control. The model estimates show large variation in households' price elasticities and willingness to pay. Low-income and less-educated households are more sensitive to rent changes. In the counterfactual without regulation, and accounting for spillovers, market rents in the unregulated segment would be 8 to 21% lower than observed. The effects differ sharply across space. Urban labor markets with inelastic supply see the largest rent reductions. Rural areas experience smaller changes. Composition effects, where households with low price elasticity cluster in the unregulated sector, amplify price pressures in cities. Supply responses mitigate but do not offset these pressures. The distributional impacts are pronounced. Incumbent tenants in regulated units capture large implicit subsidies. These subsidies are highest for older, lower-income, and less-educated households. Newcomers and mobile households bear the costs through higher rents. Across regions and demographic groups, the policy reshapes housing allocation and welfare distribution. Efficiency losses are concentrated in high-demand urban areas.

This paper contributes to a growing body of literature analyzing the causal impacts of rent control policies on various outcomes.[2] Diamond et al. (2019a) show that San Francisco's 1994 rent control law benefited incumbent tenants but imposed costs on future renters and unregulated units, effectively transferring wealth to long-term residents. Similarly, Ahern and Giacoletti (2022) find that St. Paul's 2021 rent control policy sharply reduced property values, with wealthier tenants gaining the most, contrary to the policy's intended redistributive goals. Mense et al. (2023) document that Germany's rent cap lowered regulated rents but raised unregulated ones, reduced mobility, and led to inefficient redevelopment. Using a quasi-natural experiment, Cerqueiro et al. (2024) find that rent control removal disproportionately harms low-income workers, pushing them to city outskirts with higher rents and lower-quality jobs. By capping rents below market levels, regulation may also discourage new construction and maintenance (Downs, 1988) ultimately exacerbating housing shortages in the long run (Asquith, 2019). Our study extends this literature by quantifying how tenancy rent control redistributes housing consumption across income, education, and age groups, and by measuring its effects on mobility, composition of demand, and rural–urban price differentials. Studies by Nagy (1995); Gyourko and Linneman (1989); Ault et al. (1994); Munch and Svarer (2002) found reduced mobility among tenants in rent-controlled units. Further, this paper relates to the literature examining rent regulation externalities, such as the effects on land prices (Mense et al., 2019), housing quality (Olsen et al., 2004; Moon and Stotsky, 1993), crime (Autor et al., 2019), labor markets (Jiang et al., 2025), and gentrification (Autor et al., 2017).

Our paper makes several contributions to the existing literature. First, we develop a structural framework that combines a residential sorting model with an assignment algorithm to recover general-equilibrium prices and allocations absent rent control. Second, we estimate the model using linked household–unit microdata covering the entire

[2]Malpezzi (2003) provides a concise literature review on the costs and benefits of rent control up to the early 2000s, while Kholodilin and Kohl (2023) present a more recent survey.

Swiss rental market from 2010 to 2022, yielding household-specific demand elasticities and willingness-to-pay measures. Third, we quantify spillovers from regulated to unregulated segments, finding that unregulated rents would be 8–21 percent lower in the counterfactual, with effects varying by location, demand growth, and supply elasticity. Fourth, we document large and uneven distributional impacts, as older and lower-income households capture the largest subsidies. Fifth, we show that regulation reduces mobility and induces space overconsumption, generating substantial misallocation and deadweight loss.

The remainder of the paper is structured as follows. Section 2 introduces the conceptual framework motivating our analysis. Section 3 derives the residential choice model. Section 4 describes the Swiss rent-regulation system and the household-level data. Section 5 reports the estimation results, and Section 6 presents the counterfactual analysis. Section 7 concludes.

2. Conceptual framework

Rent control policies vary in design, particularly in terms of which market segments they target. The literature commonly distinguishes between two generations of rent control (Basu and Emerson, 2000; Arnott, 2003; Malpezzi, 2003). *First-generation* rent control policies apply broadly across the entire rental market and typically involve a strict rent freeze, setting rents below market-clearing levels (Arnott, 1995). In contrast, *second-generation* policies regulate only specific segments of the market, leaving other parts unaffected (George Fallis, 1984; Arnott, 1995; Basu and Emerson, 2000).

We focus on *tenancy rent control*, a prevalent form of second-generation regulation (Basu and Emerson, 2000; Arnott, 2003). This approach restricts rent increases within an existing tenancy but permits landlords to reset rents to market levels between tenancies. It is widely used in jurisdictions like Germany, France, Ireland, Spain, Sweden, and several US and Canadian cities, including New York, San Francisco, Toronto, and Vancouver. As Arnott (2003) notes, tenancy rent control represents a compromise between heavy-handed regulation and unfettered market mechanisms, creating a distinct market equilibrium that balances reduced efficiency with increased tenure security.

Tenancy rent control effectively segments the housing market into two distinct sectors: a *regulated sector*, in which incumbent tenants pay below-market rents (p^r), and an *unregulated sector*, in which new tenants face market-clearing rents (p^{ur}). This segmentation generates *spillover effects* and leads to *housing misallocation*, such that p^{ur} exceeds the unified market-clearing price p that would prevail in the absence of rent control.

Two main forces drive these inefficiencies: (i) aggregate demand increases and (ii) distortions in household composition between the two market segments. Figure 3 illustrates these mechanisms and follows a simple setup: The *total housing supply* is the horizontal length of the box, i.e., short run supply is inelastic.[3] The *total housing demand* is made up of two groups: incumbent tenants D^r and moving tenants D^{ur}. The slope of each group's demand reflects i) the relative size of this group (i.e., share of total population) and ii) how sensitive this group is to rent changes (i.e., demand elasticity). Initial market-clearing rents (p) are determined by the intersection of the two demand curves. Thereafter, rents for incumbent tenants are regulated, i.e., they no longer face market pressure.

2.1. Demand increase

The left-hand panel shows how an increase in aggregate demand, from D^{ur} to $D^{ur'}$, creates a price wedge. Without rent control, the equilibrium price would be p', strictly lower than p^{ur}. Regulation shields incumbent tenants from market pressure, so they continue to consume x at price p. Increased demand from moving households ($D^{ur'}$) does not meet the full rental stock but only the unregulated segment (i.e., vacated units), pushing its price to p^{ur}. The welfare loss from misallocation appears as the triangle with base $p^{ur} - p^r$ and height $x - x'$. The term $x - x'$ reflects overconsumption by households in the regulated sector, driven by artificially low rents. While this overconsumption is most intuitively associated with housing size, it can also involve excess consumption of other attributes such as unit quality or location. As population growth, rising incomes, or urban migration increase aggregate demand, pressure on the unregulated segment intensifies. Because rents in the regulated segment are frozen or rise only marginally, they cannot adjust to absorb this new demand. Excess demand therefore concentrates in the unregulated sector, driving up rents for new entrants to p^{ur}. This level is higher than the hypothetical equilibrium price p' that would prevail in a fully deregulated market.

2.2. Composition effect

Holding aggregate demand constant, Panel (b) of Figure 3 illustrates that further distortions arise from changes in household composition. Tenancy rent control reduces mobility. Incumbent tenants with below-market rents have little incentive to move, even

[3] The setup extends to a situation with elastic supply, in this case the vertical length of the box adjusts at the right marging, depending on equilibrium rent.

after major life changes such as divorce, children moving out, or income shocks. Downsizing would entail giving up their favorable lease terms and entering a more expensive rental market. In public discourse, this phenomenon is often described as a lock-in effect.

Consistent with lock-in arguments, Table E.8 shows that incumbent households are less likely to move when the gap between their current rent and the imputed market rent for their unit is larger. While these results are correlational, a substantial literature documents reduced mobility arising from the benefits of rent regulation (see, e.g., Munch and Svarer, 2002; Diamond et al., 2019b).

Consequently, households tend to overconsume housing. In contrast, those needing more space, often new entrants, must compete in the unregulated sector, where prices are significantly higher. This dynamic leads to misallocation, lower turnover, and reduced market efficiency (Glaeser, 2003).

This composition effect occurs when households in the unregulated sector have systematically lower price elasticity of demand than those in the regulated sector. Even if aggregate demand stays constant, because positive shocks for some households offset negative shocks for others, market segmentation changes the composition of demand. The red demand curve $D^{ur'}$ in Panel (b) of Figure 3 illustrates this shift. It widens the price wedge between sectors and increases the efficiency loss from misallocation. Composition effects thus add to the upward pressure on unregulated rents already caused by rising demand.

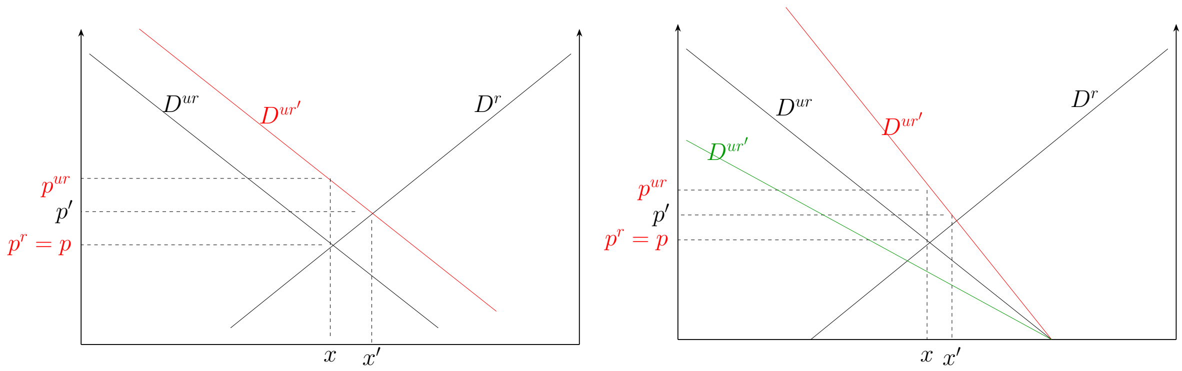
Conversely, the composition of the unregulated market could shift toward higher price elasticity, as shown by the green curve in Panel (b) of Figure 3. If such a shift occurred while total demand stayed constant, tenancy rent control would no longer bind. This scenario could arise through immigration if newcomers are more price-sensitive than incumbents. In that case, composition effects would mitigate the price divergence between p^{ur} and p' driven by aggregate demand growth.

Thus, composition effects can either amplify or offset distortions from demand growth. When stayers' overconsumption dominates, they exacerbate the inefficiencies of rent control. When newcomers are more price-sensitive, they can partially narrow the wedge created by rising demand.

Ultimately, the interaction between aggregate demand growth and composition effects shapes the rent gap between regulated and unregulated sectors. As Basu and Emerson (2000) observe, tenancy rent control redistributes housing costs between tenant types: long-term residents benefit from greater affordability and security, while mobile or newly arriving households face inflated market rents and reduced access to housing.

Leveraging the employed methodology, we can quantify the surplus for each house-

Figure 3: Comparative demand under different growth compositions



(a) Demand increase

(b) Composition effect

hold, which corresponds to the household's willingness to pay, derived from the demand function, minus the equilibrium price. Computing this difference for the observed regulated market and the counterfactual setting without tenancy rent control allows us to analyse welfare effects arising from regulation.

2.3. Housing supply

So far, we have treated housing supply as fixed. This is reasonable in the short run and will hold for most of the analysis. However, over time, supply responds to incentives from tenancy rent control. Anticipated rent increases in the unregulated segment encourage landlords and developers to adjust the housing stock.

New construction is central to absorbing demand from population growth, income gains, and composition effects. By building in the unregulated segment, developers serve newcomers and households excluded from or leaving the regulated market. Landlords may also shift existing units into the unregulated sector through renovations, conversions, or demolitions, followed by redevelopment (Mense et al., 2019; Diamond et al., 2019a). Such strategies expand the supply of uncontrolled units and partly offset the distortions from tenancy rent control. A possible consequence is higher construction activity than without tenancy control, as developers respond to $p^{ur} > p'$. Construction activity may further be biased toward the preferences of tenants in the unregulated market. In Figure 3, an increase in supply would correspond to a widening of the x-axis.

The extent to which supply responds to rent spillovers depends on local housing supply elasticity. In cities with inelastic supply – due to land scarcity or regulatory barriers – spillovers are larger than in more elastic markets, such as rural areas or suburbs (Saiz, 2010; Hilber and Vermeulen, 2016). Generally, higher expected rents also raise capitalization rates and encourage investments (Büchler et al., 2021). In contrast, the expected future rent increase can lead to reduced construction activity in settings with tenancy regulation. This effect arises if developers wish to preserve the option value of their land (Quigg, 1993).[4] In summary, supply adjustments ease pressure but are second-order and cannot fully offset spillovers. Removing tenancy rent control is thus expected to lower average rents for new tenants, especially in supply-constrained markets, while raising costs for incumbent tenants.

2.4. Tenure choice

Tenancy rent control also shapes tenure choice, i.e., whether households rent or buy. The user-cost framework of Poterba (1984) states that, in equilibrium, a landlord sets rent equal to the annual cost of owning a comparable unit p^{buy}:

$$p^{ur} = p^{buy}(i + \delta + \tau_p + m + \rho - \pi_e)\,, \tag{1}$$

where p is annual market rent in a situation without regulation, p^{buy} the house price, i the interest rate, δ depreciation, τ_p property tax, m maintenance, ρ a risk premium, and π_e expected price appreciation. If p falls below this user cost, renting is cheaper. If it exceeds it, ownership is preferred. With tenancy regulation, this indifference condition depends on whether the household rents in the regulated market and pays p^r, or in the unregulated market and pays p^{ur}.

Hence, tenancy rent control breaks this parity by capping rent growth for incumbents, pushing p below user cost. Protected tenants receive an implicit subsidy and greater security, so many delay or forgo buying. Newcomers, facing market rents, often find ownership more attractive. These forces create two tenure paths: long-term renters stay put, while newcomers buy when feasible.[5]

Lifting rent control would raise incumbents' rent, reduce the subsidy, and make renting less attractive. More households would shift toward ownership. This effect mirrors evidence that policies lowering user costs, such as mortgage-interest deductions or excluding imputed rent, increase homeownership rates (Rosen, 1985; Bourassa et al., 2003; Poterba and Sinai, 2008).

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[4] Developers expecting tenants to remain in a contract for long may postpone construction to benefit from higher future demand. By delaying, they can lock in higher future rents rather than committing to lower present rents.

[5] As detailed in Section 4.1, Swiss regulation allows landlords to pass on costs from higher mortgage rates (i). It also permits passing on costs from property investments (m), ensuring that user costs along these margins are equalized between tenure choices.

Tenancy rent control reshapes rent levels, housing allocation, supply responses, and tenure choice. Measuring its net impact requires estimating the counterfactual rent that reflects demand spillovers from the controlled to the uncontrolled sector and the resulting supply response. To this end, we build a residential choice model incorporating these mechanisms.

3. Residential choice model

We only observe the intersection of demand in the unregulated and regulated markets. We cannot observe how regulated households would behave under market rents, or how rents would form if all households competed in a single market. Our counterfactual removes regulation so every household bids for the full rental stock. To simulate this, we estimate household-level demand functions that map housing attributes and prices into choice probabilities. This approach captures heterogeneity in housing supply and household preferences, including differences in willingness to pay.

To implement this, we build a discrete residential choice model following Bayer et al. (2003) and Bayer et al. (2007), grounded in the framework of McFadden (1978).[6]

In housing markets with tenancy regulation, sitting tenants pay less than the market rent for the same unit. Using their choices and rents to estimate demand can be misleading. Bayer et al. (2003) and Bayer et al. (2007) address this by estimating market rents for discounted units with a hedonic model, then combining these with observed market rents for movers. This approach assumes sitting tenants would stay even if they had to pay the higher, imputed rent, which is unlikely. Another option is to keep the actual rent for sitting tenants and use the imputed rent only when their units appear as non-chosen alternatives for others. This, however, assigns different prices to the same unit and makes contraction mapping infeasible. We therefore focus on movers, for whom we observe valid market prices for all potential alternatives. Their choices reveal true preferences.

Focusing on movers has the advantage of allowing us to observe competitive prices. However, it may still introduce sample selection bias. We address this by adding a selection stage following Heckman (1979). This step allows us to correct for potential selection bias.[7]

[6]By choosing a discrete choice model over a simple hedonic demand model, we account for the fact that households cannot adjust housing consumption continuously, but must choose from a finite set of available alternatives.

[7]An alternative is to estimate the model jointly for movers and stayers, treating the current unit as the chosen alternative for stayers. This requires knowing the counterfactual price they would face if

In the selection stage, we estimate a logit model of the household's decision to move or stay. The model includes all household characteristics from the residential choice model and two change indicators as instruments: newly married and newly separated. These variables affect the decision to move but, conditional on current characteristics, should not directly influence the choice among available units. From this stage, we compute the Inverse Mills Ratio (IMR) for each household. We include the IMR in the residential choice model as an additional household characteristic. This corrects for the fact that unobserved shocks affecting the move decision may also shape preferences for unit or neighborhood attributes. For example, starting university may trigger both a move and an increase in demand for units near campus. Including the IMR adjusts for this selection into the mover sample and reduces bias in the estimated demand parameters.

We model the indirect utility of housing unit h for household i as:

$$V_h^i = \delta_h + \lambda_h^i + \epsilon_h^i, \tag{2}$$

where δ_h captures housing-unit fixed effects (i.e., the utility the average household derives from residing in unit h), λ_h^i is the heterogeneous household–unit-specific utility component, and ϵ_h^i is an idiosyncratic error term.

We specify λ_h^i as:

$$\lambda_h^i = \sum_{k=1}^{K}\sum_{u=1}^{U} \alpha_{k,u} z_k^i x_{h,u} + \left(\sum_{k=1}^{K} \beta_k z_k^i\right) p_h + f(\text{coord}_h, \text{coord}^i), \tag{3}$$

where $x_{h,u}$ denotes the value of housing characteristic $u \in \{1, \ldots, U\}$ for unit h, and z_k^i denotes household characteristic $k \in \{1, \ldots, K\}$ for household i. The IMR from the selection stage enters as one of the household characteristics z_k^i. The parameter $\alpha_{k,u}$ captures how household characteristic k interacts with housing characteristics u, while the coefficients β_k measure price sensitivity as a function of household characteristics. The term p_h is the rent of unit h. The function $f(\text{coord}_h, \text{coord}^i)$ flexibly captures location attachment based on the distance between the household's current and potential units. We allow this distance effect to vary with two household traits: having children and being retired. We assume that households are price takers. They treat the choice set as fixed and pick the unit whose characteristics best match their needs. Individual choices do not affect prices.

moving, which is usually unobserved and confounded by regulation.

The average utility component for housing unit h can be further decomposed as

$$\delta_h = \sum_{u=1}^{U} \alpha_{0u} x_{h,u} - \beta_0 p_h + \phi_h, \tag{4}$$

where $x_{h,u}$ are the observable characteristics of the unit, p_h is its rent, and ϕ_h is an unobserved unit-specific error term. The coefficients α_{0u} capture how each attribute contributes to utility for the average household, while β_0 measures the average price sensitivity.

3.1. Estimation

Selection stage: In the selection stage, we estimate the probability that household i moves with a logit model:

$$M_i^* = \sum_{k=1}^{K} \gamma_k z_k^i + \nu_i, \tag{5}$$

where $M_i^* = 1$ if household i moves and 0 otherwise. The vector $\mathbf{z}_i$ includes current household characteristics and changes in these characteristics since the last period, and ν_i is an i.i.d. error term.

The selection model implies

$$P(M_i = 1 \mid \mathbf{z}_i,\, w_i) \;=\; \frac{\exp\bigl(\mathbf{z}_i^\top \boldsymbol{\gamma} + \pi\, w_i\bigr)}{1 + \exp\bigl(\mathbf{z}_i^\top \boldsymbol{\gamma} + \pi\, w_i\bigr)}, \tag{6}$$

where w_i is an excluded instrument that affects selection but is not included in the outcome equation and $\mathbf{z}_i^\top \boldsymbol{\gamma} + \pi\, w_i \;=\; \sum_{k=1}^{K} \gamma_k\, z_k^i \;+\; \pi\, w_i$. From the estimated model, we compute the generalized IMR for household i:

$$\mathrm{IMR}_i = \begin{cases} \frac{\phi(\mathbf{z}_i^\top \boldsymbol{\gamma})}{\Phi(\mathbf{z}_i^\top \boldsymbol{\gamma})} & \text{if } M_{it} = 1, \\ \frac{\phi(\mathbf{z}_i^\top \boldsymbol{\gamma})}{1-\Phi(\mathbf{z}_i^\top \boldsymbol{\gamma})} & \text{if } M_i = 0, \end{cases} \tag{7}$$

where $\phi(\cdot)$ and $\Phi(\cdot)$ are the probability density function and cumulative distribution function of the logistic distribution, respectively. We then include the IMR as an additional household characteristic in the residential choice model, following Heckman et al. (1998).

First stage: We estimate (2) using a conditional logit model. For each household i, we draw $n = 19$ non-chosen alternatives from units vacated in the same year and labor market region, and add the true choice to form the choice set.[8] We then estimate the parameters in λ_h^i from (3) and the unit-specific fixed effects δ_h, maximizing the probability that each household chooses its observed unit h^*.[9]

The likelihood function is

$$l = \sum_i \sum_h I_h^i \ln \left(P_h^i\right) \quad \text{where} \quad P_h^i = \frac{\exp\left(V_h^i\right)}{\sum_{j \in \mathcal{C}_i} \exp\left(V_j^i\right)}, \tag{8}$$

where I_h^i equals 1 if i chooses its true housing choice h^* and 0 otherwise, and P_h^i is the probability of individual i choosing unit h.

Following Bayer et al. (2003, 2007), we apply contraction mapping to ensure that demand for each unit does not exceed supply (see Berry et al., 1995). The market-clearing condition is

$$\sum_i \left(P_h^i\right) = 1 \quad \text{for all } n \tag{9}$$

is met at every iteration of refitting the model. For the case at hand, the contraction mapping relevant for the presented application is simply

$$\delta_h^{t+1} = \delta_h^t - \ln\left(\sum_i \hat{P}_h^i\right). \tag{10}$$

Second stage: We decompose mean indirect utilities δ_h based on the set of housing characteristics x_h and prices p_h. We estimate 4 while addressing the endogeneity of prices. Unobserved unit and neighborhood attributes (ϕ_h) may correlate with rents p_h. To address this, we instrument p_h with a shift–share variable based on sectoral employment in municipality m in 2000. We combine the municipality's sectoral employment shares with growth rates at the cantonal sector level. This approach uses nearly 20-year-old historical sectoral shares that interacted with aggregate sectoral shifts to generate exogenous variation in rents. Work on shift–share designs (Adão et al., 2018; Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022) shows that validity requires predetermined exposure shares or random aggregate shocks. We rely on the first condition. Initial sectoral shares are persistent, shaped by natural amenities and market access, and unlikely to correlate with recent changes in local housing–supply shifters.

[8]We restrict non-chosen alternatives to the same labor market region, taking the decision of which labor market to move to as given.

[9]Bayer et al. (2007) and Bayer et al. (2003) provide a detailed description of the approach.

$$\delta_h = \sum_{u=1}^{U} \alpha_{0u} x_{h,u} \; - \; \beta_0 \, \widehat{p}_h \; + \; \phi_h \tag{11}$$

3.2. Willingness to pay

With the estimated parameters in (3) and (4), we compute each household i's willingness to pay (WTP) for each housing option h. To obtain the marginal WTP for a specific attribute x_1, we hold utility constant and solve for the rent change ΔWTP^i that offsets a change Δx_1. For the average household, WTP for unit h with all characteristics identical to the average unit except $x_1$is:

$$WTP_h = \exp(\bar{p}) \times \exp\left(-\frac{\alpha_1^i}{\beta_0}\,(x_{h,1} - \bar{x}_1)\right), \tag{12}$$

where $(x_{h,1} - \bar{x}_1)$ is the deviation of unit h in characteristic x_1 from the mean, and $\bar{p}$ is the average rent. If household i differs from the average in characteristic z_1 by Δz_1, its WTP for unit h, again differing in x_1 from the mean, is:

$$WTP_h^i = \exp(\bar{p}) \times \exp\left(-\frac{\alpha_{01} + \alpha_{11}\Delta z_1}{\beta_0 + \beta_1 \Delta z_1}\,(x_{h,1} - \bar{x}_1)\right) \tag{13}$$

3.3. Market clearing prices

We compute equilibrium prices that clear the market for moving households, so that $D_{\text{move}} = S_{\text{move}}$.[10] We use a Hungarian auction algorithm, following Gilbert and French (2024) and Demange et al. (1986). Households value units according to the WTP parameters from the residential choice model. Households value units according to the WTP parameters from the residential choice model. The auction begins with all prices set to zero. Each household chooses the unit h that maximizes their utility, i.e., the unit for which WTP_{ih} - p_h is largest. An equilibrium occurs when every unit is allocated to exactly one household, maximizing that household's utility. If no such matching exists, the algorithm raises the prices of units with excess demand. It repeats this process, increasing the prices of scarce units, until it reaches an equilibrium allocation. The Hungarian auction is well-suited for this task because it ensures the final allocation is efficient and market-clearing. It finds a price vector and a matching where no household-unit pair would prefer to deviate, and no unit remains unmatched.

[10] We assume supply is limited to units vacated by movers and do not allow for new construction.

We add outside options with zero utility to capture the possibility that households leave or do not enter the regional labor market. We choose the number of outside options so that the algorithm's mean market-clearing price matches the observed mean. In our preferred specification, about 3.5% of options are outside the mover market, consistent with average net immigration. We keep this share constant across simulations.

The algorithm uses an $I \times H$ matrix of WTP values for all household-unit combinations within a labor market region. To speed computation, we randomly sample 800 i–h combinations per market, yielding an 800×800 matrix. We run the routine 20 times for each labor market region.

3.4. Overidentification check

We interpret the prices obtained for movers as the unregulated market prices, denoted p_h^{UR}. To validate the model, we compare the predicted p_h^{UR} with the observed rents for newly contracted leases on each unit. Tests for potential sampling bias ensure stable results. The panel structure of the dataset enables a stronger backtesting framework. The model is estimated for one year and used to predict p_h^{UR} for the preceding or following year. Such out-of-sample predictions provide a robust check on the validity and stability of the estimates.

4. Institutional background and Data

4.1. Institutional background

Switzerland, with over 60% of households renting, provides a pertinent setting for studying the effects of tenancy rent control. Table 1 compares tenure types in Switzerland, Germany, and the USA, highlighting notable differences. Switzerland's high rental share stands in sharp contrast to the USA, which has a similar GDP per capita, and to Germany, its closest geographical and cultural neighbor.[11] Renting in Switzerland spans all income levels: nearly 50% of households in the top income quintile rent, a pattern uncommon in the USA and Germany. Private households own almost half (47%) of rented apartments. Institutional investors hold 34%, cooperatives own 8%, real estate firms account for 7%, and public housing initiatives manage only 4% of the rental market.

Swiss rental law, established in the 1980s and 1990s, builds on emergency rental regulations introduced during the two World Wars (Hausmann, 2016; Rohrbach, 2014).

[11] Werczberger (1997) and Bourassa and Hoesli (2010) discuss the drivers of Switzerland's low homeownership rate.

Table 1: Comparison Tenure Types 2020

		Income Quintile				
	All households	Lowest	2nd	3rd	4th	Highest
Switzerland						
Renter	60.8	68.9	65.8	63.1	57.4	49.0
Owner with mortgage	33.9	22.8	29.0	32.4	39.3	45.9
Owner outright	4.4	6.6	4.0	4.1	2.9	4.5
Other, unknown	0.9	1.6	1.2	0.4	0.4	0.6
Germany						
Renter	51.0	68.7	58.8	50.7	42.2	34.7
Owner with mortgage	25.6	11.8	17.4	26.0	33.4	39.1
Owner outright	19.8	14.4	19.3	19.5	21.6	24.2
Other, unknown	3.6	5.1	4.5	3.8	2.8	1.9
United States						
Renter	32.1	52.6	37.9	29.8	23.2	17.0
Owner with mortgage	40.4	17.4	31.1	42.9	52.4	58.4
Owner outright	25.7	26.2	28.8	25.9	23.5	24.1
Other, unknown	1.7	3.7	2.1	1.4	0.9	0.6

Source: OECD Affordable Housing Database.

While landlords and tenants can freely negotiate initial rents, the law limits rent increases during existing tenancies.[12]

Rents within existing contracts can rise only under three conditions: (i) higher refinancing costs from rising mortgage rates, (ii) inflation adjustments, and (iii) passing on costs from value-enhancing investments such as major refurbishments or new installations. The Federal Agency for Housing sets a quarterly reference interest rate, which allows landlords to adjust rents when mortgage rates increase. Regulations also permit inflation adjustments, capped at 40% of the inflation rate. Tenants benefit from strong protections. Rental agreements are typically open-ended, and tenants can terminate with three months' notice. Landlords face stricter rules and may terminate only for personal use or when major renovations require it.[13]

[12]Regulations nominally restrict rent increases between tenancies, but enforcement is weak. No central agency monitors compliance; households must appeal if they believe initial rents are excessive. With little access to information on previous rents, only 0.3% of new leases face legal challenges (BWO 2022). According to the common practice of arbitration courts, rent increases of up to 10% are not considered excessive. Even if a few appeals occur, regulation could still have a disciplinary effect on landlords. In this case, we would expect to observe bunching around the 10% cutoff. If so, we would expect rent changes to bunch around the 10% cutoff. Figure B.11 shows no such bunching, suggesting the regulation does not bind rent increases between tenancies.

[13]Cantonal and communal authorities can impose further restrictions, such as in Geneva, Basel, and Vaud.

4.2. Data sources

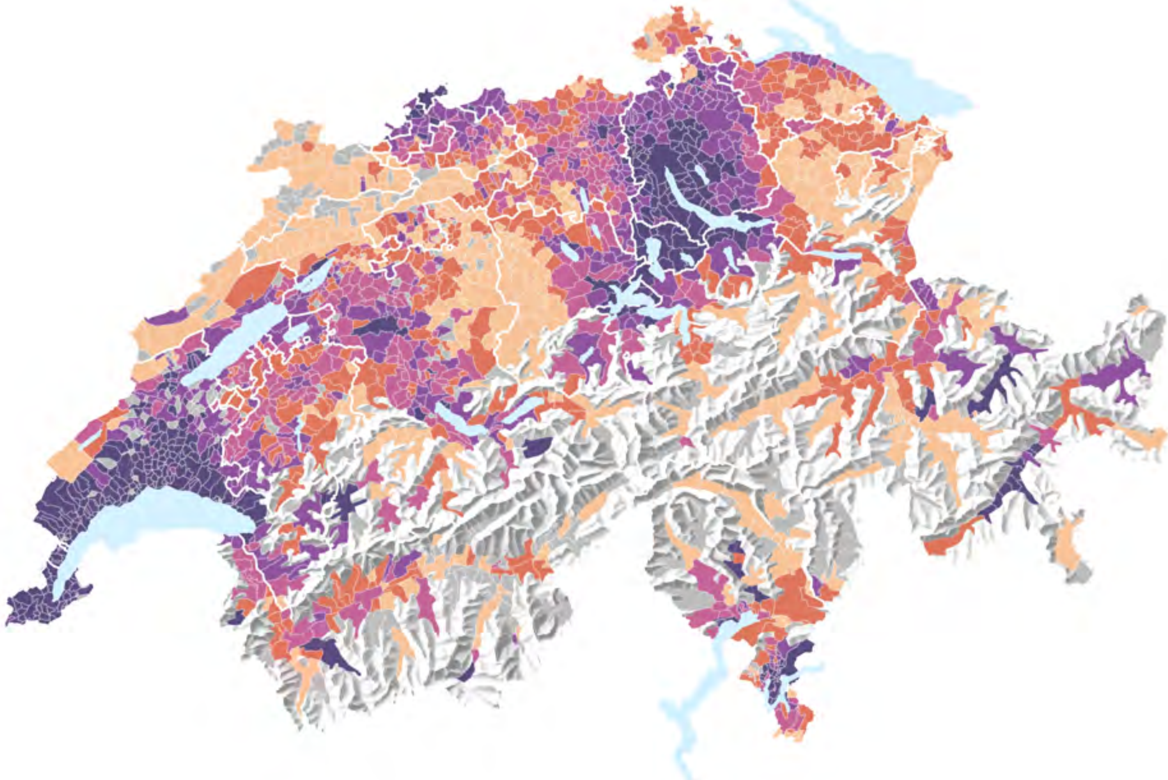
We combine individual-level data from multiple administrative registries for 2010–2022. Our core dataset is the Population and Households Statistics (STATPOP), which tracks all individuals and households in Switzerland over the 13 years. We link these records to yearly labor incomes from the Old Age and Survivors' Insurance (AHV). Each individual is matched to their apartment, enabling us to merge structural characteristics from the Federal Register of Buildings and Dwellings maintained by the Swiss Federal Statistical Office (FSO).

Further, we supplement this with the Structural Survey (SE), which covers a yearly sample of about 200,000 households. The SE provides socio-economic information such as education, residential status, and the net rent paid. To impute market rents for the unregulated segment (new contracts), we use the IAZI database on offered rents from 2004–2022.[14] This dataset includes detailed housing characteristics, precise geo-coordinates, and asking rents. Finally, we use a rich set of tenancy contracts curated by IAZI to back-test our results and assess model performance.

Figure 4 shows the spatial distribution of average rents across Swiss municipalities. Large cities such as Zurich and Geneva, as well as their surrounding areas, have the highest rents. High rents also appear in municipalities near lakes and tourist destinations like Interlaken and Grindelwald.

[14] Appendix B details the imputation of market rents for the unregulated segment.

Figure 4: Spatial Distribution of Rents on Unregulated Market Segment



5. Results for the estimation of housing demand parameters

We estimate heterogeneous housing preferences in three steps using (2) to (4). First, we model selection between movers and stayers and compute the Inverse Mills Ratio (IMR) for the next step. Second, we solve a conditional choice model by contraction mapping to recover heterogeneity in housing demand across household types. Third, we use the stage-two valuations to estimate inverse demand for the average household. These steps deliver willingness-to-pay (WTP) by household type and unit.

We implement four specifications that vary housing characteristics x_h and household characteristics z_k. The comprehensive model uses seven household and 12 unit dimensions. The benchmark uses six and nine. The minimal uses four and seven. A data-based model retains only statistically significant variables from the first stage, excluding the IMR as a household characteristic.

5.1. Selection stage estimates

Table 2 reports selection-model coefficients. Figure 5 plots IMR scores for movers and non-movers. The specification includes all household characteristics z_k from (5) and indicators for changes in marital status (newly married or newly separated).

The estimates show that transitions from single to married and married to divorced significantly raise moving propensity. Other coefficients align with expectations. The coefficient on *retired* may look surprising, but age fully identifies it.

The grouped histograms reveal apparent IMR differences between movers and non-movers. These differences are consistent with unobserved shocks tied to expectations, networks, or constraints. To address selection on unobservables when estimating the residential choice model on movers, we include the estimated IMR as a household characteristic in the indirect utility function, following Heckman (1979). The substantial overlap in IMR distributions provides common support for this control.

Figure 5: Distributions of inverse-Mills ratio

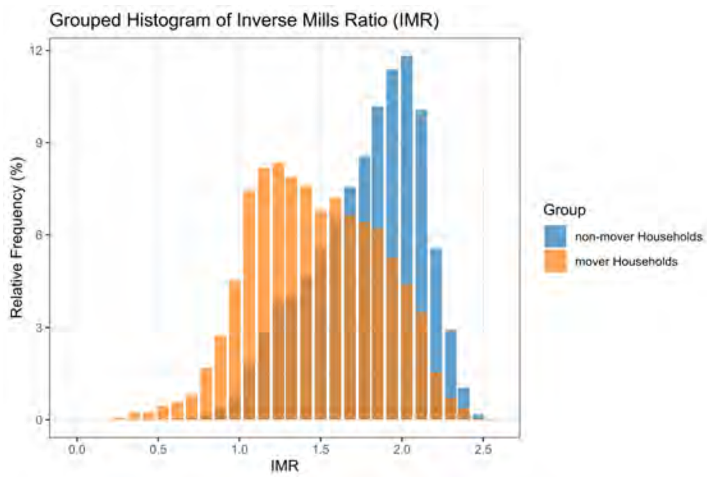
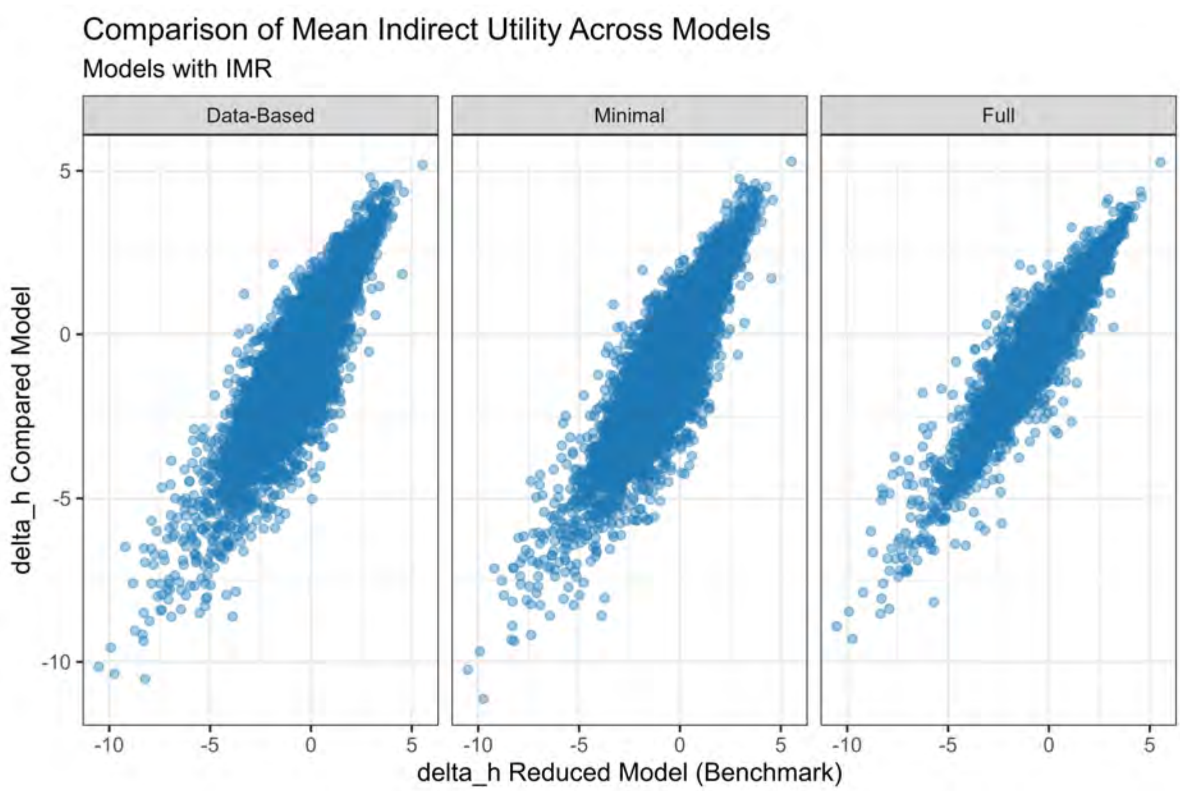


Table 2: Selection model estimates

	Moved = True
Δ Separation	1.399***
	(0.012)
Δ Marriage	0.313***
	(0.006)
Children U18	−0.042***
	(0.004)
HH Income	−0.044***
	(0.001)
HH Age	−1.332***
	(0.004)
HH Size	−0.084***
	(0.004)
Retired	0.091***
	(0.005)
Married	−0.079***
	(0.003)
Observations	115,933

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

Figure 6: Predicted average valuations across models



5.2. First-stage estimates

Table B.7 in Appendix B reports first-stage coefficients from (5). These coefficients are not directly interpretable in levels, but statistical significance reveals the direction and relevance of unit and household characteristics. Higher-income households, households with children, and larger households demand more living space, ceteris paribus. Price elasticity is larger for households with children and smaller for higher-income and older households. Multicollinearity cautions against strict causal interpretation. For example, the positive link between having children and high-income neighborhoods weakens once we account for the negative link between household size and neighborhood income.

Figure 6 compares recovered unit-level mean utilities δ_h from the baseline to three alternatives. The left panel uses the "data-based" model with only statistically significant interactions. The middle panel uses the minimal heterogeneity parameterization. The right panel uses the comprehensive heterogeneity specification. The δ_h align tightly across panels. Correlations are high, and slopes are close to one. Unit rankings remain stable across specifications. This supports δ_h as a credible approximation to the latent mean indirect utility for the average household. Figure C.12 in Appendix B shows additional stability checks across specifications.

5.3. Second-stage estimates

Table 3 decomposes the recovered δ_h into observable unit characteristics. Column (1) reports OLS estimates, which suffer from rent endogeneity. Column (2) shows the first stage of the 2SLS in (11), instrumenting rent with a shift-share measure. Column (3) presents the second-stage 2SLS estimates. The instrument is strong (first-stage $F = 612$). The 2SLS price semi-elasticity is -7.9, so a 1% rent increase lowers demand by about 8%.

Combining the price coefficient with the living-space coefficient yields an average substitution rate of $4.2/7.9 \approx 0.53$. Households pay about 0.53% higher rent for a 1% increase in living space.

Other coefficients show clear trade-offs. Households pay about 0.14% more rent for a 1% increase in neighborhood income. They pay about 0.11% more rent for a 1% reduction in building age, consistent with a premium for newer stock. The small and statistically insignificant effect for distance to public transport reflects measurement limits that omit service frequency, travel times, and high baseline accessibility.

These elasticities align with prior work. The estimated tax elasticity of rents is 0.28, within the Swiss range reported by Basten et al. (2017).

5.4. Willingness to pay estimates

Table 4 reports willingness to pay (WTP) from the residential choice model. Column (1) gives the average household's WTP for unit and neighborhood attributes in monthly

Table 3: Estimation of second-stage – average valuations

Dependent variable:	δ_h	$ln(p_h)$	δ_h
	OLS	First Stage	IV
	(1)	(2)	(3)
Net Rent	0.188^{***}		-7.943^{***}
	(0.010)		(0.389)
Living Surface	0.285^{***}	0.486^{***}	4.214^{***}
	(0.008)	(0.001)	(0.188)
Building Age	-0.024^{***}	-0.044^{***}	-0.377^{***}
	(0.002)	(0.0004)	(0.017)
Neighbourhood Income	-0.356^{***}	0.359^{***}	2.644^{***}
	(0.014)	(0.003)	(0.146)
Neighbourhood Population	-0.163^{***}	0.041^{***}	0.196^{***}
	(0.004)	(0.001)	(0.019)
Distance to School	0.037^{***}	0.0005	0.044^{***}
	(0.004)	(0.001)	(0.007)
Distance to Transit	-0.041^{***}	0.007^{***}	0.019^{***}
	(0.003)	(0.001)	(0.007)
Urban	-0.240^{***}	0.060^{***}	0.321^{***}
	(0.007)	(0.001)	(0.030)
Tax Burden	-0.493^{***}	-0.230^{***}	-2.198^{***}
	(0.043)	(0.008)	(0.114)
Weak Instrument F		612	
Observations	268,934	268,934	268,934
Note:			*p<0.1; **p<0.05; ***p<0.01

CHF for a one-unit change relative to the mean unit. The average household pays CHF 102 more monthly for an extra 10 m^2 of living space. It pays CHF 43 for a neighborhood with a CHF 10,000 higher average income, CHF 28 for a building 10 years newer, and CHF 22 for a CHF 1,000 reduction in annual taxes. The premium for an urban location is CHF 22 relative to the mean location. These averages match standard hedonic patterns.

Columns (2) to (8) show heterogeneity by household traits. Higher-income households have slightly higher WTP for space. A CHF 10,000 income increase raises WTP for 10 m^2 by about CHF 4 (from CHF 102 to CHF 106). Older households and households with children also value space more. Household size has the largest effect. One additional member raises WTP for 10 m^2 by about CHF 58.

Richer and older households value neighborhood income more, consistent with homophily. Larger households value neighborhood income less, reflecting a trade-off with affordability. Married households prefer urban locations more. Older households and households with children like them less, which points to demand for suburban or rural amenities. Retired households place a large negative value on building age and a large positive value on lower taxes, consistent with preferences for newer units and lower fixed costs, including senior housing.

We do not impose non-homotheticity or homophily. The data reveal these patterns. Preferences vary systematically with demographics, especially household size. This heterogeneity is central for counterfactual price and choice responses in Section 6.

Table 4: Heterogeneous Willingness to Pay (CHF)

		Household Characteristics (Dev. mean)						
Unit Characteristics (Dev. mean)	Mean WTP	Income	Age	Child	Household Size	Married	Retired	IMR
Area	101.88	3.75	9.16	4.36	57.73	-9.39	3.11	0.80
Neighborhood Income	43.49	2.86	1.17	5.93	-6.96	0.67	15.92	0.38
Urban	21.90	1.00	-8.49	-12.16	1.04	4.55	-15.70	15.30
Population Density	3.28	-0.02	-1.23	1.00	-0.12	-0.20	4.08	-0.37
Dist. School	2.62	-0.08	-1.97	-0.05	-1.14	-1.63	-9.15	1.77
Dist. Public Transport	1.60	0.46	3.36	2.01	1.54	-0.51	-6.48	-4.37
Taxes	-22.07	-1.55	-2.04	-7.74	8.32	-4.56	13.50	-8.71
Building Age	-28.16	-1.54	2.40	-1.01	2.40	-3.12	-35.45	-1.86
Dist. prev. Residence	-25.81			-1.47			-6.87	

5.5. Model evaluation

We evaluate the model by simulating household auctions using the estimated WTP. As detailed in Section 3.3, following Gilbert and French (2024), we implement a Hungarian auction to allocate units and extract prices. In each labor market, we draw 800 mover households and 800 units. Using Table 4, we compute each household's valuation for each unit to form an 800×800 matrix. We then run the auction to obtain the optimal assignment and the implied price vector. We repeat this procedure 1,000 times per labor market and weight markets by their population shares.

Figure 7 Panel (a) shows a near one-to-one relationship between estimated and observed rents. Panel (b) plots predicted rent-to-income shares against observed shares and shows a similar one-to-one fit. The assignment model replicates market outcomes at the unit and household margins. As in Gilbert and French (2024), rents do not enter this stage. Remarkably, we predict valuations only from observable unit and household characteristics.

Figure 7 Panel (c) shows that the relationship between predicted and observed rents also holds within labor markets.

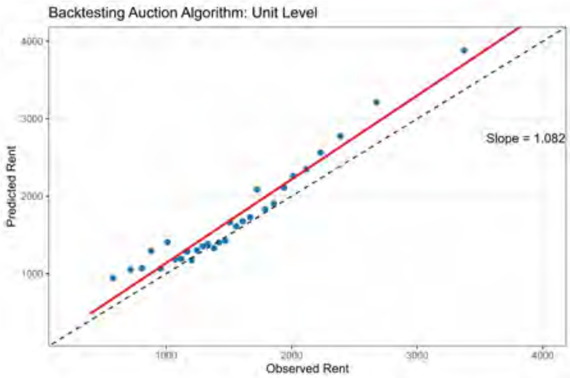
6. Counterfactual analysis: No tenancy rent control

We now examine counterfactual market rents without tenure regulation, that is, the rents that would prevail if the entire renter population competed over the whole stock of rental housing. Conceptually, the exercise removes the segmentation between regulated and unregulated market segments as incumbent (sitting) residents are now exposed to direct competition from newcomers by enforcing a single unified market that clears at one price for both groups.

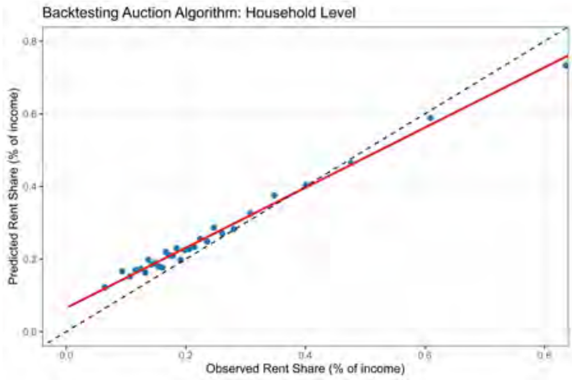
We proceed as follows. As in Section 5.5, we draw 800 households jointly with their units for each labor market.[15] Whereas the earlier analysis considered only mover households, the current draws consist of a representative mix of movers and stayers. For each draw, we calculate the valuation that each household assigns to each of the 800 units and use the Hungarian algorithm to determine the optimal assignment of households to units along with the vector of market-clearing rents. As before, the number of units

[15] Since the algorithm becomes computationally infeasible for larger samples, we restrict it to subsamples of 800 households and 800 units, resulting in an 800×800 WTP matrix. The procedure is repeated for many iterations, generating sufficient predictions for each unit–household type combination to support reliable inference.

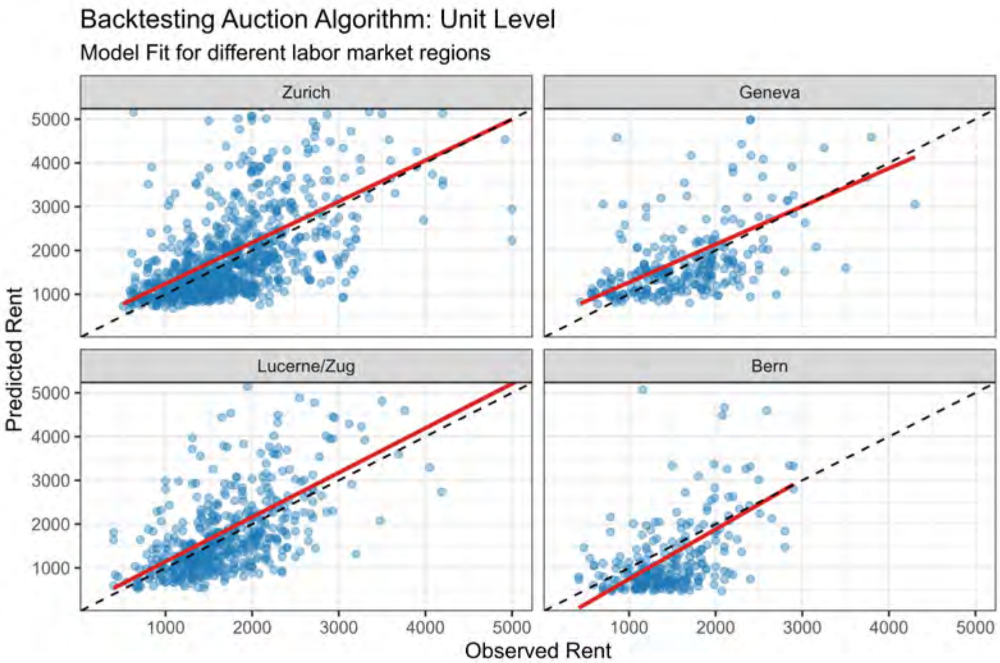
Figure 7: Model evaluation – backtesting



Panel (a): Predicted vs. observed rents by unit



Panel (b): Predicted vs. observed housing expenditure share by household



Panel (c): Predicted vs. observed rents by unit and labor market converted to outside options is dynamically determined by the share of immigrants in the drawn household sample.

This procedure is repeated 1,000 times for each labor market, yielding a distribution of predicted prices for each unit–household type combination. Specifically, we use the estimated preference parameters to compute the willingness to pay (WTP) for each household–unit pair within a labor market. The Hungarian algorithm—akin to an auction—then computes the equilibrium rents implied by these WTP values.

Sampling is done jointly, so each household is paired with its actual inhabited unit, corresponding to either the existing residence for stayers or the newly chosen unit for movers. Households thus enter the auction as both demanders (bidders) and suppliers (offering their current unit).[16] We run this procedure both for the counterfactual full-market scenario (yielding p') and for the unregulated mover-only market (yielding $\hat{p}^{ur}$). The latter can be compared to the observed p^{ur} to assess model validity (see Section 5.5). Iterating the procedure, we obtain multiple predictions of $\hat{p}^{ur}_h$ and p'_h for all units h, from which we compute the descriptive statistics per labor market region.

Jointly drawing households and their units means that each household has at least one choice at a minimal distance, namely, its current residence. The WTP for minimal distance reflects location attachment as if the household had resided at that location for the same number of years as the average mover in its original residence (this is the coefficient being estimated in the first stage). Essentially, this procedure assumes that households that moved to their current unit very recently (e.g., in the given year) have the same neighbourhood attachment as incumbents who have stayed there longer.

The simulation exercise presented in Section 5.5 considered distances from the mover's origin; this implied that moving distance to all alternatives was strictly above zero for most households. This is also the range of distances for which the distance coefficients are estimated, as this mirrors the sampling of non-chosen alternatives described in Section 3.1. The sampling procedure used to compute counterfactual scenarios produces average moving distances that are substantially smaller, as origins (i.e., current unit) are restricted to be within the considered labor market region. This raises potential concerns about neighbourhood attachment being modeled at an excessively granular level, and it requires stark extrapolation based on the estimated distance coefficient. We address these concerns as follows: In a first exercise, we impose a minimum distance of 4,000 meters (i.e., the average diameter of a municipality). In a second exercise, we impose

[16]We thus implicitly assume that supply is fully inelastic, with no new units being built and no existing units demolished.

a minimum distance of 500 meters (i.e., the definition of neighbourhoods used in the residential choice model). Figure E.14 reports how these changes affect the simulation output. Importantly, the almost one-to-one relation between simulated rents and the rent share of income is preserved. However, we now observe a level shift of ca. 900 CHF, indicating that we overestimate actual rents for the mover sample by this amount.

Table 5 reports the results for the first exercise, restricting distances to a minimum of 4000m.In Table 6 we report results of the second exercise, restricting distances to a minimum of 500m. We depict the obtained values of $\hat{p}_h^{ur}$ and p'_h for the different labor markets, as well as the Swiss average, which is a weighted average accounting for the size (in terms of units) of each labor market.

Because the actual duration of residence, and hence location attachment, systematically differs between movers and stayers, this aspect is being explored in more detail in later versions of this paper. In particular, we plan to employ configurations that either eliminate location attachment entirely or calibrate the corresponding effect by restricting moving distances of migrating movers to labor market region sizes. The configuration without neighborhood attachment can be achieved by using the preference coefficients estimated in the reference model, but dropping the distance coefficients when computing WTP. This should effectively turn off location attachment in the counterfactual analysis.

Furthermore, we will explore more sophisticated approaches that restrict the sample of stayers to those who have lived at their current residence for no more than 10 years, thereby reducing potential asymmetries in location attachment. In addition, we plan to interact tenancy duration with moving distance to capture the expected increase in location attachment throughout a tenancy. We will also employ a dedicated setting where target units are defined and then exposed to different samples of auction participants to confirm the robustness of the reported results.

Table 5 reports model predictions for median p^{ur} when only units from the unregulated market are supplied and only observed movers compete for them. Column (2) presents the predicted percentage difference between p' (the counterfactual unified-market price) and p^{ur}, using only units from the unregulated segment. For Switzerland, the median unit in the unregulated market would rent for about 8% less in a fully competitive, unified market absent regulation.

Since regulated and unregulated units differ in quality, with regulated units tending to be slightly older and lower quality, the counterfactual gap changes when computed across all units. Column (3) shows that, in this broader comparison, the median rent would be about 10% lower without regulation. Across local labor markets, the estimated rent reduction ranges from close to zero in regions Sottoceneri and Westalpen to over

15% for Berner Oberland. Results from the second exercise reveal even bigger spillovers ranging up to 28% in Berner Oberland.

Overall, the results for both exercises indicate sizable spillovers from regulation: by segmenting the market and protecting incumbent tenants, regulation pushes up rents in the unregulated segment by roughly 10% to 21%. Comparing this to the observed regulated–unregulated gap ($p^{ur}/p^{r} \approx 1.2$) implies that, under a unified market, regulated-segment households would face 8-16% higher rents, while unregulated-segment households would pay 10% - 21% less. Furthermore, the tenancy duration required to be an average net beneficiary of the regulation ranges between 8 and 12 years.

Table 5: Median Simulated Prices and Relative Differences: Municipality Attachment

	Median p^{ur}	$1-(p'/p^{ur})$ Only Units from Unregulated Market	$1-(p'/p^{ur})$ All Units
Aareland	1 885	5%	10%
Zentralschweiz	2 305	9%	12%
Zurich	2 477	7%	10%
Sopraceneri	1 680	2%	7%
Sottoceneri	1 638	0%	5%
Bodenseeregion	1 961	5%	6%
Ostalpen	2 054	6%	4%
Neuenburg	1 460	9%	9%
Freiburg	1 916	5%	10%
Biel-Jura	1 697	7%	10%
Bern	1 757	12%	11%
Westalpen	1 840	2%	1%
Basel*	1 982	7%	9%
Berner Oberland	1 726	15%	16%
Swiss Median	2 088	8%	10%

Note: The labor market regions of Geneva and Vaud are omitted because they have stricter rent regulations at the cantonal level. *The city of Basel introduced a stricter form of rent regulation in 2021; our analysis focuses on the year 2018.

In the following, we explore the incidence of tenancy control in more detail across different household types and compute the welfare costs caused by misallocation induced by tenancy control.

6.1. Incidence

To be completed: Incidence in different dimensions: at what age, what tenure duration, etc. We show for different labor market regions the incidence of tenancy control, i.e., at which duration of tenancy, what age, level of income, etc., households tend to be

Table 6: Median Simulated Prices and Relative Differences: Neighbourhood Attachment

	Median p^{ur}	$1-(p'/p^{ur})$ Only Units from Unregulated Market	$1-(p'/p^{ur})$ All Units
Aareland	2371	17%	21%
Zentralschweiz	2856	22%	22%
Zurich	3071	24%	24%
Sopraceneri	2406	15%	18%
Sottoceneri	2296	8%	11%
Bodenseeregion	2544	23%	23%
Ostalpen	2794	20%	18%
Neuenburg	2005	24%	19%
Freiburg	2506	19%	21%
Biel-Jura	2173	22%	22%
Bern	2397	22%	20%
Westalpen	2496	23%	17%
Basel*	2843	13%	14%
Berner Oberland	2178	27%	28%
Swiss Median	2731	21%	21%

Note: The labor market regions of Geneva and Vaud are omitted because they have stricter rent regulations at the cantonal level. *The city of Basel introduced a stricter form of rent regulation in 2021; our analysis focuses on the year 2018.

winners or losers of tenancy control. This is displayed in graphs similar to Figure 2 [17]

6.2. *Misalloction*

To be completed: We follow two ways to compute misallocation: First, we measure the efficiency loss from tenancy control by quantifying the degree of overconsumption (e.g., in square meters) of regulated households compared to the counterfactual. More specifically, we compute $x - x'$ and multiply with $1/2 \times (p^{UR} - p^{R})$. Second, we compute a full utility based measure accounting for all dimensions of unit and households heterogeneity by computing the consumer surplus – estimates of WTP minus prices i.e. $WTP_i - p^{ur}$, $WTP_i - p^{r}$, $WTP_i - p'$. – in the observed and the counterfactual scenario. Therby, we quantify the full misallocation by labor market region, household type, and unit type.

[17] Essentially, the y-axis in Figure 2 panel (a) - (c) would be shifted down by 10-20 percentage points. Indicating that, e.g., households with a tenancy duration below 10 years are net losers of the regulation.

7. Conclusion

Housing affordability and availability remain pressing concerns worldwide. Rent control policies continue to generate debate over their effectiveness and unintended consequences. This paper develops and implements a structural framework to quantify the general-equilibrium incidence of tenancy rent control. We combine administrative microdata on all Swiss households with a residential sorting model and a market-clearing assignment algorithm to recover counterfactual prices and allocations absent regulation.

Accounting for spillovers, we find that unregulated rents would be 8–21 percent lower without regulation. Reductions are largest in urban areas with inelastic supply. Incumbent tenants capture large implicit subsidies, concentrated among older, lower-income, and less-educated households. Moving households face higher rents than they would in the absence of regulation. The policy reduces mobility and induces overconsumption of space, generating misallocation and deadweight loss, especially in tight markets.

These results show that tenancy rent control shapes both the distribution and efficiency of housing consumption. It protects long-term tenants and delivers progressive transfers along some dimensions, yet it imposes substantial costs on mobile households and distorts allocation in high-demand areas. The proposed framework provides a transparent tool to evaluate these trade-offs and can be adapted to assess alternative policy designs or other institutional settings.

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Appendix A. Data Sources and Methodologies for Rent Gap Estimates

Figure 1 shows estimates of the rent gap between market (new-lease) rents and controlled (sitting-tenant) rents from reputable sources, each drawing on different data and methods. Donner and Kopsch (2023) measure the Swedish gap by comparing regulated rents with estimated market-clearing rents using a hedonic regression that controls for unit characteristics and location. Diamond et al. (2019a) estimate the Los Angeles gap as the average rent discount for rent-controlled tenants under the city's Rent Stabilization Ordinance, applying a hedonic regression to adjust for differences in units and neighborhoods. Canada Mortgage and Housing Corporation (2022) report the gaps for Toronto and Vancouver using a fixed-sample approach that compares average rents for the same set of units across years, distinguishing between turnover and non-turnover units. NYC Department of Housing Preservation and Development (2021) provide the New York estimate by reporting median rents for rent-stabilized and unregulated units. Residential Tenancies Board (2023) give the Ireland figure as median rents in existing versus new tenancies in Rent Pressure Zones. Observatoire des Loyers de l'Agglomération Parisienne (OLAP) (2022) supply the Paris figure, reporting average rent per square meter for new versus ongoing private tenancies, grouped by unit size category without hedonic controls. We calculate the Swiss gap ourselves from administrative rental data, comparing the average rent paid by sitting tenants to the estimated market rent for the same units. These estimates differ in methodology, but all reflect robust, locally accepted approaches. Readers should keep this heterogeneity in mind when comparing gaps across locations.

Appendix B. Data

Appendix B.1. Estimating rents in unregulated segment for units in regulated segment

First, we estimate how much a household benefits from the Swiss rent control policy. To do so, we calculate the gap between the (regulated) rent a household pays under its current lease agreement and the rent it would have to pay in the unregulated market segment. The latter represents the household's replacement costs (e.g., renting its unit under current conditions). As the corresponding rent on the unregulated market segment is unknown, for each observed rent within an existing contract (p^R_{ht}), we estimate the rent on the unregulated market segment (p_{ht}) based on machine learning:

We train a machine-learning model (XGBoost) to predict market rents. Below, we present a stylized version of the model to convey to the reader an intuitive understanding:

$$P^{UR}_{ht} = \beta_0 + \delta_t + \theta' X_{ht} + \lambda' N_{ht} + \kappa' M_{ht} + \epsilon_{ht} \tag{B.1}$$

The model is trained to predict the market rent (P^{UR}_{ht}) for unit h at time t. It takes into consideration dwelling characteristics (X), neighborhood characteristics (N), and municipality characteristics (M) as well as year-fixed effects (δ). The model is trained on over 900,000 units advertised for rent online. The IAZI training data is used to determine parameters ($\hat{\beta}_0, \hat{\delta}_t, \hat{\theta}, \hat{\lambda}$ and $\hat{\kappa}$). We use the trained model parameters to predict rents on the unregulated market segment for households within an existing tenancy:

$$GAP_{ht} = \hat{P}^{UR}_{ht} - P^R_{ht} \tag{B.2}$$

GAP_{ht} represents the benefit that the household living in unit h in neighbourhood n in municipality m at time t draws from the rent regulation. It is the difference between the rent the household would pay under current conditions at time t on the unregulated segment and the rent the household pays. To document the distributional consequences of the regulation, we are interested in estimating the following stylized regression. We replace subscript h (for unit) by i, indicating the household living in unit h, where Z_{it} is the households dimension of interest (e.g. income, age).

$$GAP^*_{ht} = \alpha + x_{ht}\beta + \varepsilon_{ht} \tag{B.3}$$

However, we only have a noisy measurement of GAP^*_i:

$$GAP_{ht} = GAP^*_{ht} + u_{ht} \tag{B.4}$$

Where GAP_{ht} represents our estimate, which is equal to the true GAP^*_h plus some noise, introduced by the fact that $\hat{R}entUnregulatedMarket_{it}$ in (B.2) is an estimate of the true rent in the unregulated segment.

Therefore, we can only estimate:

$$GAP_{ht} = \alpha + x_{ht}\beta + \varepsilon_{ht} + u_{ht} \tag{B.5}$$

In this case of attenuation bias (measurement error in outcome), the OLS estimate is not biased if three conditions are met:

$$1.\ E\left[u_{ht}\right] = 0 \qquad 2.\ Cov\left(u_{ht}, x_{ht}\right) = 0 \qquad 3.\ Cov\left(u_{ht}, \varepsilon_{ht}\right) = 0 \tag{B.6}$$

While we can not test whether these conditions are met, we can provide suggestive evidence. Training the algorithm (B.1) on the IAZI data set and then using it for prediction on the SE dataset provides us with a natural way to provide such evidence. A subsample (ca. 10%) of the households in the SE data only moved into their current unit in the year they were surveyed. For these households, the trained algorithm should predict the paid rent reasonably well - after all, the rent these households pay is based on current market conditions. For this sub-sample of households, the true GAP^*_{ht} should be equal to zero. Therefore, if $GAP^*_{ht} = 0$ then Equation (B.4) simplifies to $GAP_{ht} = u_{ht}$. In other words, for households with tenancy duration zero, any non-zero GAP_{ht} that we observe is only due to an error in our prediction.

Figure B.8: GAP_{ht} ($= u_{ht}$) for Households with Tenancy Duration Zero

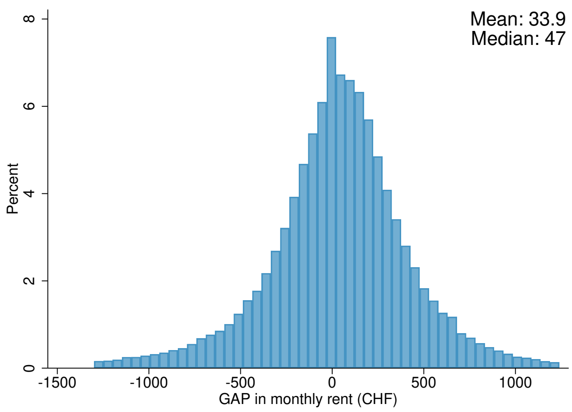


Figure B.8 shows the distribution of GAP_{ht} for households with tenancy duration zero. Our model slightly underestimates actual rents, on average the model is off by 33 Swiss Francs or 2.75%. Therefore, we do not meet $E\left[u_{ht}\right] = 0$ perfectly, but the mean error is very close to zero, and any remaining difference will be absorbed by the constant in (B.5). In Figures B.9 and B.10, we assess the plausibility that we meet the second condition ($Cov\,(u_{ht}, x_{ht}) = 0$). While the quality of our rent estimate in the unregulated segment (based on unit characteristics) does not differ by household age, it does systematically by household income. Currently, we slightly underestimate the true rent for richer households, both in absolute and relative terms. This is problematic as it will bias our results from (B.5). To assess the plausibility that we meet the third condition ($Cov\,(u_{ht}, \varepsilon_{ht}) = 0$), we test whether the quality of our prediction is systematically correlated with unit characteristics. While this is not the case for most characteristics, we systematically overestimate the rent for buildings constructed before 1980.

Figure B.9: Living Space Consumption by Age

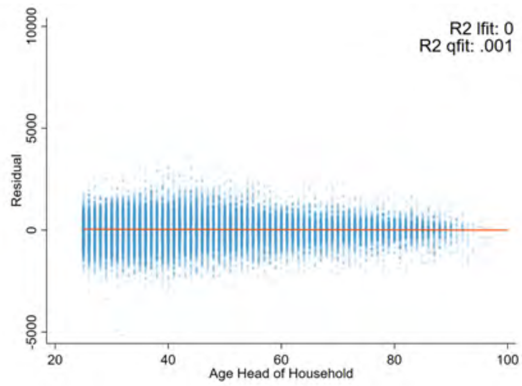
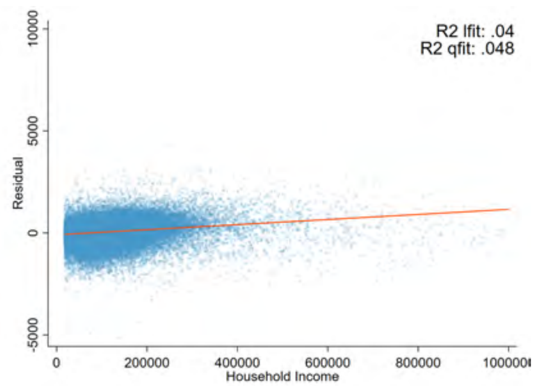


Figure B.10: Living Space Consumption by Age



Appendix B.2. Estimating rents absent regulation

As outlined in Section 2, comparing the rent a household is currently paying to the rent the household would have to pay on the unregulated segment does not accurately capture the true effect of the rent regulation scheme. Absent regulation, there would be no spillover between regulated and unregulated segments. Therefore, our measure GAP_{ht} is informative on the relative benefits households draw from the regulation but not on the absolute benefit. Most importantly, we do not observe the rent that would manifest absent regulation. Therefore, we do not know which households have a positive and which households have a negative benefit. In the next step, we will estimate the counterfactual rent absent rent regulation by employing exogenous variation in the local demand elasticity.

Figure B.11: Distribution of rental changes following a tenant change

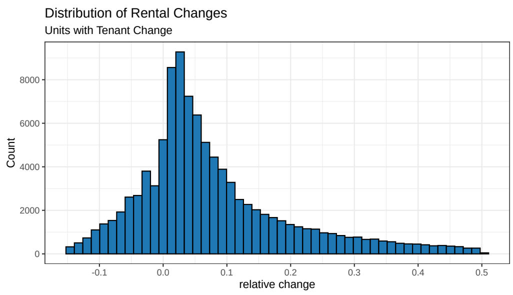
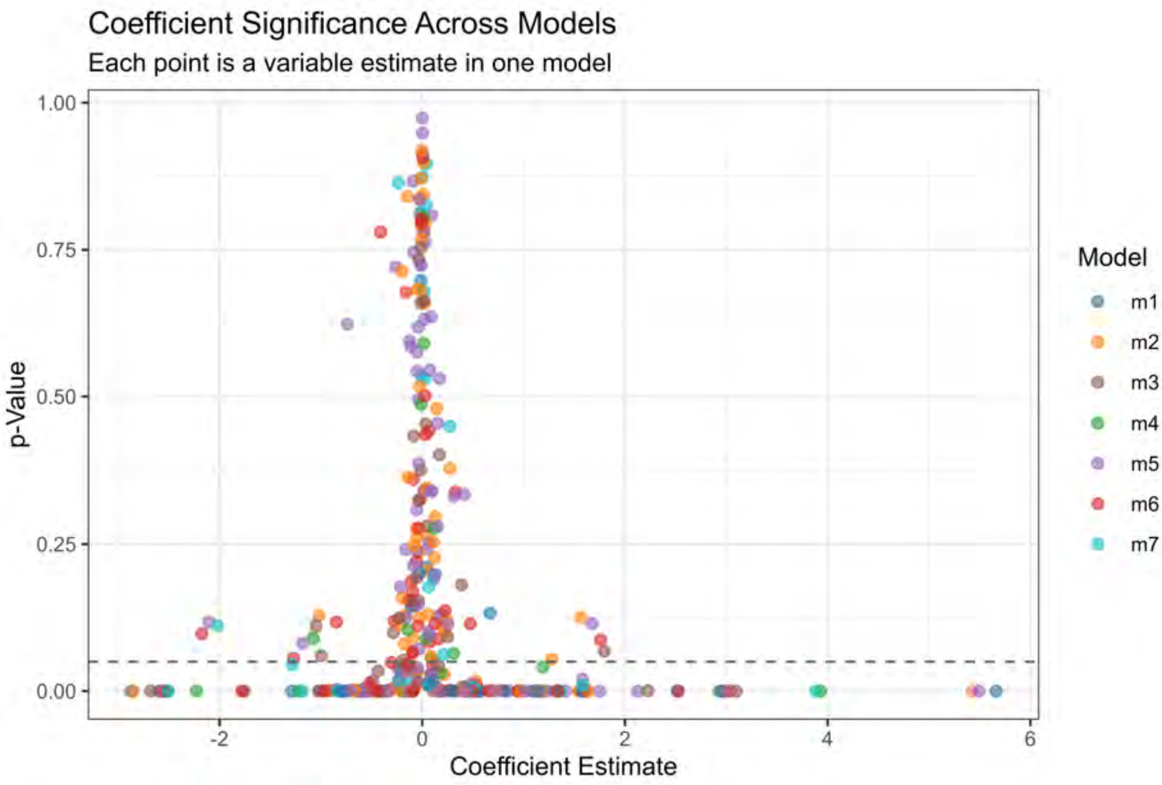


Table B.7: Interaction Coefficients with Standard Errors in Parentheses. Bold coefficients are statistically significant at the 5% level.

Variable	**Child U18**	**HH Income**	**Age Max**	**HH Size**	**Retired**	**Married**	**IMR**
Wareas	**0.525**	**0.460**	**0.783**	**2.984**	-0.006	**-0.347**	**0.561**
	(0.127)	(0.040)	(0.240)	(0.117)	(0.180)	(0.104)	(0.211)
Building Age	-0.044	**-0.106**	**0.197**	**0.205**	**-0.487**	**-0.088**	-0.097
	(0.026)	(0.011)	(0.058)	(0.026)	(0.041)	(0.021)	(0.051)
Nbr Income	**0.680**	**0.971**	-0.164	**-1.819**	**0.895**	0.217	0.358
	(0.187)	(0.067)	(0.390)	(0.182)	(0.304)	(0.152)	(0.344)
Nbr Pop Tot	0.097	-0.052	**-0.335**	-0.087	**0.244**	-0.007	-0.024
	(0.052)	(0.020)	(0.102)	(0.051)	(0.086)	(0.043)	(0.089)
School Quality	0.002	-0.020	-0.142	-0.054	**-0.158**	-0.040	0.067
	(0.048)	(0.018)	(0.098)	(0.047)	(0.074)	(0.039)	(0.086)
Urban	**-0.232**	0.062	**-0.563**	-0.089	**-0.246**	**0.122**	**0.506**
	(0.100)	(0.039)	(0.200)	(0.098)	(0.158)	(0.081)	(0.175)
Tax ¿200k	-1.254	-0.919	-0.464	**2.480**	1.458	-0.843	-2.112
	(0.666)	(0.267)	(1.478)	(0.661)	(1.057)	(0.542)	(1.314)
Transit Stops	0.036	0.045	**0.164**	0.030	-0.082	-0.008	-0.110
	(0.045)	(0.017)	(0.093)	(0.045)	(0.070)	(0.037)	(0.081)
Net Rent	**-0.520**	**1.633**	**1.359**	**2.955**	0.252	**-0.461**	**-0.929**
	(0.141)	(0.050)	(0.293)	(0.138)	(0.215)	(0.117)	(0.259)
Distance Prev. Home	**-0.374**			**-0.624**			
	(0.035)			(0.063)			

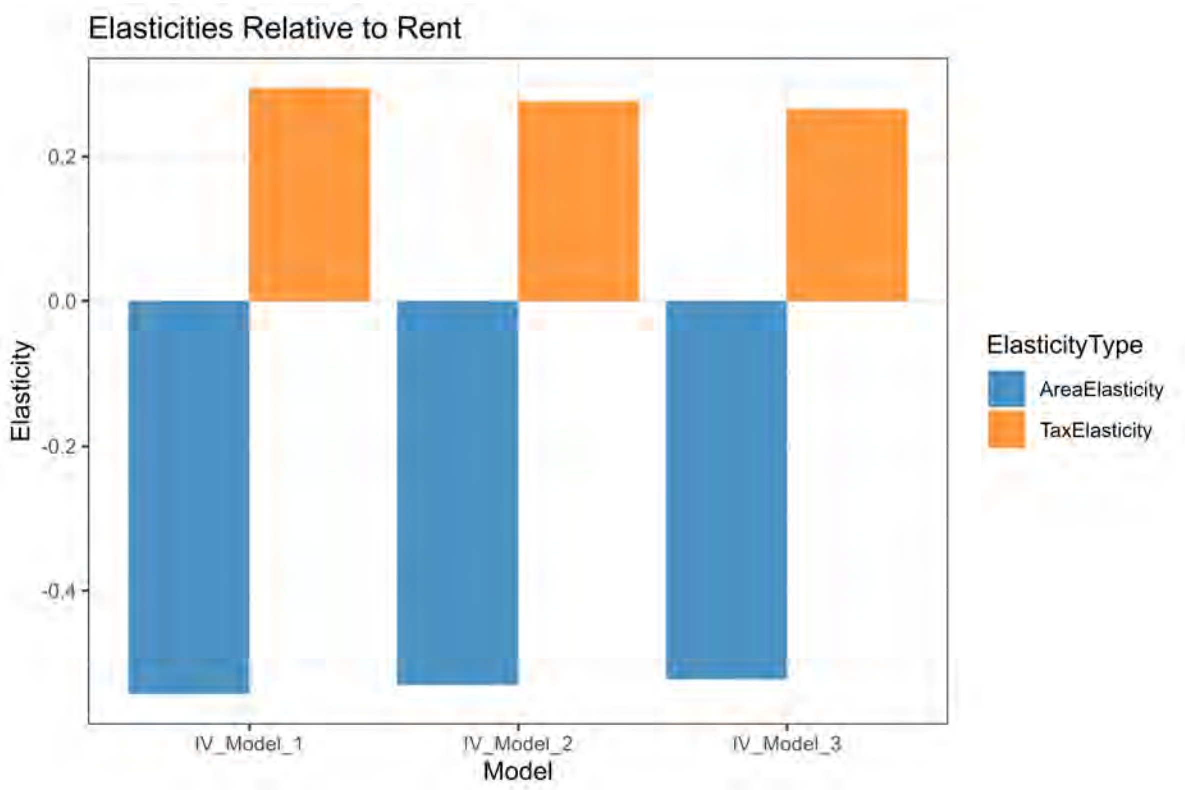
Appendix C. First-stage

Figure C.12: Comparison of beta- and p-values across model specifications



Appendix D. Second-stage

Figure D.13: Estimates elasticities for different instruments



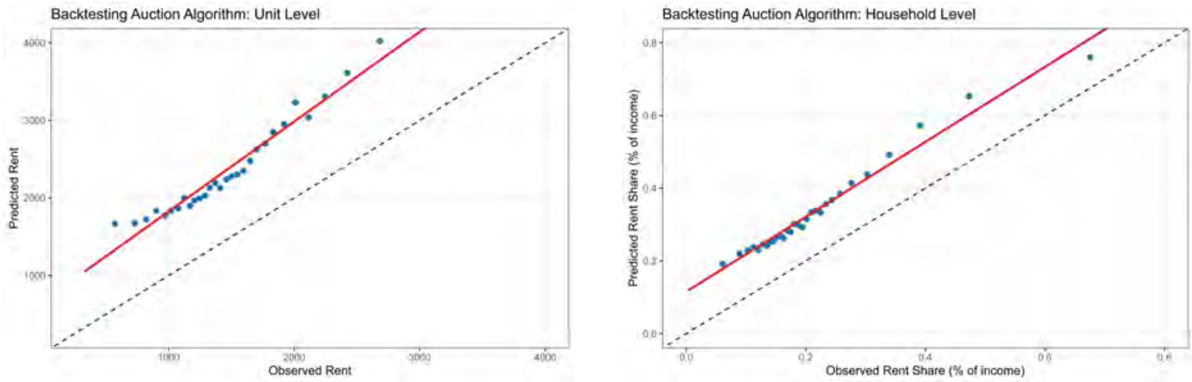
Appendix E. Further Results

Table E.8: Determinants of Moving Propensity

	(1)	(2)	(3)
GAP	-0.662***	-0.394***	-0.263***
	(20.02)	(11.36)	(7.08)
Δ Income	0.0581***	0.0574***	0.0362**
	(5.53)	(5.41)	(3.21)
Δ Children	0.498***	0.400***	0.375***
	(12.39)	(9.92)	(9.25)
Age	-0.0194***	-0.0127***	-0.0140***
	(27.97)	(17.11)	(18.32)
Rooms	-0.281***	-0.254***	-0.254***
	(31.46)	(28.32)	(27.06)
Tenancy Duration		-0.0478***	-0.0435***
		(29.04)	(26.07)
Labor Market, Education and Residence Permit FE			Yes
Observations	120,761	120,761	120,761

Notes: Robust t-statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Based on specification (3): At the 25th percentile of the GAP variable, the predicted probability of moving is 12.7%; at the 75th percentile, it decreases to 11.7%.

Figure E.14: Model evaluation – backtesting



Panel (a): Predicted vs. observed rents by unit

Panel (b): Predicted vs. observed housing expenditure share by household

Appendix F. More Descriptives