The Impact of New Housing Supply on the Distribution of Rents*

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Abstract

I estimate the impact of market-rate new housing supply on the local rent distribution. As an exogenous shifter of new housing supply, I exploit local weather shocks during the construction phase that lead to temporary delays in housing completions at the municipal level. Adding one new housing unit to the stock for every 100 rental housing units offered on the market in a given month reduces rents by 0.4–0.7%. A series of instrumental variable quantile regressions show that shocks to new housing supply shift the rent distribution as a whole, suggesting that market-rate new housing supply effectively reduces housing costs of all renter households. I rationalize this finding by analyzing moving decisions in the German Socio-Economic Panel. The housing quality at a household's previous address is a poor predictor of the housing quality at the current address, suggesting that new housing supply triggers supply of (rental) housing units across the housing quality spectrum. [153 words]

Keywords: Rental housing; new housing supply; housing demand elasticity; housing supply elasticity; filtering.

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1. Introduction

In recent years, housing costs have increased substantially in many places around the world. This evolution seems most dramatic in productive, amenable places that are attractive to workers and firms, but where housing supply elasticities are typically low. Prominent examples are San Francisco, New York, London, Tokyo, or Paris. The rising housing costs have triggered various policy responses. However, most–if not all–of these policies lead to considerable distortions or have miniscule quantitative effects (see Metcalf, 2018, for a recent survey). At the same time, there seems to exist little confidence in private markets as a source of affordable housing. This is despite the fact that theories of filtering have been proposed at least since the seminal contributions of Sweeney (1974b,a). According to this line of reasoning, private markets provide affordable housing by matching household income to housing unit quality, or building age. If filtering works well in a housing market, the supply of newly built, high-quality housing units should reduce rents throughout the rent distribution.

In this paper, I exploit clearly exogenous weather shocks as a temporary shifter of new housing supply at the municipal level in order to identify the effect of new housing supply on the local distribution of private-market rents. I rely on a unique administrative data set of the universe of building completions in Germany between 2010 and 2017. I combine the buildings data with a large data set of rental housing units offered online, on the three largest real estate and rental housing market places in Germany. The data cover Germany as a whole over the period 2011-2018. Finally, I use georeferenced daily rainfall data provided by the German weather service.

In order to identify exogenous shifts in local housing supply, I rely on rainfall shocks during the construction phase. Weather shocks can cause delays in the construction process of housing, leading to temporary reductions of new housing supply

(Coulson and Richard, 1996; Fergus, 1999). I make use of the fact that the building data contain information on the month of completion, which allows me to use monthly variation in weather to identify monthly variation in housing completions. I find that the instrument is relevant: Unusual rainfall spells during the summer reduce significantly the number of housing completions in the following December. I also provide evidence that the delays are temporary and last several months to one year. Moreover, the rainfall shocks are clearly exogenous: Rainfall during the preceding summer affects today's rents only through the supply of new housing.[1]

I then use the variation in housing completions to estimate a price elasticity of local housing demand, building on a simple model of a local housing market. An expansion of housing supply by 0.1% of the stock causes a decrease of mean private-market rent per square meter by approximately 3%. If new supply is measured as a percentage of the *flow* of rental unit, the estimates imply that adding one new housing unit to the housing stock for every 100 rental housing units offered on the market per month reduces rents by $0.004 - 0.007$ log points. Quite remarkably, the effect is visible already in the month of completion, whereas no effect can be found in the months before this. As an example, real rents in Munich increased by about 4.9% per year over the period 2011–2018. The estimates suggest that real rents in Munich would have been stable over that period, if 21 additional new units had been supplied to the market for every 100 new units that were completed over this period.

In the simple theoretical model, the combination of the estimated housing demand elasticity with estimates of the housing expenditure share and the rents-earnings elasticity yield an estimate of the housing supply elasticity in Germany during the

[1] In the regressions, I also condition on location and year fixed effects. The rainfall shocks are demeaned, so that they capture summers that are unusually wet or dry relative to the average summer at the location.

latest boom, 2011–2017. I find that the elasticity ranges between 3.5 in the short- and 5.8 in the medium-term for Germany as a whole, in a regression with individual housing units as the units of observations. This regression implicitly weighs strongest the largest rental markets of Germany. In a regression that gives relatively more weight to smaller districts, the elasticity is 16, suggesting that peripheral locations have much more elastic housing supply, as expected.

The German private rental market is qualitatively diverse. There is no stigma associated with renting, and home ownership rates are very low by international standards. This makes the German rental market a perfect test bed for analyzing the impact of new housing supply on the distribution of rents.[2] Clearly, new housing units are of relatively high quality, as compared to older ones. This suggests that they are close substitutes to other housing units located at the top end of the rent distribution. However, if potential utility gains from moving are fairly small, moving costs could prevent agents from realizing these gains. This makes an unambiguous theoretical prediction difficult. A series of instrumental quantile regressions (IVQR) reveal that the rent distribution as a whole shifts in response to new housing supply. The results indicate that—if anything—the lower end of the distribution reacts first. Overall, the differences are small, suggesting that new housing supply leads to a location shift of distribution.

In order to rationalize these findings, I analyze moving decision of households in the German Socio-Economic Panel (GSOEP). Two main results emerge from this analysis: (1) Owner households in Germany are very immobile, while renters move house more often. This suggests that new housing supply triggers moves mostly among renter households, which could explain the immediacy of the impact on rents.

[2] Here and in the following, rents refer to rents per square meter, unless noted otherwise.

(2) Conditional on moving, households with higher incomes choose to move into new housing units. In contrast, the quality of the previous housing unit, as proxied by the building age, is a comparably poor predictor, and it varies substantially among mover households that choose newly built housing units. Overall, this suggests that market-rate new housing supply triggers the supply of a variety of rental housing units that differ substantially in their quality. A direct implication is that new housing supply can help greatly to reduce the housing costs among households with lower incomes.

The paper's main contribution is three-fold: First, to the best of my knowledge, the paper is the first to provide clean, quasi-experimental evidence that new housing supply by private markets reduces effectively the housing cost burden of *all* renter households. This finding has very important implications for housing policy in general. To the best of my knowledge, this is the first paper that investigates this link directly, in a quasi-experimental setting. Moreover, I base my analysis on rents rather than house prices, a relatively clean measure of housing costs. Rents are largely unaffected by real interest rates, house price expectations, or household discount factors.

Second, the paper provides a causal estimate of a (local) housing demand elasticity. This is a key component of regional models with downward-sloping local housing demand curves, and this elasticity is central to local planning decisions. It can inform planners how many units need to be built in order to keep rents constant—the flip-side of the typical policy prescription to loosen housing supply constraints (which cannot be measured easily).

Third, I provide a structural estimate of the housing supply elasticity in Germany. Housing supply elasticities are important components of macroeconomic models of the housing market, because they govern the responsiveness of prices to demand shocks.

This paper ties into three important strands of the literature on housing markets. First, the paper adds to a small, but growing empirical literature on filtering and the effects of new housing supply by private markets (Mast, 2019; Nathanson, 2019; Rosenthal, 2014, 2019). The paper is most closely related to Nathanson (2019) and Mast (2019). Nathanson (2019) builds a structural model of moving choices in response to new housing supply. He finds that—under certain conditions—new high-quality housing supply improves welfare among poor households. Mast (2019) builds a data set of "moving chains", triggered by new housing supply at market rates. He also finds that the moving chains quickly reach poorer neighborhoods, in the sense that households leave these neighborhoods when housing units are completed in richer neighborhoods. However, neither Nathanson (2019), nor Mast (2019) consider the impact of new supply on the house price distribution in a quasi-experimental setting. This paper fills this gap. It extends the results of the aforementioned papers based on a rich data set that covers several years and an entire country and a clean identification strategy. Importantly, both the main results and the supplementary evidence are consistent with earlier findings.

Second, there is a vast literature on the intended and unintended consequences of housing policies, including demand- (Collinson and Ganong, 2018; Eriksen and Ross, 2015; Gibbons and Manning, 2006; Fack, 2006) and supply side policies (Diamond and McQuade, 2019; Aliprantis and Hartley, 2015; Eriksen and Rosenthal, 2010; Baum-Snow and Marion, 2009), as well the regulation of market prices (Diamond et al., 2019; Mense et al., 2019) and market participants (Hilber and Schöni, 2018). Although some of these policies are found to be effective, a common theme in this literature is the inability of most housing policies to reduce the housing cost burden of low-income households on a broader scale (Metcalf, 2018). Although the present paper does not study a particular housing policy, its results strongly suggest that effective housing policy should focus on constraints to housing supply, and on the supply side in generally.

Third, numerous authors have pointed to the strong impact of housing supply constraints on house prices (Hilber and Vermeulen, 2016; Gyourko et al., 2013; Saiz, 2010; Quigley and Raphael, 2005; Glaeser et al., 2005). Typically, this literature considers the differential impacts of demand shocks across space on the price of owner-occupied housing. The results in this paper lend further support to the policy advice that emerges from this literature, namely that policy makes should focus on (regulatory) constraints that prevent an adequate reaction of supply to (local) shocks to housing demand. Moreover, this paper provides a view from a different angle, by looking directly at the effect of new housing supply on the user cost of housing. This way, the results also validate an implicit assumption of the housing supply literature.

The remainder of this paper is structured as follows: Section 2 describes in detail the rainfall instrument. In section 3, I analyze the effects of new housing supply on average rents. The section builds on a simple model of a local housing market that I use to derive the housing supply elasticity. Section 4 provides evidence for the impact of new housing supply on the tails of the local rent distribution. In the final section, I offer some conclusions for policy and future work.

2. Rainfall Shocks as an Instrument for New Housing Supply

In order to identify shifts in new housing supply, I exploit fluctuations in housing completions in December that are caused by bad weather conditions during the preceding summer. Previous studies have found that local weather conditions influence the number of housing completions (see, e.g. Fergus, 1999, for the U.S.). Typically, housing construction starts in early spring, and developers usually erect the building walls until mid-summer. In this period, heavy rainfall may lead to delays because concrete, bonding agents, and certain other materials cannot be applied when there is heavy rainfall.[3] Secondly, on sunny days, construction work in the summer months is possible between the early morning hours until the late evening without electric light. To the contrary, on cloudy days with rainfall, the "effective daytime" is shortened considerably, making it more costly to build. Thirdly, many building materials, such as concrete and mortar, need to dry up before roof and windows can be closed. Otherwise, moisture can lead to damages, and encourage mold inside the building. If it is too wet in the summer, this process takes longer, so that construction work cannot be completed before the winter.[4]

Winters in Germany are usually too cold and too windy to allow outside construction work on buildings, and most types of plaster and concrete cannot be handled below certain temperatures.[5] Consequently, outside work has to be completed by late November in most areas of Germany. Hence, if a wet summer prolongs drying times into October or November, the building cannot be completed before the winter, and construction work can only resume once the winter is over.

Figures 1 A and B provide evidence for these arguments. The figures plot the share of houses completed in November (A) and December (B) against the average

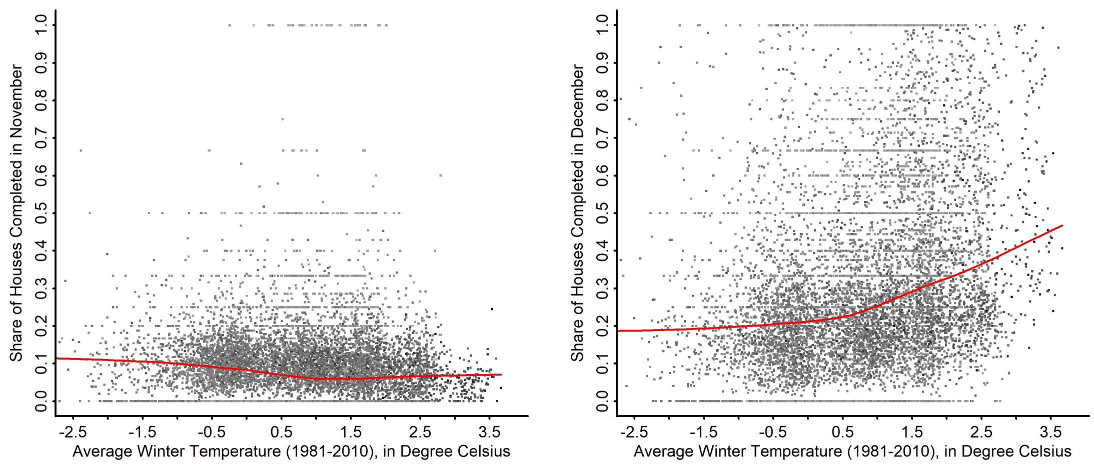
[3]See https://www.nwzonline.de/bauen-wohnen/hausbau-am-besten-im-fruehjahr-starten_a_1,0,2996588202.html

[4]There is no official statistic on building starts in Germany, and I am not aware of a data set that documents the timing of the construction process. However, various newspaper and magazine articles suggest that most housing starts occur in late winter or early spring, and that walls are erected within approximately four to five months, e.g. https://www.immonet.de/service/zeitplanung-hausbau.html, https://www.hausausstellung.de/news-anzeigen/wann-ist-der-beste-zeitpunkt-fuer-den-hausbau-1659.html, or https://www.n-tv.de/ratgeber/Wann-ist-die-beste-Zeit-fuer-den-Baubeginn-article19787710.html. Moreover, the building completions statistic reports a substantial share of housing units for which the date of the building permit and the date of completion lie in the same calendar year.

[5]Many materials require outside temperatures above five degree Celsius. Although it is technologically feasible to build also in a cold winter, this increases tremendously the construction costs (see, e.g., Wilke, F. (2016) "Fünf Grad, die magische Grenze" [*Five degree Celsius, the magic threshold*], *Sueddeutsche Zeitung* January 1 2016, https://www.sueddeutsche.de/geld/bauen-fuenf-grad-die-magische-grenze-1.2800713). Bad weather conditions as a reason for an extension of building time are recognized by German building law, see §6 Abs. 2 Nr. 1 VOB/B.

winter temperature at the location.[6] In places in Germany with lower average winter temperatures, relatively more units are completed in November, but the relationship is very weak (A). In December, relatively fewer units are completed in these places (B). The relationship is much stronger in this case, suggesting that December temperatures represent a binding constraint in many areas in Germany. Moreover, it is non-linear, with a clear kink between -0.5 and $+0.5$ degrees Celsius. This reflects the fact that construction work is possible only above certain temperatures that, in some places, are not reached very often during the winter. Overall, this clearly shows that winter temperatures can be a barrier to finishing housing construction.

Figure 1: Houses Completed in November and December, and Average Winter Temperature



A. Share completed in November

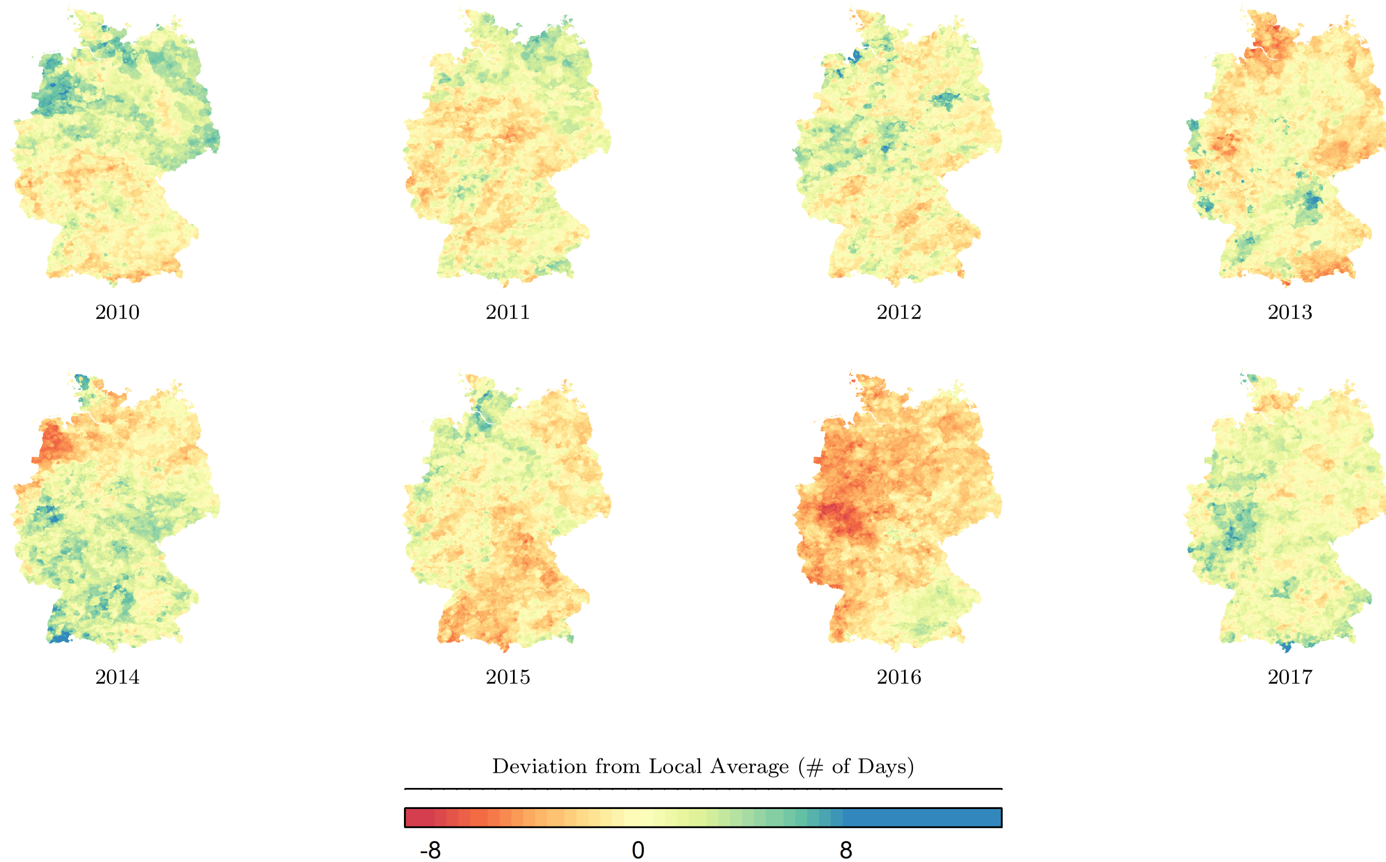
B. Share completed in December

Note: The two graphs plot the share of units completed in November (A) and December (B) against the average winter temperature (Dec/Jan/Feb, 1981–2010). Each dot represents a municipality-year. The red lines are loess fits, with span 2/3 and degree 1. The grey shading corresponds to the size of the municipality, with darker shades representing larger municipalities.

The basis for the instrumental variable are data on daily rainfall by 1×1 km^2 grid cells, provided by the German Weather Service for 2010–2017. I first calculate, for

[6]The winter temperature is aggregated from 1×1 km^2 grid data provided by the German Weather Service. It refers to the average temperature in Degrees Celsius in during the winter months December, January, and February, measured over the period 1981–2010.

Figure 2: Spatial and Temporal Variation in the Instrument



Notes: Each graph displays the variation in the rainfall shock, by municipality. The rainfall shock is measured as the number of consecutive days with rainfall above 20mm during the summer months (Jul-Aug-Sep), relative to the average number of consecutive rainfall days at the location during the summer months. A larger number indicates more rainfall in the particular year than in an average year. This variable is used as instrumental variable in the IV rents regressions below.

each grid cell and month, the largest number of consecutive days with rainfall above 20mm per square meter, which I refer to as a "rainfall spell". In order to control for time-constant differences in weather between different locations, I subtract from each grid cell the grid cell mean of the particular month. I then aggregate the resulting variable by municipality and year-month. The instrument used in the regressions is the sum of this rainfall spell variable in the three months July, August, and September. This is a variable that varies by municipality and year, and it predicts well the number of housing completions in December of the same year (as I show below). In order to further minimize the threat that time-constant confounders, aggregate shocks, or sample composition bias the regression results, I additionally control for municipality and year fixed effects, as well as for housing characteristics. To summarize, the identifying variation comes from deviations of weather conditions from the weather conditions that are typical at that location or in the particular year. Figure 2 displays the spatio-temporal variation in the instrument.[7]

Even conditional on covariates, the local summer rainfall shock is a strong predictor of December housing completions. Table 1 displays the results from a set of regressions with the summer rainfall shock as the explanatory variable. The units of observation are municipalities by year. In regressions (1) to (3), the dependent variable is the number of new housing units completed in December. The baseline model is a plain bivariate regression, and the coefficient is highly significant and negative, as expected. It is very stable when year- and municipality-fixed effects are added in regressions (2) and (3). Models (4) to (6) are variants of these regressions, where the

[7]Taking the actual rainfall instead of the demeaned rainfall leads to similar results when controlling for location fixed effects. The main difference comes from the aggregation within municipalities. When each grid cell is demeaned separately, the measure is cleaner in municipalities where rainfall tends to be clustered spatially, such as in mountainous areas. I therefore choose to demean by grid cell before aggregation.

dependent variable is scaled by the number of units in the housing stock × 1000, as measured in the last census in 2011. Once again, all coefficients are highly significant and very stable across specifications.

Table 1: Summer Rainfall Spells and December Completions

Dependent variable:	New housing units completed in December					
	in total			as share of the stock × 1000		
	(1)	(2)	(3)	(4)	(5)	(6)
Summer rainfall shock (deviation from local average)	-0.167*** (0.027)	-0.147*** (0.030)	-0.151*** (0.032)	-0.031*** (0.007)	-0.042*** (0.008)	-0.043*** (0.009)
Year-FE	no	yes	yes	no	yes	yes
Municipality-FE	no	no	yes	no	no	yes
R^2	0.0002	0.0008	0.7595	0.0001	0.0012	0.1902
R^2 (projected model)	0.0002	0.0001	0.0004	0.0001	0.0002	0.0003
Observations	86,032	86,032	86,032	86,032	86,032	86,032

Note: Standard errors are clustered by municipality; *: $p < .05$, **: $p < .01$, ***: $p < .001$.

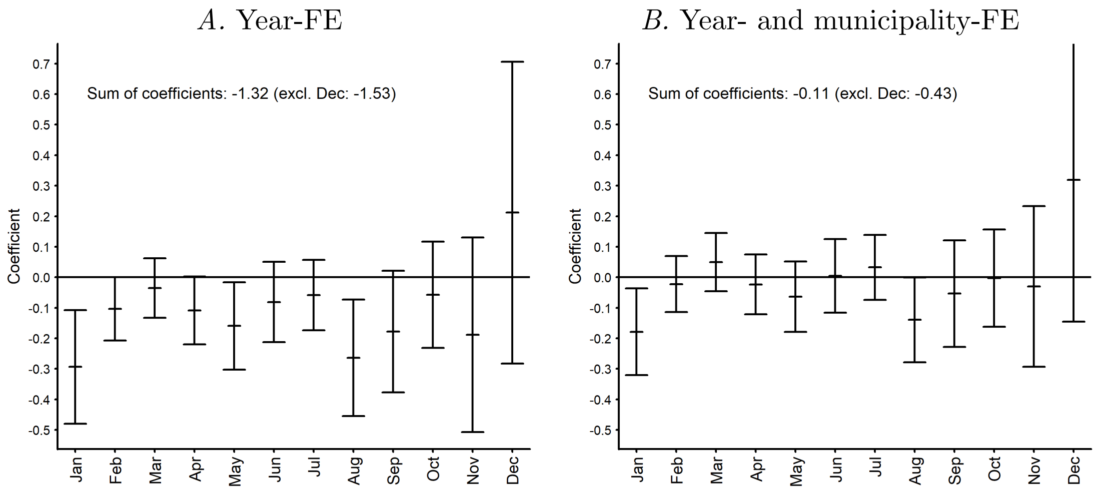
It is unlikely that rainfall shock have permanent effects on the number of building completions. Rather, they should lead to a temporary delay. If this delay is very short-lived, it might not lead to measurable effects on the rent distribution. Here, I consider building completions rather than individual housing units, because the building is the unit that is "treated" by the weather shock. To investigate the average length of the delay, I regress the the number of residential building completions in month m of year t, $B_{t,i}^{(m)}$ on the number of December completions in year $t-1$, $B_{t-1,i}^{(12)}$, and on year and municipality fixed effects.

$$B_{t,i}^{(m)} = \psi_i + \phi_t + \beta_m B_{t-1,i}^{(12)} + \varepsilon_{i,t}, \quad m = 1, ..., 12. \tag{1}$$

Figures 3 A and B display the β_m coefficients for $m = 1, ..., 12$ and 95% confidence intervals, whereby each point corresponds to one regression. Panel A is a regression with just year fixed effects, and Panel B also includes municipality fixed effects. Standard errors are clustered by municipality. The coefficients in the graph represent the impact on the number of buildings completed in the given month of one additional building completed in the preceding December, while variation in the December building completions is due to unusual weather conditions during the construction phase. On average, more building completions due to good weather conditions reduce the number of building completions in the subsequent year, as expected. If the rainfall shocks cause temporary delays, the sum of the coefficients should be equal to -1. In fact, it is reasonably close to -1 in Panel A, when controlling only for year fixed effects. According to this graph, the impact is temporary, dissipates gradually, and lasts close to one year. When adding municipality fixed effects in Panel B, the impact is reduced somewhat, suggesting more persistent delays. Potentially, the weather shocks could lead to longer-lasting delays if local housing construction is at its maximum capacity and the weather shocks reduce temporarily the maximum speed of construction. In line with this argument, Coulson and Richard (1996) documents that unusual weather conditions lead to long-lasting temporal delays of housing starts and completions in the U.S. An alternative explanation for the difference between the two graphs is statistical uncertainty, given that the standard errors are relatively large.

According to Figure 3, the weather shocks should lead to a big enough shift of housing completions in December so that rents in the first months of the subsequent year need to react. Consider a person who is waiting for her new unit to be completed. Presumably, she is in close contact with the developer, so that she will adjust her plans if there is a delay. If chances are high that the building is going to be completed in December, she will most likely take action already in October or November: If she owns her previous unit, she will put it on the market for sale. Otherwise, she will inform her landlord that she will be moving out in December, so that the landlord can offer the unit for rent. In both cases, the previously occupied unit will be on the market by December. This suggests that the effect of the newly completed unit on

Figure 3: When are the Delayed Units Completed?



Note: The figures display the β_m coefficients of regression (1) and 95% confidence intervals. December completions of the preceding year, $B^{(12)}_{t-1,i}$, are instrumented by the rainfall shock instrument. The regressions depicted in Panel A (B) include year-FE (year- and municipality-FE). Standard errors are clustered by municipality.

local rents is immediate.

3. The Effect of New Housing Supply on Average Local Rents

Before turning to the impact of new housing supply on the local rent distribution as a whole, I consider the effect on average local rents, in linear instrumental variable regressions. First, I present a simple model of a local housing market that yields estimating equations and offers an interpretation of the estimated coefficients. Then, I estimate the impact of new supply on average rents and use these estimates to determine how many additional new housing units would have kept real rents constant in different locations. In a final step, I empirically identify key parameters of the model by running a set of complementary regressions. This yields estimates of housing supply elasticities in Germany during the last boom, 2011–2018.

3.1. A Simple Model of a Local Housing Market

Assume a representative region, and an outside option that offers utility u. The region is characterized by a wage level w and amenities. The economy is populated by a mass M of households who derive utility of Cobb-Douglas form from goods and housing consumption. The user cost of a housing unit is r, while the price of goods consumption is normalized to 1.

Households differ in their preference for the region's amenities, which is summarized by a parameter $a \sim \mathcal{U}_{(0,1/\eta)}$, where $\eta > 0$. Indirect utility is given by $a^{-\zeta} w r^{-\alpha}$, where $\zeta \geq 0$. Small values of ζ mute utility differentials between low-a and high-a households, while large values amplify them. That is, higher values of ζ lead to stronger taste dispersion.

There is a marginal resident with preference $\bar{a}$ whose utility equals u, that is

$$\bar{a} = \left(\frac{w}{u r^{\alpha}}\right)^{1/\zeta}. \tag{2}$$

Because a is drawn from a uniform distribution and every household with $a \leq \bar{a}$ decides to live in the region, $M\eta\bar{a}$ represents the number of households in the region. This suggests the following relationship between rents and (exogenous shifts in) the number of housing units, Q_H:

$$\ln r = \frac{\zeta}{\alpha}\ln(M\eta) + \frac{1}{\alpha}(\ln w - \ln u) - \frac{\zeta}{\alpha}\ln Q_H. \tag{3}$$

That is, a regression of log rents on log housing supply identifies the parameter $\zeta\alpha^{-1}$.

At the intensive margin, each household demands $h = \alpha w r^{-1}$ units of housing. Total housing demand is

$$H = M\eta\alpha w^{\frac{1+\zeta}{\zeta}} r^{-\frac{\alpha+\zeta}{\zeta}} u^{-\frac{1}{\zeta}}. \tag{4}$$

I assume that developer-landlords supply land at increasing marginal costs. The cost function is $c(S) = \psi S^{1+\phi}$. Marginal costs increase linearly in $\psi > 0$ and nonlinearly in $\phi > 0$. One interpretation for these two parameters is that they represent constraints to housing supply. Abstracting from financing conditions, developers provide housing at user cost r per unit of housing, to maximize profits. This implies that $(1+\phi)\psi S^{\phi} = r$, and hence $\ln S = \phi^{-1} \ln r + \ln \psi - \phi^{-1} \ln(1 + \phi)$. The housing market clears to determine r,

$$\ln r = A + \frac{\phi \frac{1+\zeta}{\zeta}}{1 + \phi \frac{\alpha+\zeta}{\zeta}} \ln w, \tag{5}$$

where A combines several terms in order to improve readability, and it does not depend on w.

Although the model is simple, it entails relationships and parameters that are key to analyses of regional and place-based policy, taxation, local housing markets, and migration. It suggests the following procedure to identify ϕ, ζ, and α, and thus the housing demand and supply elasticities. First, (3) and the supply shock can be used to identify the housing demand elasticity, $\zeta^{-1}\alpha$. Second, α can be identified from household-level data on housing consumption and income via the household's demand for housing services. Third, (5) suggests that ϕ can be identified from a regression of housing costs, $\ln r$, on local wages, $\ln w$, given the estimates of ζ and α.

3.2. Estimation Results

This section investigates the impact of new housing supply on average local rents empirically, drawing on three data sources. Housing completions are provided by the administrative Building Completions Statistic.[8] It contains data on the number of new housing units completed by municipality and month, 2010–2017. Unfortunately,

[8]Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, *Statistik der Baufertigstellungen*, survey years 2010-2017, own calculations.

it is not possible to separate the supply of social housing from the supply of private-market housing in the empirical analysis. However, in recent years, only a small share of new housing supply in Germany was subsidized social housing.[9] In all other cases, developers are free to sell their units at any price. Hence, the results should reflect the impact of private-market housing on rents. A more extensive description of the data can be found in Appendix A.

The rent data were collected from three large online real estate market places (Immonet, Immowelt, Immobilienscout24) on a monthly basis between July 2011 and December 2018. The data contain information on the net rent, the unit size in square meters, the postcode of the unit, the month of its first appearance, and a list of housing characteristics. The sample is described further in Appendix B. The outcome of interest is the log rent per square meter, net of utilities and heating costs. Finally, the instrument is derived from rainfall data provided by the German Weather Service as grid cell data (1×1 km^2) for the years 2010–2017.[10]

As a first step, I run district-level panel IV regressions, where a district-level yearly rent index is regressed on the housing completions in the preceding December (instrumented by the rainfall shocks). I then turn to individual-level IV regressions that allow to consider in greater detail the dynamics of the impacts.

The panel regression equation reads

$$\ln \text{Index}_{d,t} = \gamma \frac{S^{(12)}_{d,t-1}}{H_d} + \psi_d + \phi_t + \varepsilon_{d,t}, \tag{6}$$

[9]Since 2007, the German Länder (federal states) are responsible for social housing, and a unified statistic does not exists. According to a parliamentary interpellation from March 2017, about 6% of new housing supply was subsidized in 2013 and 2014 (Deutscher Bundestag, 18/11403). Unfortunately, the Building Completions Statistic also does not provide information on subsidies.

[10]Source: DWD Climate Data Center (2010-2017). REGNIE grids of daily precipitation.

where $\text{Index}_{d,t}$ is a hedonic rent index of district d in year t, $S^{(12)}_{d,t-1}$ is the number of units completed in district d in December of year $t-1$, H_d is the number of units in the housing stock in 2011, and ψ_d and ϕ_t denote district- and year-fixed effects. The estimation of the hedonic index is described in greater detail in Appendix B. The results are summarized in Table 2. In column (1), the main coefficient, γ, is highly significant and negative. To make sense of the effect size, consider a municipality with the median number of housing completions in December (0.09% of the stock) and assume that housing supply expands to bring the municipality up to the third quartile (0.18%). The estimate suggests that this reduces mean rents in the subsequent year by about $0.09\% \times 29.8 \approx 2.6\%$. When moving from the median down to the first quartile (0.04%), rents increase by $0.05\% \times 29.8 \approx 1.5\%$.[11] The coefficient is similar when taking the sum of the completions in December and November relative to the stock (column 2) or the log of (the number of completions in December plus the number of units in the stock in 2011) (column 3). The Kleibergen-Paap F statistic does not indicate weak instruments problems.

The aggregation necessary for building the hedonic index on the district level makes it difficult to study the temporal dynamics of the effects. I thus turn to individual-level regressions, where the units of observation are rental housing units i offered for rent in municipality g_i, year t_i, and month m_i. For a given calendar month $m \in \{1, ..., 12\}$ and lag $k \in \{0, 1, 2\}$, the estimating equation is of the form

$$\ln R_i = \gamma^k_m \frac{S^{(12)}_{t_i-k,g_i}}{H_{g_i}} + \beta X_i + \psi_{g_i} + \phi_{t_i} + \varepsilon_i \qquad \forall i \in \{j : m_j = m\}. \tag{7}$$

[11] The distribution of December housing completions as % of the stock is highly skewed, with a very long right tail. I therefore report the effect associated with moving up the quartiles of the distribution, instead of relying on the standard deviation.

Table 2: Impact of New Housing Supply on District-Level Average Rents

Dependent variable:	Log Rent Index (Mean Rent)		
	(1) IV	(2) IV	(3) IV
Units completed in Dec of year $t-1$ (share of the stock 2011)	-29.8** (10.5)		
Units completed in Nov + Dec of year $t-1$ (share of the stock 2011)		-27.6** (9.5)	
Log(# of units completed in Dec of year $t-1$ + # of units in the stock 2011)			-29.9** (10.5)
Year FE	yes	yes	yes
District FE	yes	yes	yes
Kleibergen-Paap F	16.1	17.3	16.1
Observations	3,136	3,136	3,136

Note: Standard errors are clustered by district; *: $p < .05$, **: $p < .01$, ***: $p < .001$. The instrument is the rainfall shock in the summer of year $t-1$.

The sample consists of rental units i observed in month $m_i = m$. Each unit is located in a municipality g_i and is observed in a year t_i. A more extensive description of the rents data is relegated to Appendix B.[12] R_i is the net rent per square meter, and X_i are characteristics of unit i (log living area, year of construction, year of construction squared, presence of floor heating, parquet flooring, and elevator, a fitted kitchen, a second bathroom, a garden, a balcony or terrace, the type of dwelling unit, and self-reported housing unit quality). $S^{(12)}_{t_i-k,g_i}$ is the number of completions in December of year $t_i - k$, and H_{g_i} is either the number of housing units in municipality g_i, or the average number of units offered for rent in a given month. As an alternative for the log-linear form, I also estimate the regression in log-log form, by replacing $S^{(12)}_{t_i-k,g_i}/H_{g_i}$ with $\ln\left(H_{g_i} + S^{(12)}_{t_i-k,g_i}\right)$. This is closer to the theoretical model described in Section 3.1. Finally, ψ_{g_i} and ϕ_{t_i} are municipality- and year-fixed effects.

Equation (7) is estimated separately for each calendar month in the year of the

[12]In Hamburg and Berlin, the instrumentation strategy does not work because the first-stage relationship very weak there. Combined, Hamburg and Berlin make up about 10% of the total sample size in the rents data, so that the weather shocks in those two cities have great influence on the first-stage regression, leading to weak instruments problems. I therefore drop observations from Berlin and Hamburg from the individual-level instrumental variable regressions.

weather shock ($k = 0$), in the year after the weather shock ($k = 1$), and in the year after that ($k = 2$). Clearly, γ_m^0 should be zero when $k = 0$ and $m = 1, ..., 11$, i.e. before the buildings were completed. This is akin to a test of common pre-trends in an event study design, where the treatment is in December, and the treatment effect is plotted for the months January to November of that year, as well as for the months following the treatment. Standard errors are clustered by municipality.

Figure 4 summarizes the results.[13] It displays the γ_m^k coefficients and their 95% confidence intervalls over time. The red vertical line indicates the December in which the new housing units enter the market. The December completions are instrumented by the rainfall spell in the preceding summer (July to September). In Panel A, new housing supply is normalized by the number of units in the housing stock in 2011. As an alternative, I consider the number of new housing units relative to the average number of rental units offered on the market in a given month in Panel B.[14] Arguably, this regression makes it easier to understand the magnitude of the effects: The coefficient expresses the change in the log rental price when new housing supply increases by one unit for every 100 units offered on the local market in an average

[13] Results for covariates and model summary statistics are in Appendix C (Figure C1 and Table C1)

[14] The number of rental units offered on the market is derived from the rents data by calculating the average number of observations per month, by municipality. As noted above, the rents data were collected via web scraping (24/7) from the three largest online real estate market places in Germany, Immobilienscout24, Immonet, and Immowelt. Duplicates are removed, based on a comparison of key variables. The combined overall market share is approximately 80–85%; all other market places are considerably smaller, see the report "Freigabe des Zusammenschlusses von Online-Immobilienplattformen", Bundeskartellamt B6-39/15 [Federal Cartel Office]. Immonet and Immowelt merged in 2015. In February 2018, Immobilienverband Deutschland conducted a survey "Usage of Real Estate Online Market Places" [*Nutzung von Immobilienportalen*] among 1,287 real estate agents, 99.3% of the respondents use third-party real estate market places for marketing purposes. 76% use Immonet/Immoscout, and 74.4% use Immobilienscout24 (multiple answers possible). Respondents also indicated that 84% of all rental units were offered on at least two different real estate market places. Overall, this suggests that the three websites cover most of the market, so that the measure of flow is fairly accurate.

month. The two graphs differ only marginally in their overall pattern.

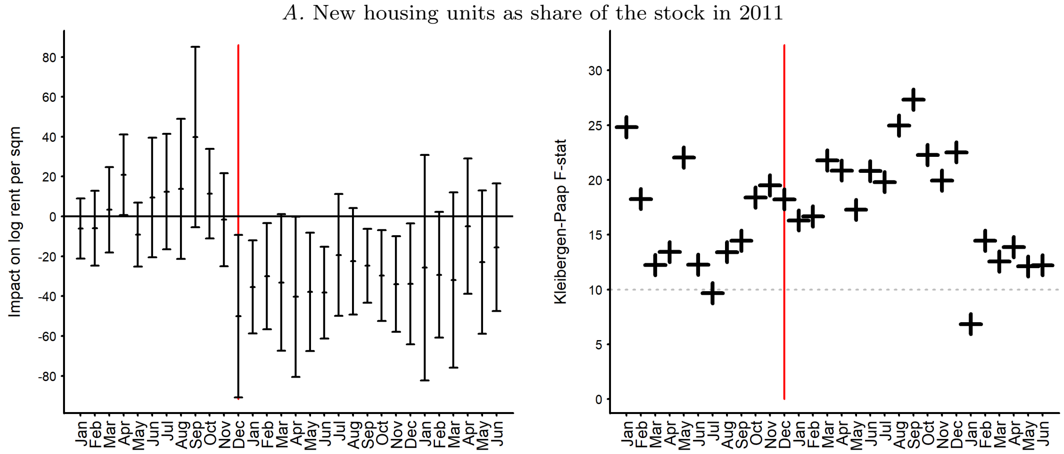
Clearly, there is no effect on rents before the new housing units enter the market. The graphs also reveal that the effect on rents is significantly negative already in the month when the new units enter the market. Consistent with the temporary nature of the weather shock and with the results from Figure 3, the effect becomes insignificant and smaller about one year after the new units were completed. The fact that it dissipates only gradually is consistent with earlier findings for the U.S., where weather shocks had very persistent effects on the number of housing completions (Coulson and Richard, 1996).

Since the regression is of log-linear form, and the endogenous variable is measured relative to the total stock/ flow of rental units, the coefficient indicates the relative change in rents due to a relative change in overall housing supply. In Panel B, the coefficient ranges between -0.7 and -0.4, suggesting that rents decrease by around 0.4% to 0.7% when one new housing unit is completed for every 100 housing units offered for rent per month.

Housing markets might be considerably larger than municipalities. I therefore test the robustness of the results to measuring housing completions at the district level instead, in Figure 5. Panel A displays results for the baseline specification (Panel A of Figure 4). The results are virtually unchanged. Secondly, the model described in Section 3.1 suggests a log-log specification for the relationship between rents and housing supply. Panel B therefore replaces the endogenous variable, the number of new housing units relative to the stock, by $\ln(\text{stock in 2011} + \text{new housing units})$. This also does not influence much the results. Recall that the two parameters that govern heterogeneity of location preferences, ζ, and household-level housing demand, α, are fixed by the relation $\zeta = b \times \alpha$, where b is the estimated coefficient. The regression for the December when the new housing units enter the market suggests

Figure 4: Average Effect of New Housing Supply at Municipality Level on Rents per sqm (linear IV)

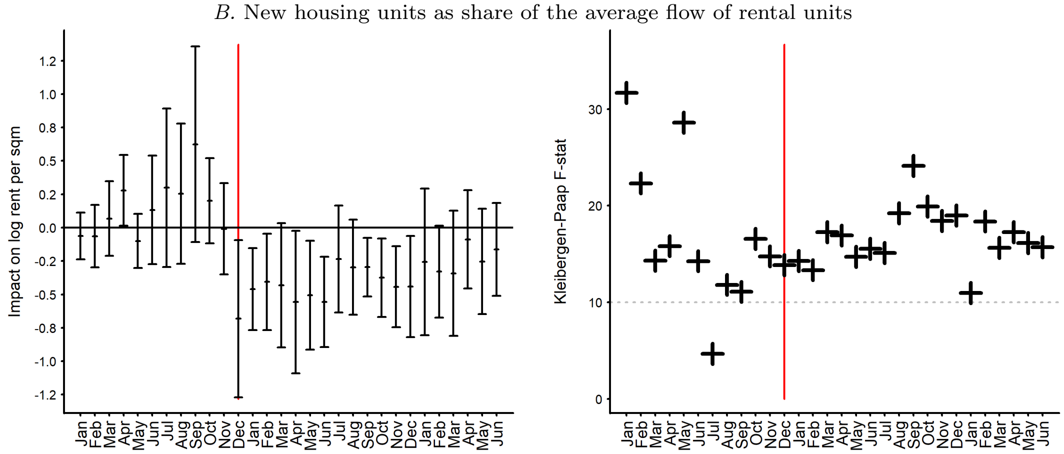
A. New housing units as share of the stock in 2011



Second stage coefficients

Kleibergen-Paap F's

B. New housing units as share of the average flow of rental units



Second stage coefficients

Kleibergen-Paap F's

Notes: The figures display regression coefficients together with their 95% confidence intervals. "Second stage coefficients" refer to the coefficient of the housing supply variable in the second-stage IV regression. The right-hand side displays the corresponding Kleibergen-Paap F statistic. Each month refers to a separate regression of log rents in a given month on housing completions in December, see the description of equation (7) in the main text. Housing completions are measured at the municipal level.

$b = 58.4$.

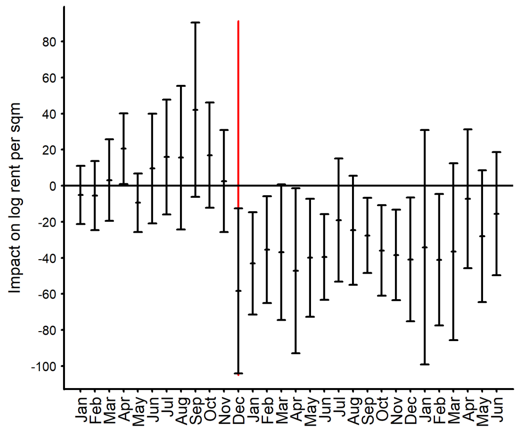
In Appendix Figures C2, and C3, I investigate the robustness of the results pictured in Figure 4, Panel A. So far, the regressions did not control for micro-locations within large municipalities. One potential issue arising from this are sample composition effects. Although it is unlikely that sample composition is correlated with the instrument, I run an additional regression that controls for location fixed effects at the postcode level instead of the municipality level. The corresponding graph in Panel A of Figure C2 shows that the results are clearly robust to this change in controls.

Secondly, in Panel B of Figure C2, I add to the edogenous variable the weighted sum of housing completions in nearby municipalities. As weights, I use the inverse distance (in km) between the municipality centroids, while setting weights below 0.02 to zero. Thus, weights are positive for distances between zero and 50 km, which can be considered a feasible commuting distance. I then replace the endogenous variable by the share of housing completions in the observation's own municipality, plus the weighted sum of the housing completions (relative to the stock) nearby. The Kleibergen-Paap F statistic increases considerably, presumably, because the endogneous variable and its instrument are both spatially autocorrelated. More importantly, the coefficients are again remarkably stable.

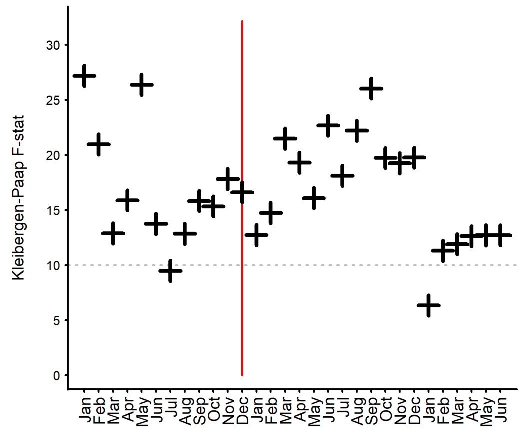
A threat to identification could be that severe rainfall shocks cause floods. Large floods may have more severe effects on the local economy, so that the regressions would capture the effects of new housing supply *and* changes in housing demand. Moreover, the instrument would also reduce total supply of housing to the extent that floodings destroy existing buildings. These changes could also be permanent. In 2013 and 2016, there were two larger floods in Germany that affected the basins

Figure 5: Average Effect of New Housing Supply at District Level on Rents per sqm (linear IV)

A. New housing units as share of the stock in 2011

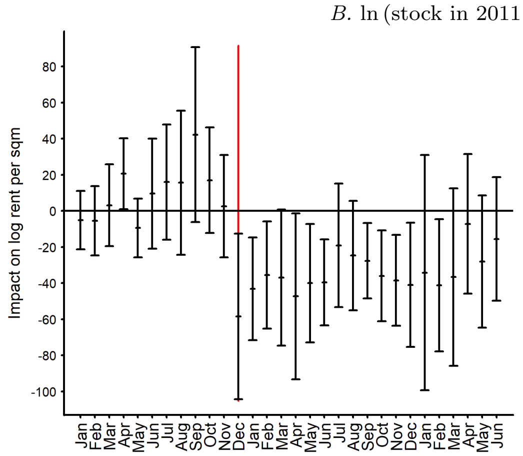


Second stage coefficients

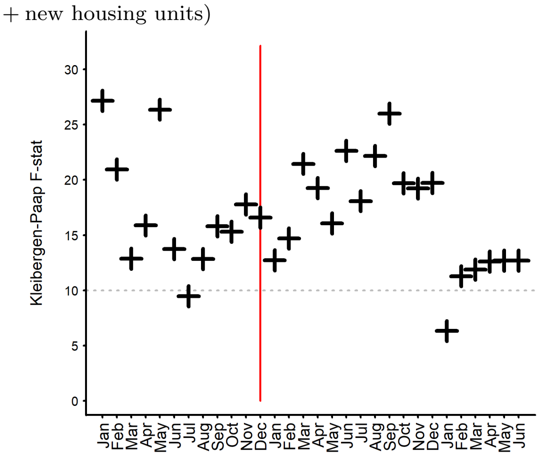


Kleibergen-Paap F's

B. $\ln(\text{stock in 2011} + \text{new housing units})$



Second stage coefficients



Kleibergen-Paap F's

Notes: The figures display regression coefficients together with their 95% confidence intervals. "Second stage coefficients" refer to the coefficient of the housing supply variable in the second-stage IV regression. The right-hand side displays the corresponding Kleibergen-Paap F statistic. Each month refers to a separate regression of log rents in a given month on housing completions in December, see the description of equation (7) in the main text. Housing completions are measured at the district level.

of several large rivers[15]. Panel A of Figure C3 displays results for regressions that exclude the respective years. Although this leads to weak instruments problems in some regressions (see the first stage coefficients in Panel B), the regression results are qualitatively and quantitatively very robust. In these regressions, the effects are negative only in the year after the rainfall shock, and insignificant and small thereafter.

Finally, the results could be driven by extreme observations. In some smaller municipalities, new housing units amounting to more than five or even ten percent of the stock are completed in a single December. I therefore re-run the baseline regression, excluding observations where new housing supply in December is larger than 5% of the stock (Panel B of C3). Again, this does not have significant impact on the results.

The regressions presented so far strongly suggest that new housing supply by private markets shifts average local rents. Next, I conduct a series of counterfactual exercises to illustrate the quantitative impact of new housing supply.

3.3. How Much New Construction is Lacking to Keep Real Rents Constant?

Most larger cities in Germany experienced substantial real earnings and rent increases over the sample period. A highly policy-relevant question in this context is: How much additional supply would have reduced the observed real rent increases to zero?

To investigate this, I estimate the actual nominal rent rent increases for the largest cities in Germany and a few additional smaller university cities in simple hedonic regressions, based on the rents data. I deflate rents by the consumer price inflation

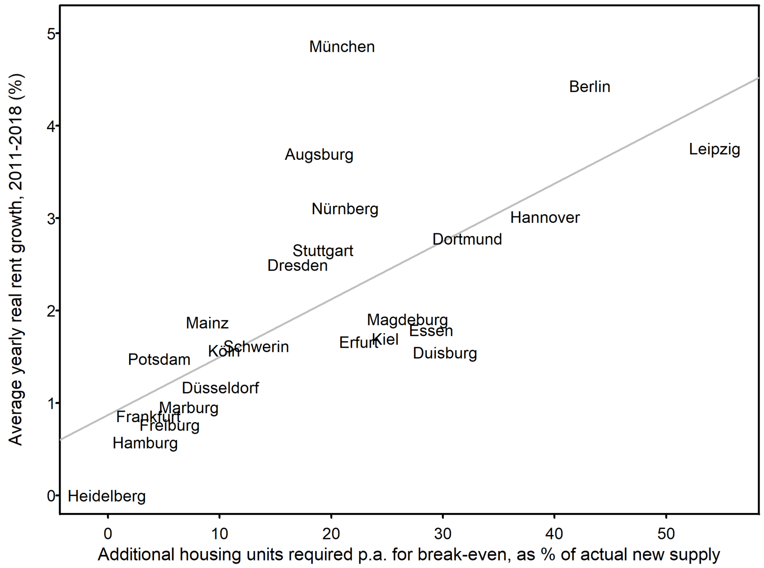
[15]In 2013, the Danube, Elbe, and Saale Rivers were affected, while in 2016, there were severe floodings in the Danube, Rhine, and Neckar River basins.

and take the observed new housing supply from the building completions data. Then, I divide the estimated real rent increases by 29.8, the estimated coefficient from Table 2, column (1). This gives the share of units that would have to be added to reduce real yearly rent growth to zero. Figure 6 plots this number relative to the actual supply per year against the observed real rent growth. Munich and Berlin have seen the strongest real rent growth rates of approximately 4.9 and 4.5% per year. The estimates suggest that, if housing supply in Munich had expanded by about 21% more than the actual expansion of housing supply observed over the period 2010–2017, rents would have remained stable in real terms. In Berlin, the lack of supply amounts to over 40% of the actual supply, despite slightly smaller overall rent increases. On the other hand, cities such as Essen and Duisburg experienced much smaller real rent increases. Housing supply in these cities did not expanded very much either, so that the required relative expansion is large, 30%. These numbers are remarkably close to the projection of housing units required per year of the Bundesverband deutscher Wohnungs- und Immobilienunternehmen [*Federal Association of German Housing and Real Estate Companies*], claiming that the actual new supply of housing units in Germany as a whole in 2017 was 29% short of the required number of new housing units.[16]

The positive correlation visible in the graph could relate to the fact that it is more difficult for housing supply to meet additional demand if demand is growing quickly. Alternatively, it could reflect long-run housing supply elasticities, with relatively elastic places in the lower left, and inelastic places in the upper right part of the graph.

[16]Source: Annual Press Conference of the Federal Association of German Housing and Real Estate Companies, June 27 2018, "Daten und Trends der Wohnungs und Immobilienwirtschaft 2017/2018", https://web.gdw.de/uploads/pdf/Pressemeldungen/JPK_2018_Praesentation_final.pdf.

Figure 6: Additional Housing Supply per Year Required for Reducing Real Rent Growth to Zero

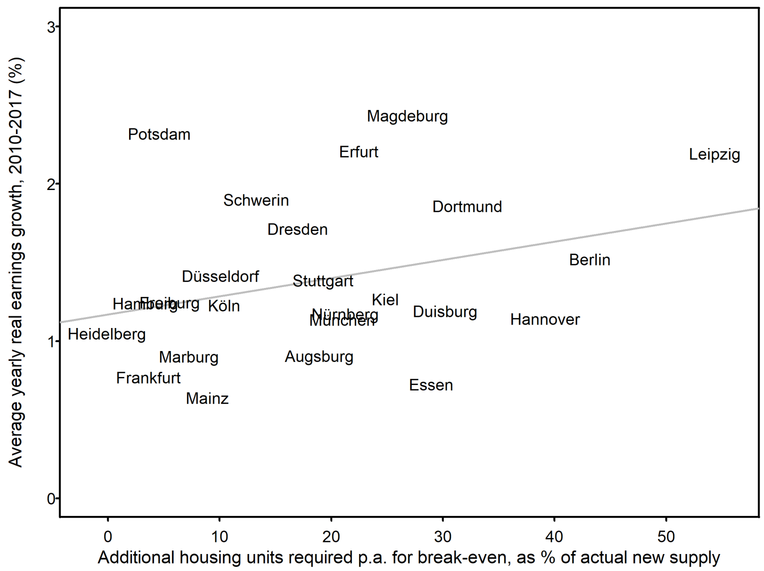


A strong correlation between the lack of housing supply and the strength of contemporaneous housing demand shocks indicates that short-run supply constraints such as planning and construction lags are relatively important. On the other hand, long-run supply constraints should lead to greater relative lack of supply also if the demand shocks are weak.[17] Figure 7 plots the lack of housing supply against the growth rate of real earnings per worker over the period 2010 to 2017, which represents a shock to local housing demand. Real earnings are based on the Regional Accounts of Germany.[18] The correlation between the lack of supply and the strength of the earnings shock is clearly positive, but it is rather weak, suggests that both short- and long-run supply constraints matter. Moreover, there could be other demand shocks that overlay the bivariate relationship depicted here.

[17]This is complicated by a potential correlation between long-run housing supply constraints and housing demand shocks

[18]*Bruttolöhne und Gehälter, Volkswirtschaftliche Gesamtrechnung der Länder.*

Figure 7: Real Earnings Growth and Additional Housing Supply per Year Required



3.4. *Deriving Estimates of the Housing Supply Elasticity*

Finally, this section investigates in a more structural way the relationship between demand shocks, housing supply elasticities, and real rent growth, by building on the simple model described in Section 3.1. In order to identify the three key parameters of the model, I estimate the housing expenditure share, α, from household-level data taken from the German Socio-Economic Panel, and equation (5) from a regression of housing rents on local income.[19]

In order to estimate the housing expenditure share α, I regress expenditures for net rent of renter households in the GSOEP on household income net of taxes and social security contributions, excluding the intercept. Appendix D contains details of the estimation and results, suggesting $\alpha = 0.21$, with 0.18 as a lower bound. As the last step towards determining ϕ, the average local housing supply elasticity, I regress log housing rents of individual housing units on average local earnings at the

[19]The GSOEP version is v34; it covers waves 1984–2017.

district-year level (see also Hilber and Vermeulen, 2016, for a similar strategy), year-, calendar month-, and postcode-fixed effects, and a set of housing characteristics. The regressions are described in Appendix D. The estimated coefficients suggest a contemporaneous rent-earnings elasticity of 0.25. If log rents are regressed on lagged log earnings instead, the resulting elasticity is smaller, 0.17. Moreover, in a weighted regression that assigns more weight to districts with smaller rental markets, the rents-earnings elasticity shrinks to 0.07.

Together with equations (3) and (5), these results allow to identify the parameter ϕ that governs the elasticity of housing supply. The model assumes that the cost of supplying a total square footage S of housing to a local market is given by $\psi S^{1+\phi}$. This cost includes the cost of structure and the purchasing cost of the underlying land. Hence, for $\phi > 0$, an expansion of the local housing stock by 1% increases the average cost per square meter by approximately $100\% \times \phi$. Moreover, profit maximization of the developers imply a housing supply elasticity of ϕ^{-1}. From (5), ϕ is given by

$$\phi = e_r \left(\frac{1+\zeta}{\zeta} - e_r \frac{\alpha + \zeta}{\zeta} \right)^{-1}, \tag{8}$$

where e_r is the rent-earnings-elasticity. Table 3 provides estimates of the housing supply elasticity, using different parameterizations that are based on the estimates from Figure 5, Panel B, and Tables 2, D2 and D3.

Three things are noteworthy. First, the estimate of the elasticity is much more sensitive to the estimated housing rents–earnings elasticity than to the choice of the housing expenditure share or the estimated local housing demand elasticity (which determine α and ζ). Second, based on the unweighted regression and the contemporaneous earnings measure, the average supply elasticity is around 3.4. The estimated supply elasticity is much larger, at around 15.5, when its calculation is based on the

Table 3: Structural Estimates of the Average Housing Supply Elasticity in Germany

Parametrization	Expenditure Share (α)	Taste Dispersion (ζ)	rent-earnings-Elasticity (e_r)	Housing Supply Elasticity (ϕ^{-1})
1	0.21	6.26	0.25	3.65
2	0.21	12.26	0.25	3.35
3	0.18	5.36	0.25	3.76
4	0.18	10.51	0.25	3.41
5	0.21	6.26	0.07	16.30
6	0.21	12.26	0.07	15.14
7	0.18	5.36	0.07	16.69
8	0.18	10.51	0.07	15.35
9	0.21	6.26	0.17	5.93
10	0.21	12.26	0.17	5.48
11	0.18	5.36	0.17	6.09
12	0.18	10.51	0.17	5.56

Note: Each row displays one parametrization based on the estimation results (Figure 5, Panel B, and Tables D2 and D3). ζ is calculated based on equation (3), and the housing supply elasticity, $1/\phi$, is calculated based on equation (8).

weighted rents-earnings regression that puts considerably more weight on districts with smaller rental housing markets. Third, supply is much more elastic in the longer run: When using the lagged earnings measure in the estimation of the rents-earnings elasticity, the housing supply elasticity increases from about 3.5 to about 5.8. Interestingly, these baseline esimates are in the range of estimates for U.S. metropolitan areas due to Green et al. (2005). They are a bit larger than the supply elasticities for the largest U.S. metropolitan areas provided by Saiz (2010), which are based on physical constraints to development and range from 0.6 for Miami to about 4 for Indianapolis. One potential reason could be that this baseline estimate is representative of Germany as a whole, including the periphery, where supply elasticities are likely much higher than in the urban cores.

4. Effects on the Tails of the Local Rent Distribution

The preceding section provided evidence that new housing supply shifts immediately the (conditional) mean of the local rent distribution. However, it is still an open question to what extent the tails of the local rent distribution are affected. In particular, the lower tail determines the housing costs of lower-income households. It is thus a key question for housing policy whether this part of the rent distribution also shifts in response to new housing supply.

4.1. Filtering

Although some poorer households might not have the willingness (and ability) to pay for a new unit, filtering may provide these households with adequate housing. The main idea behind filtering is that houses, as they depreciate, provide less and less housing services, so that the associated equilibrium rents and prices fall. Households can thus sort into newer units of higher quality and older units of lower quality, based on their income. This suggests that the impact of new supply by private markets on rents should not be confined to the upper part of the rent distribution. Filtering theories go back at least to Muth (1973) and Sweeney (1974a,b). They suggest that private-market housing supply could provide an important source of low-income housing supply, both theoretically (Arnott and Braid, 1997; Braid, 1984, 1986; Ohls, 1975) and empirically (Rosenthal, 2014; Skaburskis, 2006; Weicher and Thibodeau, 1988; Margolis, 1982).

In a nutshell, the literature has described this process as a chain of sequential moves (Weicher and Thibodeau, 1988): As new units enter the market, some high-income households leave vacant their unit, which can then be occupied by other households of lower income. Usually, this process is thought to continue to the bottom of the income (and housing quality) distribution.[20] Understood this way, filtering suggests that the rents of high-quality housing units, which are close substitutes to newly constructed housing, should react first, while it might take longer until rents decline at the bottom of the quality distribution.

[20] There can be incentives of landlords to upgrade their units as they reach a certain quality threshold (Arnott and Braid, 1997)

On the other hand, the standard filtering model disregards moving costs. Moving costs are an important aspect in the filtering story, but they have not gotten much attention in theoretical models of, or empirical work on filtering thus far. With positive moving costs, households have to weigh the utility benefits from living in newly built housing against the utility losses from moving house. This breaks up the strict ordering of incomes and housing qualities. If there are substantial moving costs, households stay at a location for an extended period before they re-optimize their income–housing quality–housing costs bundle. I provide a brief formal discussion of this point in Appendix E.

The immediate consequence for the empirical analysis is this: A household will not move from an almost new unit into a newly built one if moving costs are substantial. To the contrary, a household who decides to occupy a new housing unit can be expected to come from a lower-quality dwelling. This turns the prediction upside down, so that rents at the bottom of the rent distribution would react first to new housing supply. It is thus an empirical question which of these two forces dominates.

Generally, the moving costs mechanism could make filtering even more important as a means of providing housing for low-income households. If moving costs are high enough, private-market (i.e., non-subsidized) new housing supply should lead to an immediate fall in rents at the bottom of the rent distribution, irrespective of the quality of these new units.[21]

[21] Potentially, there are some special sub-markets that are not well connected to the wider housing market, such as large villas, mansions, castles, and the like. New housing supply in such sub-markets is still unlikely to have effects on rents in lower-quality segments. Arguably, these markets are less important quantitatively.

4.2. Evidence from Instrumental Variable Quantile Regressions

In this section, I run a set of instrumental variable quantile regressions (Chernozhukov and Hansen, 2005, 2006, 2008) that extend the linear IV results. Instrumental variable quantile regression (IVQR) allows to study the effect of a treatment (new housing supply) on the distribution of an outcome variable (the rent distribution). To simplify notation, let D be the endogenous variable of interest, and let X include the municipality and year fixed effects. For a combination of a month m and a lag order k (and an appropriately restricted sample),

$$\ln R = \beta(U)X + \gamma_m^k(U)D. \tag{9}$$

Here, $U \sim \mathbb{U}([0,1])$, whereby U may statistically depend on D, but it is independent of X and the instrument Z. Moreover, it is required that the right-hand side of equation (9) increases strictly in U almost everywhere (Chernozhukov and Hansen, 2006).

I follow the estimation strategy and inference procedure due to Chernozhukov and Hansen (2008). The strategy builds on the fact that

$$\mathbb{P}\left(\ln R \leq \beta(\tau)X + \gamma_m^k(\tau)D|Z,X\right) = \tau. \tag{10}$$

Chernozhukov and Hansen (2008) show that, for a fixed $\hat{\gamma}_m^k$, 0 is a τ-th conditional quantile of $\ln R - \beta(\tau)X - \hat{\gamma}_m^k(\tau)D$ given X and Z. Hence, they propose to choose $\gamma_m^k(\tau)$ such that the coefficient of Z is as close to zero as possible in an ordinary quantile regression of $\ln R - \hat{\gamma}_m^k(\tau)D$ on X and Z. Denoting the coefficient of the instrument by $\delta(\tau)$, inference about $\gamma_m^k(\tau)$ can be based on inference about $\delta(\tau)$. This procedure yields inference that is robust to weak instruments problems.

I combine this strategy with a block bootstrap procedure to obtain a 95% confidence region that is robust to dependencies of the residuals within municipalities. Specifically, I first run a line search to find the point where $\delta(\tau)$ is as close to zero as possible. This yields a point estimate for $\gamma_m^k(\tau)$. I then run two separate bisectioning algorithms to find the end points of the 95% confidence region for $\gamma_m^k(\tau)$. For each potential candidate boundary $\gamma_m^k(\tau)'$ of the confidence region, I use a block bootstrap to construct a 95% confidence region for the corresponding $\delta(\tau)'$. If the corresponding confidence region for $\delta(\tau)'$ touches zero, the candidate $\gamma_m^k(\tau)'$ is a bound of the confidence region of $\gamma_m^k(\tau)$. This procedure works for both bounds of the confidence region, and the computational upside is that it lends itself to the efficient bisectioning algorithm. Hence, according to the arguments put forward in Chernozhukov and Hansen (2008), this procedure yields a valid confidence region for $\gamma_m^k(\tau)$, without having to invert the covariance matrix. The latter operation is not feasible in the current setup, with controls for several thousand municipalities, so that inference based directly on the Wald statistic—as suggested by Chernozhukov and Hansen (2008)—is not possible.

Figure 8 displays the results. Because of the computational burden involved in the estimation procedure, I only ran a subset of 14 of the 30 regressions that correspond to the linear instrumental variable regressions from Figure 4, and I restrict attention to the quantiles 0.2, 0.3, ..., 0.8. Reassuringly, the IV and IVQR regressions lead to very similar results in terms of magnitudes and standard errors. Moreover, the IVQR results do not provide a clear indication that new housing supply shifted differentially the rent distribution. To the contrary, they suggest that new housing supply shifts the rent distribution as a whole. When considering the point estimates, it seems that the lower parts of the rent distribution reacted more strongly in the first months after the new units came on the market, while the upper part reacted more strongly several months later. Overall, none of the two main forces — substitutability of housing units, and moving costs —, seems to dominate. The key implication is that new housing supply *provided by private developers* effectively lowers rents throughout the rent distribution, shortly after the new units are completed. This finding is of first-order importance for housing policy in general.

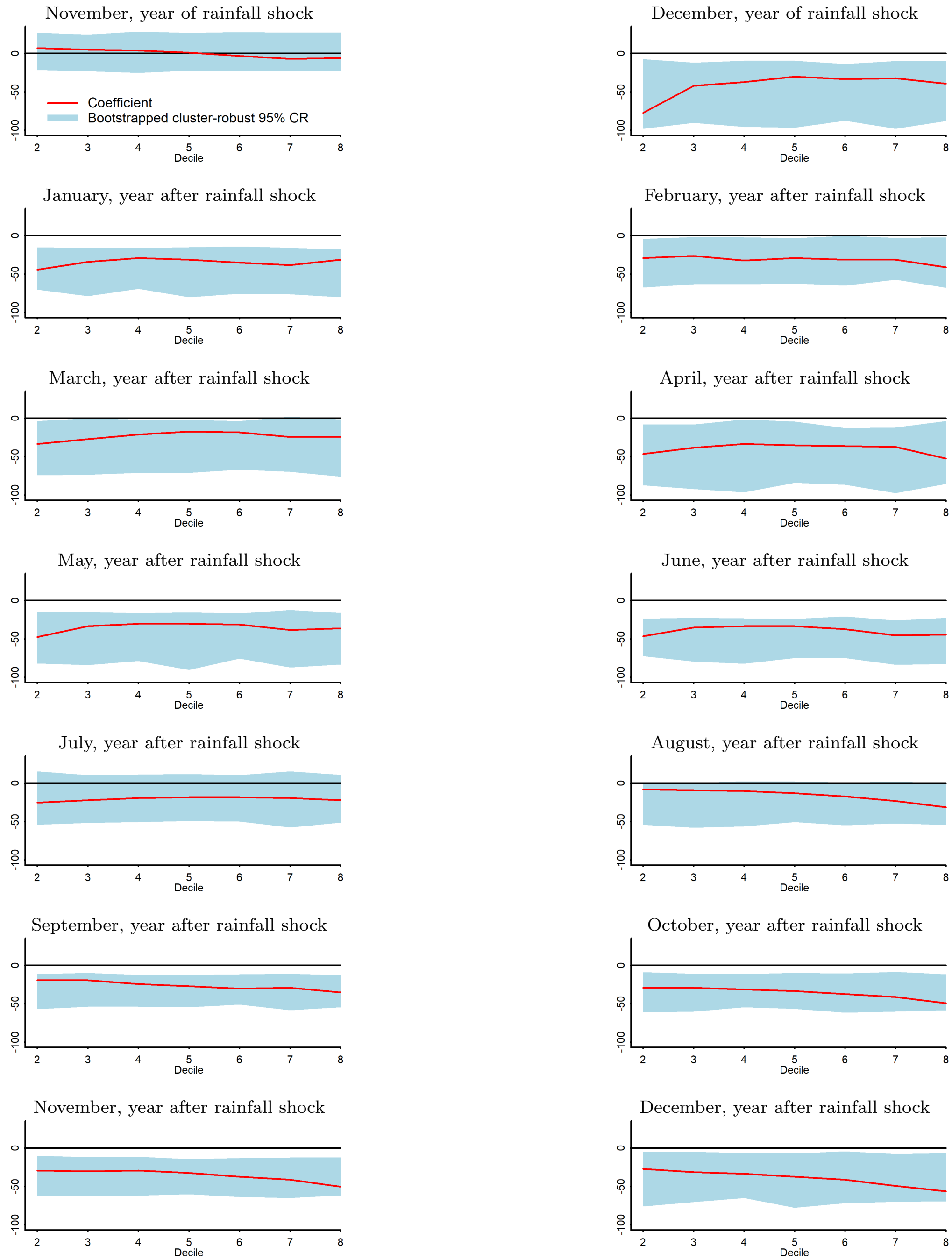
As a robustness check, I consider district-level panel regressions, with a hedonic index for the τ-th quantile of the local rent distribution as the dependent variable. These regressions are described in Appendix F. Their results corroborate the main finding from the IVQR analysis, namely that new housing supply by private markets induces a location shift of the local rent distribution. Moreover, the effect sizes are very similar in the two approaches.

4.3. Jumping Up the Housing Ladder

The empirical results presented in Section 4 as well as the theoretical considerations from Appendix E suggest that new housing supply—provided by the private market—induces a location shift of the local rent distribution. One implication is that, in a flow-sense, new housing supply triggers an expansion of supply at all housing quality levels. By leaving vacant their previous housing unit, mover households shape the way in which housing supply at different quality levels expands.

In standard filtering models, moving decisions depend on the quality and price (or rent) differentials between housing units, and on household income. Moreover, moving costs increase the quality differential that is necessary for a (utility-increasing) move. If moving costs are high enough, quality differentials can be large, so that households that move into the newly supplied housing units may come from housing units of relatively low quality. This section empirically investigates these links, by drawing on household-level data from the German Socio-Economic Panel, 1984–2017.

Figure 8: IVQR Results: The Effect of December Completions on the Distribution of Rents per sqm



Notes: Each graph displays the treatment effects $\gamma_k^m(\tau)$ for the quantiles $\tau = 0.2, 0.3, \ldots, 0.8$ in a particular month. The shaded area represents a 95% confidence band that was obtained from the bootstrap procedure described in the text. The confidence bands are robust to dependencies within municipalities.

I first establish that the average length of stay at a given address is relatively high among GSOEP households, suggesting that the overall quality depreciation during an individual stay is substantial. That is, the housing unit's quality will have decreased considerably from the time the household moved in, to the next move. In a second step, I study determinants of the decision whether to choose a new or an existing housing unit.

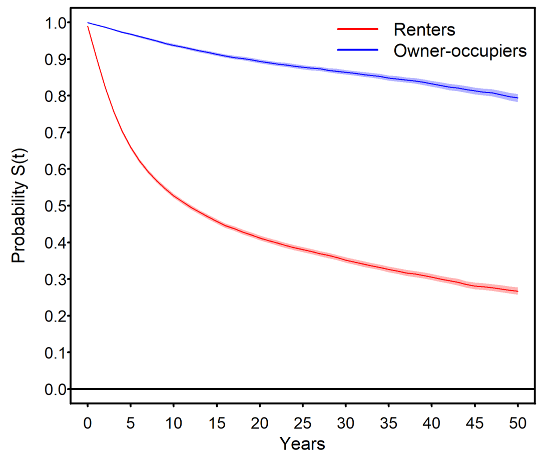
4.3.1. Mover Households

The GSOEP reports the year in which a household moved into the current dwelling unit. Figure 9 displays Kaplan-Meier curves for the moving decision of owner-occupiers and renters in the GSOEP. Clearly, renters are much more mobile than owners. This is driven mostly by the fact that owner-occupiers are very immobile. 96.8% (93.8%) of all owner-occupiers stay for at least five (ten) years in their housing unit before they move. The respective numbers are also remarkably high for renters, where 66.0% (52.7%) of all renters stay at a location for at least five (ten) years, on average. Both curves fade only very slowly, suggesting that a substantial share of households never move house.[22]

Table 4 reports summary statistics for mover households in the GSOEP. Panel A summarizes the number of movers by tenure. Per year, 2.0–4.0% of renters move, but only a few owner-occupiers do so (0.2–0.6%). Moreover, the share of owner-occupiers is comparably low (34.0–49.2%). Together, these numbers imply that an analysis of moving decisions in the GSOEP is effectively an analysis of renters' moving decisions.

[22] I acknowledge that higher attrition rates among mover households may bias the curves towards immobility. However, the substantial differences between renters and owner-occupiers, as well as the relatively low mobility rates are found also in studies based on address histories, e.g. the Postadress Umzugsstudie [*Postaddress mover study*], conducted yearly by Deutsche Post, the successor of the Federal German Post Office. According to the Postaddress Umzugsstudie 2018, about 10% of the population move per year in Germany, of which 83% were renters.

Figure 9: Kaplan-Meier Curves for Moving Decisions of Owner-Occupier and Renter Households



Notes: Each line represents nonparametric estimates of the probability that a household does not move in the first x years after she moved into her housing unit. The shaded areas denote 95% confidence intervals.

Panel B provides descriptive statistics of two key variables, real household income, and building age. Both variables are measured in the last year the household lived at the previous address. Mean monthly real net household income at the time of the move is 2,247 Euro.[23] Following Rosenthal (2014), a proxy for housing unit quality is building age, which I measure as the survey year minus the year of construction. Although building age is an imperfect measure of overall housing quality, it is a useful approximation in the present case. First, the standard filtering model treats interchangeably building age and housing quality. Second, new housing units—by definition—have a building age of zero years. The year of construction is reported in the GSOEP, but the degree of accuracy varies. Earlier waves of the survey report only a classified year of construction. More recent waves allowed respondents to report an individual year or a range of years (since 2000). When the year of construction is given as a range of values, I use the average of the upper and lower bounds of that range. At the time of the move, the average building age is 37.8 years. The third row

[23]Nominal income is deflated to 2017-Euros via the consumer price index

Table 4: Summary Statistics for Mover households in the GSOEP

Panel A. Aggregates by year					
	Min	Mean	Median	Max	Std. Dev.
Moves of renters	55	154	129	342	74
..as share of all renters	0.020	0.029	0.029	0.040	0.006
Moves of owner-occupiers	3	15	14	42	9
..as share of all owner-occupiers	0.002	0.004	0.003	0.006	0.001
Share of owner-occupiers	0.340	0.409	0.387	0.492	0.056

Panel B. Mover households					
	Min	Mean	Median	Max	Std. Dev.
Monthly real net hh income at time of move	264	2,247	1,965	58,929	1,655
Building age at time of move	0.0	37.8	37.0	82.5	20.9
Δ building age when moving	-81.5	-4.2	0.0	71.5	26.0

Panel C. Mover households who move into a newly constructed housing unit					
	Min	Mean	Median	Max	Std. Dev.
Monthly real net hh income at time of move	394	3,706	3,461	16,416	2,071
Building age at time of move	2.0	34.0	31.0	80.5	21.2

reports the change in building age between the current and the previous address. On average, households move into buildings that are 4.2 years younger than the building in which they lived before.

Panel C shows that this difference is much larger among households who move into newly constructed housing units. Prior to moving, these households lived in buildings that were 34 years old on average, with a standard deviation of 21.2. Moreover, real household income is substantially higher in this group.

4.3.2. Who Moves into New Housing Units?

Although Table 4 suggests that households move in order to "rejuvenate" their housing unit, the question remains whether this is a quantitatively important aspect of the moving decision. As a first step towards an answer to this question, I consider a panel regression with the building age as the dependent variable. This regression is related to the papers by Rosenthal (2014) and Mast (2019). In Rosenthal (2014), the empirical strategy is to follow individual buildings over time, while resident income may change between tenures. Here, I follow the reverse strategy by tracking households who move from building to building, which is more similar to Mast (2019). This means that household income as well as other household-level characteristics remain (approximately) constant, but building age may change. The estimating equation is

$$\begin{aligned}\text{Building Age}_{it} = \phi_t + &\beta_0 \text{Building Age}_{i,t-1} + \beta_1 \ln(\text{Income}_{i,t-1}) \\ &+ \beta_2 \text{Owner}_{i,t-1} + \varepsilon_{it},\end{aligned} \tag{11}$$

where $\text{Income}_{i,t-1}$ denotes the real household income of individual i at time $t-1$, $\text{Owner}_{i,t-1}$ is a dummy that is equal to one if household i was owner-occupier in year $t-1$, ϕ_t are year fixed effects, and the sample is restricted to (i,t)-pairs for which household i moved in year t. In theory, income (and wealth) are important determinants of the housing choice. Tenure is a proxy for household wealth. Moreover, it is unlikely that owner-occupiers are credit-constrained, which should allow them to realize moving plans more easily.

Column (1) of Table 5 displays the results. If the building age at the previous location was higher, the next dwelling is older as well. However, the coefficient is substantially (and statistically significantly) smaller than 1, suggesting that households tend to move from older to younger buildings also conditional on household income and tenure status. If household income is higher, or if the household was an owner, the housing unit is younger. This complements the results from Rosenthal (2014), where the household who moves into a particular existing unit tends to have a lower income than the previous occupier of that unit. Column (2) considers the change in building age as the dependent variable. Again, households with higher incomes and owner-occupiers tend to decrease more strongly the building age of their residential unit when they move. Both coefficients are smaller in magnitude, suggesting that tenure status and household income are also negatively correlated with the building age at the previous location.[24]

Table 5: Choice of Housing Unit

Dependent variable:	Building age	Δbuilding age	Indicator: Housing unit is new			
	(1) OLS	(2) OLS	(3) Logit	(4) Logit	(5) Logit	(6) Logit
Building age at previous location	0.226*** (0.019)		-0.013*** (0.004)	-0.007 (0.004)	-0.010* (0.004)	-0.035 (0.022)
Log real household income at time of move	-6.304*** (0.657)	-3.335*** (0.804)		1.433*** (0.159)	1.482*** (0.172)	0.906 (0.606)
Owner-occupier at previous location	-6.677*** (1.330)	-4.453** (1.677)		0.618** (0.211)	0.631** (0.216)	1.142* (0.536)
Year FE	Yes	Yes	No	No	Yes	Yes
Adjusted R^2	0.162	0.012	-	-	-	-
Pseudo R^2	-	-	0.012	0.131	0.197	0.258
Mean of dep. var.	33.4	-4.3	0.080	0.080	0.080	0.256
Observations	3,510	3,510	2,405	2,405	2,405	121

Note: The sample consists of mover households only. The samples for columns (3) to (5) are restricted to waves where respondents had the option to report the precise year of construction (2000-2013, 2015-2017). In column (6), the sample is further restricted to respondents who reported a precise year of construction. The dependent variables refer to the building age at the new location (after having moved). Standard errors are clustered by household; *: $p < .05$, **: $p < .01$, ***: $p < .001$.

The results presented so far are consistent with the basic filtering model. As the second step, in columns (3) to (6), the dependent variable is replaced by a dummy that equals 1 if the destination housing unit is new, and a logit regressions is estimated. One potential problem is that the outcome variable is measured with error because year of construction is available in classified form only for part of the panel. I therefore restrict the analysis to survey years 2000–2013 and 2015–2017, where households had the chance to report a specific year of construction *or* a range of years (in case the respondent was not sure). Arguably, households who live in newly constructed units know the exact year of construction, so that the indicator should be valid.

The regression reported in column (3) includes the building age at the previous location as the only regressor. While the coefficient is significantly negative, as expected, the building age at the previous location on its own is a quite poor predictor.

[24]This result also follows from a regression of the building age at the previous location on income and tenure status at the time of the move (not reported).

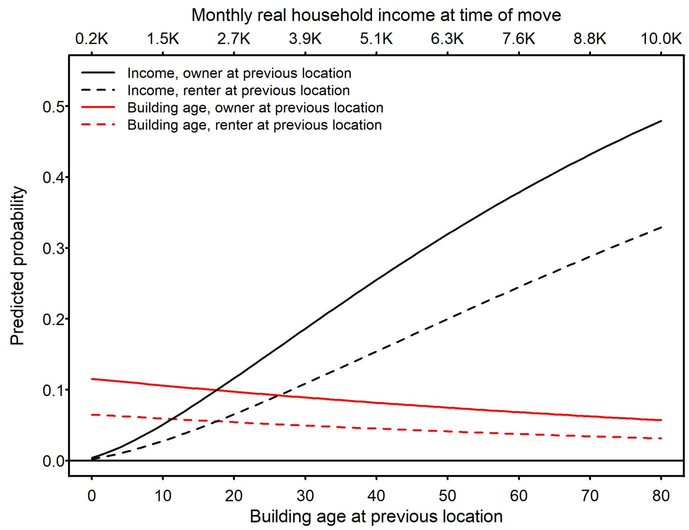
Column (4) adds the log real household income at the time of the move, and the owner-occupier dummy as regressors. Both variables are positively related to the likelihood of moving into a newly constructed housing unit (conditional on moving). While the coefficient of the previous location's building age is fairly stable, it is only marginally significant in this regression.[25] The regression in column (5) additionally controls for year fixed effects, but this hardly affects the coefficients. Once again, these regression results lend support to the basic assumptions behind filtering models. However, they also show that households "jump up" the quality ladder, taking several steps at once.

To be able to gauge the relative quantitative importance of the three explanatory variables, Figure 10 displays the marginal impacts of the three variables on the predicted probability of moving into a new housing unit, based on column (5) of Table 5. Clearly, household income and tenure status have far greater impact on the predicted probability. Panel C of Table 4 has shown already that the variation in previous building ages is large among households who move into new units. As a whole, this suggests that households who move into new units come from housing units that differ substantially by building age. This fits nicely with the results presented in Section 4, that the rent distribution as a whole shifts in response to a shock to new housing supply.

As a robustness check, column (6) of Table 5 restricts the sample to observations where the exact year of construction is reported. Although this reduces substantially the sample size, from 2,405 to a mere 121 observations, the coefficients are remarkably stable.

[25]Higher real household income and being an owner-occupier are negatively correlated with building age, but the correlation is rather weak (-0.12 and -0.06, respectively; both measured in the year prior to moving house).

Figure 10: Predicted Probabilities of Choosing a New Unit (based on Table 5, column (5))



Notes: Each line represents the predicted probability of choosing a new unit, for different values of the previous building age, the real household income at the time of the move, and tenure status. The omitted variable is set to the sample mean.

5. Conclusions

This paper's results provide a simple, yet difficult to implement prescription for housing policy: Housing costs of the population as a whole can be reduced effectively by letting developers provide enough market-rate housing. Consequently, denser development has great potential to reduce the housing cost burden of low-income households—in addition to other possible benefits such as shorter commuting distances and larger productivity spillovers. While local planning and building codes are very important tools for preventing negative externalities associated with the built environment, policy makers should rethink carefully the degree to which these tools are applied.

One reason why it is difficult for policy makers to foster housing supply is severe local opposition against denser development. The underlying causes are structural (Hilber and Robert-Nicoud, 2013). Based on the results from this paper, it is not necessary to permit highly concentrated social housing in order to reduce housing costs of low-income households. Rather, planning policies could opt for gradual densification without constraining the building quality. This should increase the acceptance of construction projects on the local level. Future work should investigate in greater detail whether such a trade-off between the speed of expansion of housing supply on the one side, and local acceptance of construction projects on the other, could be an alternative to existing housing policies. In general, it seems highly valuable to design and evaluate policies that balance the interests of local residents and stakeholders against the interests of (renter) households with high housing cost burdens. Otherwise, there is a great risk that policy makers opt for cheap, but ineffective measures of housing policy that have great potential to hurt, rather than help, socially vulnerable households.

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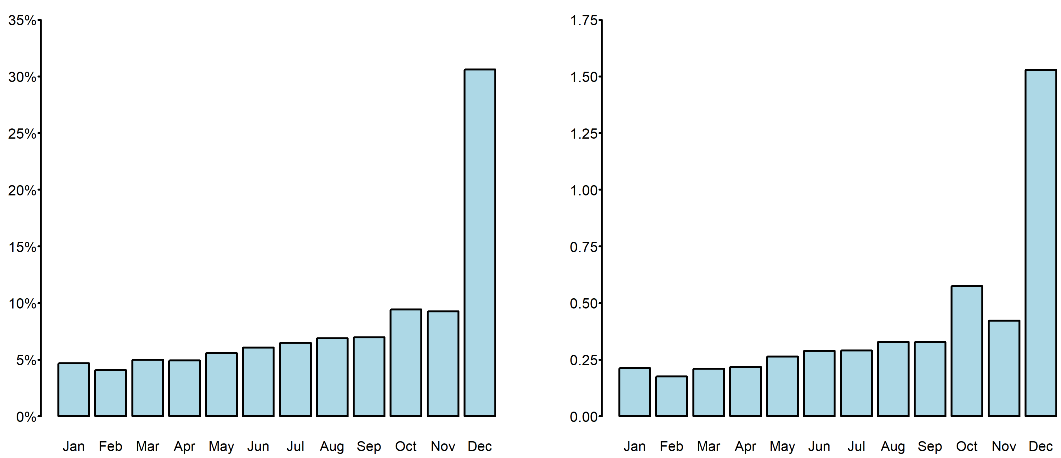
Appendix

A. Building Completions Data

The main explanatory variable in the rents regressions is the number of housing units completed in a municipality in December. This variable is aggregated from individual observations in the Building Completions Statistic. The Building Completions Statistic is an administrative statistic that contains all building completions in Germany. There are severe penalties for developers who do not acquire permission to build from the local authorities. Fines range from 500 to 50,000 Euro, and the authorities can oblige the owner to demolish the building at the owner's expense. Information on the month of completion is not provided in individual years by some federal states. I exclude the respective state-years from the analysis.

Figure A1 shows the variation in building completions by calendar month. Most buildings are reported to be completed in December (Panel A: shares; Panel B: completions by month per 1000 units in the stock (2011)).

Figure A1: New Housing Units Completed in Germany 2010–2017, by Calendar Month



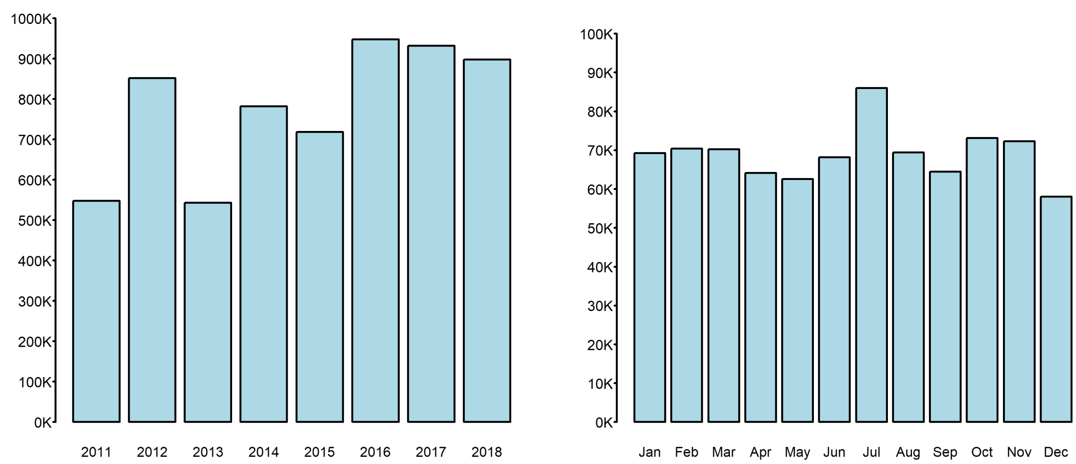
A. Shares

B. New units per 1000 units in the stock (2011)

B. Rental Housing Data and District-Level Rent Indices

Data. The rents data were collected between July 2011 and December 2018 via web scraping from three large online real estate market places, Immoscout24, Immonet, and Immowelt. Immonet and Immowelt merged in 2015, but continue to coexist as websites. The three websites have a combined market share of 80–90%, according to Immoscout24 and the Federal Cartel Office of Germany. It seems unlikely that transacted rents differ systematically with the weather shocks. Moreover, alternative data sources of transacted rents that have a comparable spatio-temporal coverage do not exist. Figure B2 displays the number of observations by year (Panel A) and by calendar month (Panel B).

Figure B2: Number of Observations in the Rental Housing Data, by Year and Calendar Month



A. Number of observations, by year — *B.* Average number of observations, by month

Summary Statistics. Table B1 contains summary statistics for the variables used in the rents regressions. The average monthly rent per square meter is 7.8 Euro (median: 6.8 Euro). The monthly rent refers to the rent posted on the day the offer appears online for the first time. The rents data also include an identifier of the postcode, of the month of first appearance, and of the exact address (with missings), and list of unit characteristics that are used as controls.

Table B1: Summary Statistics for the Rents Sample

Panel A. Non-categorial and binary variables

	Min	Mean	Median	Max
Monthly rent per sqm	1.6	7.8	6.8	85.2
Living area in sqm	15.0	71.4	67.0	300.0
Year of construction	1800	1970	1974	2018
Floor heating	0.000	0.084	0.000	1.000
Parquet flooring	0.000	0.032	0.000	1.000
Elevator	0.000	0.165	0.000	1.000
Fitted kitchen	0.000	0.316	0.000	1.000
Second bathroom	0.000	0.148	0.000	1.000
Garden	0.0000	0.1855	0.0000	1.0000
Balcony or terrace	0.000	0.602	1.000	1.000
New units in December, relative to				
the stock (2011)	0.000	0.001	0.001	1.324
the avg. # of rental units on the market	0.000	0.142	0.053	14.240

Panel B. Categorial variables (shares)

	0	1	2	3	4	5	6	7	8
Dwelling type	0.586	0.112	0.131	0.009	0.033	0.002	0.006	0.010	0.110
Quality	0.017	0.147	0.831	0.005					

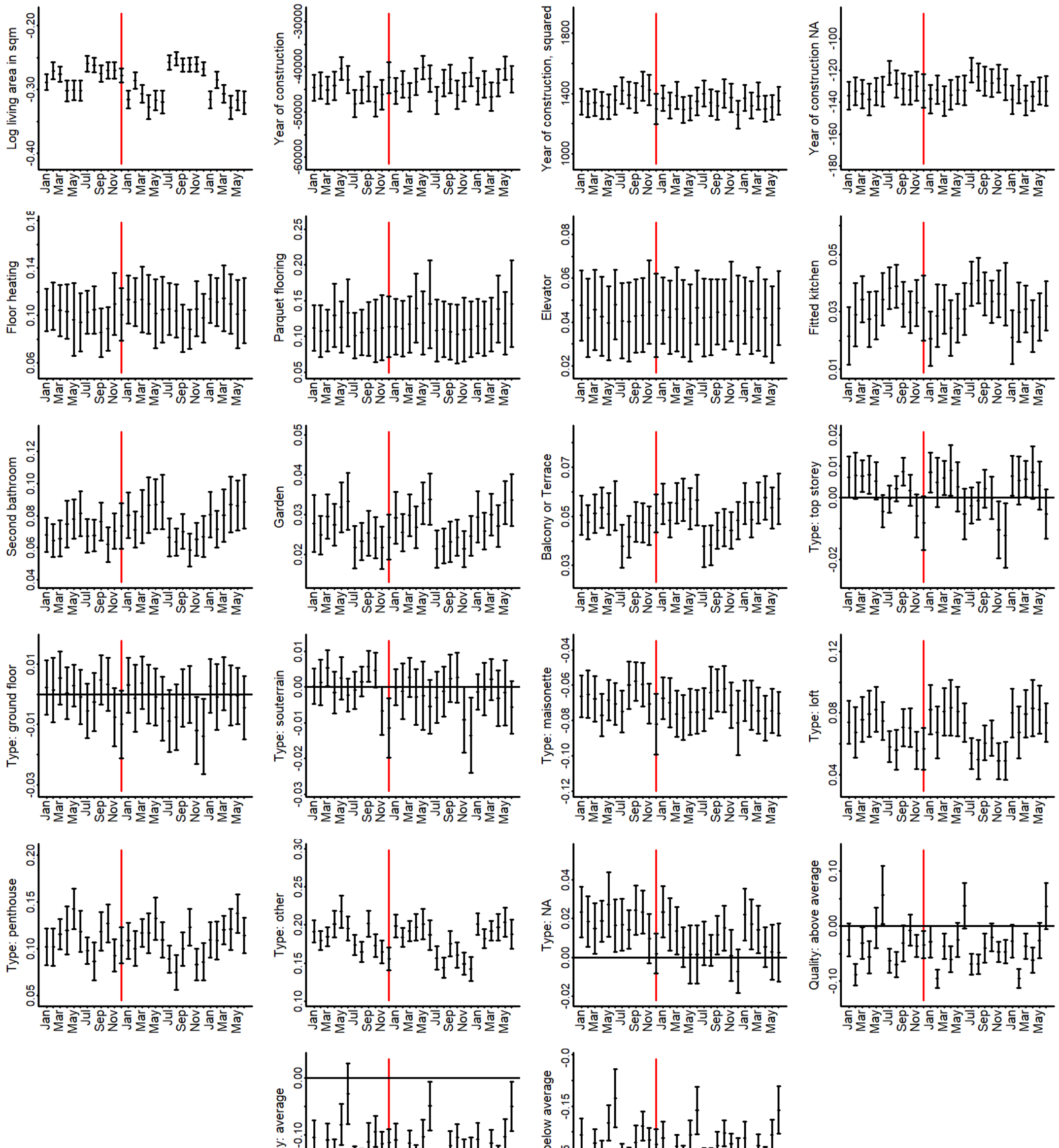
Note: Dwelling type categories are 0: regular, 1: roof storey, 2: ground floor, 3: souterrain, 4: maisonette, 5: loft, 6: penthouse, 7: other, 8: NA. Quality categories are 0: luxurious, 1: above average, 2: average, 3: below average.

District-Level Rent Indices. In order to calculate the district-level rent indices, I run separate hedonic regressions for each district, with the log rent per square meter as the dependent variable, and a set of housing characteristics and year fixed effects as controls. The resulting index value for year t is given by $\exp(\mathrm{FE}_t)$, the exponential of year t's fixed effect.

The index reflecting the average rent is based on an OLS regression, controlling for log floor area, a second-order polynomial in the year of construction, an indicator variable for observations where the year of construction was not reported, dummies for the presence of floor heating, parquet flooring, an elevator, a fitted kitchen, a second bathroom, a balcony or a terrace, a garden, and categorial quality and condition indicators. The index reflecting the τ-quantiles of the local rent distribution were obtained from quantile regressions, so that the quantile indices represent constant-quality τ-quantiles. There, some control variables (the second-order term of the year of construction, the quality and condition indicators, and the dummies for parquet flooring, floor heating, and garden) led to numerical convergence problems in some districts. For reasons of consistency, I dropped these covariates from the calculation of the quantile indices.

C. Results of the Linear Instrumental Variable Regressions

Figure C1: Model Summary for Baseline Linear IV Regression (Figure 4, Panel A)



Note: The figures display regression coefficients and 95% confidence bands for all covariates used to estimate the baseline linear IV model (see equation (7) and Figure 4, Panel A in the main text). Each graph corresponds to one control variable and displays the coefficients of 30 separate regressions. Table C1 contains summary statistics of the models. Standard errors are clustered by municipality.

Table C1: Model Summary for Baseline Linear IV Regression (see Figure 4A)

A. Year of the rainfall shock

	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	444,253	420,113	431,055	388,771	364,842	397,296	602,980	463,136	423,613	500,426	488,666	396,746
adjusted R^2	0.665	0.677	0.660	0.654	0.657	0.643	0.684	0.676	0.640	0.664	0.675	0.646
F-stat excluded instrument	24.8	18.3	12.2	13.4	22.1	12.3	9.7	13.4	14.5	18.4	19.5	18.2

B. Year after the rainfall shock

	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	484,782	492,607	491,385	448,895	437,668	477,204	688,201	555,328	515,915	584,814	578,246	464,211
adjusted R^2	0.642	0.667	0.643	0.642	0.647	0.634	0.694	0.686	0.674	0.670	0.673	0.682
F-stat excluded instrument	16.3	16.7	21.8	20.9	17.3	20.8	19.8	25.0	27.3	22.3	19.9	22.5

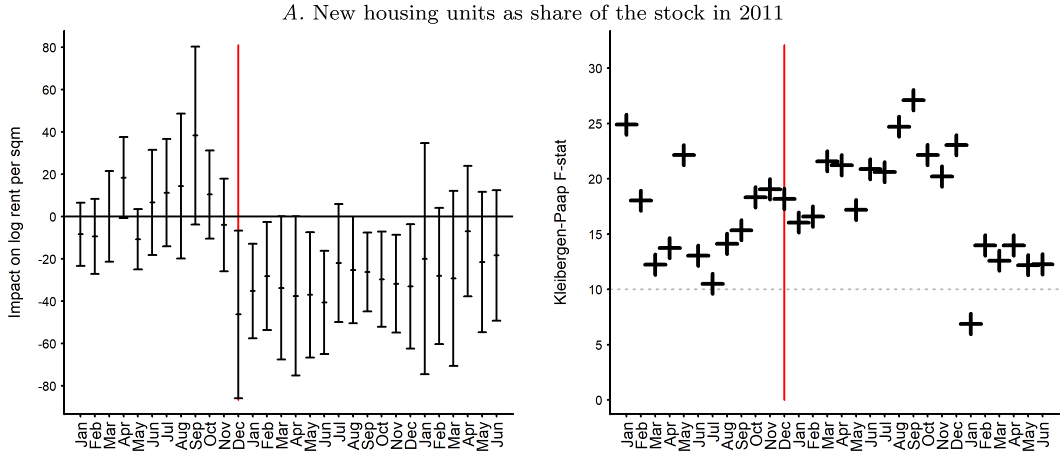
C. Two years after the rainfall shock

	Jan	Feb	Mar	Apr	May	Jun
Year FE	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes
Observations	484,782	492,607	491,385	448,895	437,668	477,204
adjusted R^2	0.653	0.670	0.646	0.662	0.660	0.650
F-stat excluded instrument	6.8	14.5	12.6	13.9	12.1	12.2

Note: The table display summary statistics of the models reported in Figure 4.

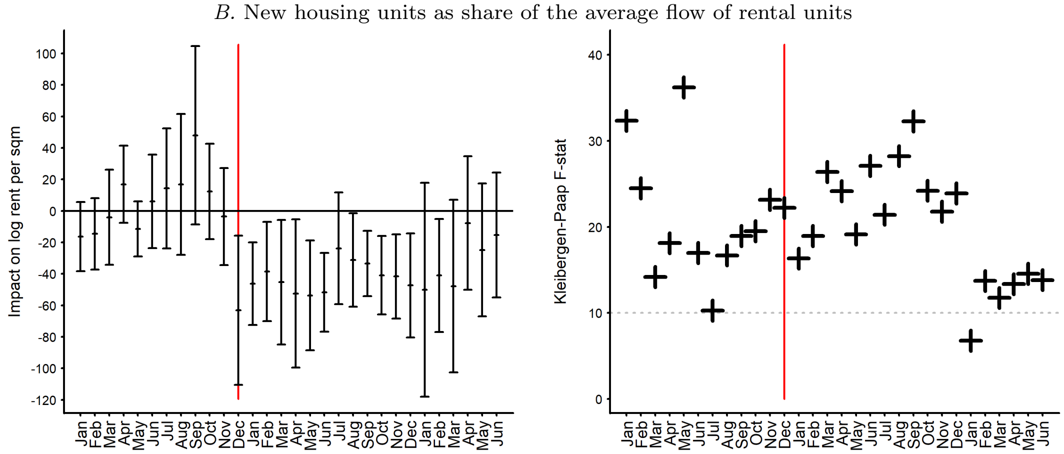
Figure C2: Additional Linear IV Regressions: Postcode Fixed Effects and Spatially Weighted Housing Completions

A. New housing units as share of the stock in 2011



Second stage coefficients Kleibergen-Paap F's

B. New housing units as share of the average flow of rental units

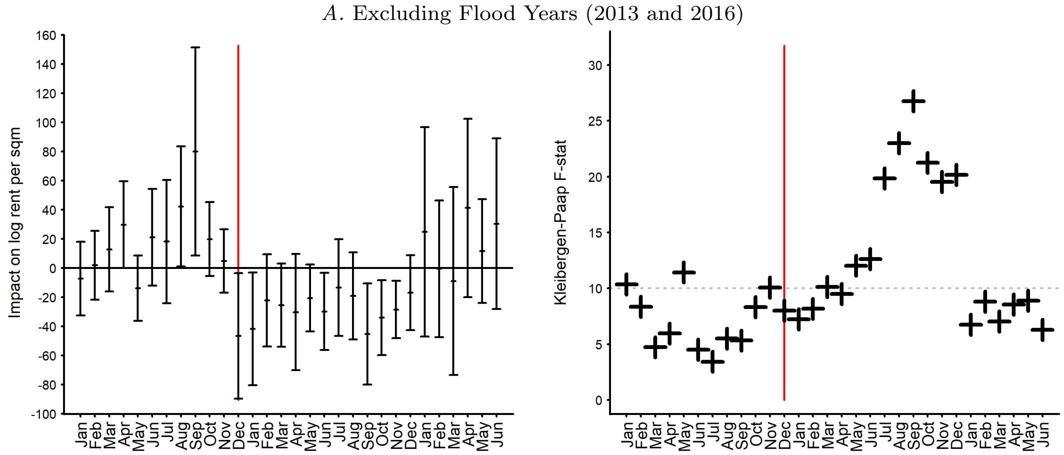


Second stage coefficients Kleibergen-Paap F's

Notes: The figures display regression coefficients together with their 95% confidence intervals. "Second stage coefficients" refer to the coefficient of the housing supply variable in the second-stage IV regression. The right-hand side displays the corresponding Kleibergen-Paap F statistic. Each month refers to a separate regression of log rents in a given month on housing completions in December, see the description of equation (7) in the main text. Housing completions are measured at the municipal level (Panel A) and as the inverse-distance-weighted sum of housing completions in nearby municipalities (Panel B).

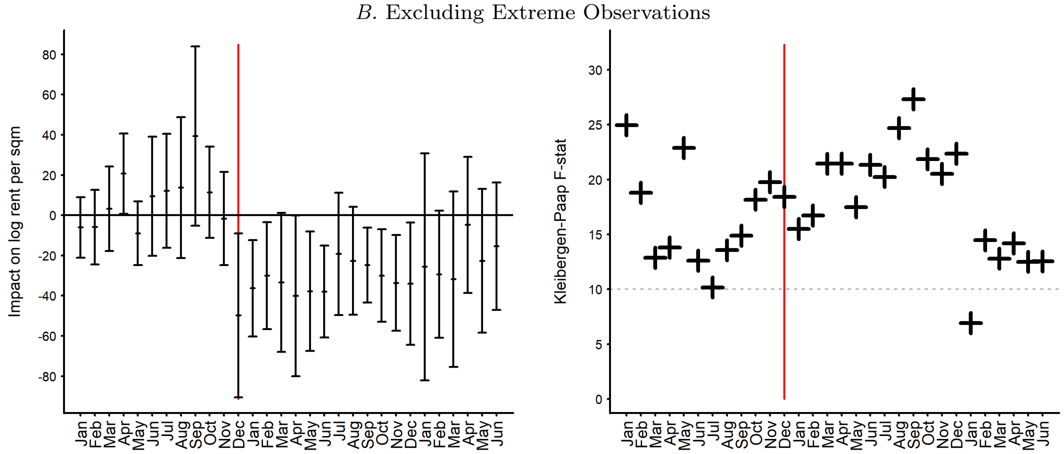
Figure C3: Additional Linear IV Regressions: Excluding Flood Years and Outliers

A. Excluding Flood Years (2013 and 2016)



Second stage coefficients | Kleibergen-Paap F's

B. Excluding Extreme Observations



Second stage coefficients | Kleibergen-Paap F's

Notes: The figures display regression coefficients together with their 95% confidence intervals. "Second stage coefficients" refer to the coefficient of the housing supply variable in the second-stage IV regression. The right-hand side displays the corresponding Kleibergen-Paap F statistic. Each month refers to a separate regression of log rents in a given month on housing completions in December, see the description of equation (7) in the main text. Housing completions are measured at the municipal level. In Panel A, years with severe floods were excluded (2013 and 2016). In Panel B, observations were dropped when the number of housing units completed in the respective December amounted to more than 5% of the stock.

D. Details on the Estimation of Key Parameters of the Model

This section describes the estimation of the housing expenditure share, α, and of the rents-earnings elasticity. Using the structure of the model, these two ingredients, together with the estimated housing demand elasticity, allow to determine the housing supply elasticity.

Housing expenditure share. The housing expediture share is estimated based on the GSOEP. I restrict the sample to the period 2010–2017, and to years in which households moved into a new dwelling. The latter is supposed to account for the fact that landlords cannot increase rents easily in existing contracts under German rental housing laws. One potential problem could be measurement error in the sense that households move into more expensive housing units in expectation of income changes. In this case, a simple OLS regression would over-estimate the true housing expenditure share. To account for this, I instrument for household income by the national-level earnings evolution in the household's industry mix. The household's industry mix is defined as the industries where the household earners were employed in 2010, or when entering the survey. In case there are multiple earners in one household, I weigh the contribution of each person's industry by the respective person's contribution to the total household income in 2010. The regression results are summarized in Table D2.

The simple OLS regression in column (1) reveals a mean housing expenditure share of 21.1%, which is close to the 22.5% reported in Albouy et al. (2016) for the U.S., based on the Census 2000. If earnings are instrumented by the extrapolated average industry earnings, the estimated expenditure share is 23.8%, with a Kleibergen-Paap F of 10.8. Adding controls for the year, the household's current industry composition, and the actual average earnings in the household's current industry only leads to a slightly smaller coefficient. Overall, this suggests that the mean housing expenditure

Table D2: Estimating the Housing Expenditure Share

Dependent variable:	Housing Expenditure		
	(1) OLS	(2) IV	(3) IV
household net income	0.21115***	0.23757***	0.17688**
	(0.00390)	(0.00335)	(0.05977)
average earnings per employee in household's current industries			0.00004
			(0.00007)
Year FE	no	no	yes
Industry FE	no	no	yes
Kleibergen-Paap F	-	10.8	12.6
Observations	2,058	2,031	2,031

Note: Standard errors are clustered by household; *: $p < .05$, **: $p < .01$, ***: $p < .001$. The instrument is the average earning in the industry in which the household earner worked when entering the sample (2010 or later). If there are multiple earners, the industry earnings are weighted by the income share of each earner in the base year.

share is a reliable estimate, so that $\alpha = 0.21$.

Rents-earnings elasticity. In order to identify ϕ, the average local housing supply elasticity, I regress log housing rents on average local earnings at the district-year level (see also Hilber and Vermeulen, 2016, for a similar strategy), year-, calendar month-, and postcode-fixed effects, and a set of housing characteristics.[26] Earnings are measured as average earnings per worker, for the years 2000–2017. Table D3 displays the results.

In column (1), the estimated average rent–earnings elasticty in Germany is 0.248. This estimate is remarkably close to the estimated house price–earnings elasticity of 0.317 by Hilber and Vermeulen (2016) for an average English local authority. Three things are noteworthy here: England is well-known for its very restrictive planning policy that is arguably stricter than the rules-based system of local planning applied in Germany. This suggests that the elasticity for Germany should be much lower than the elasticity for England. Second, rent-earnings elasticities could differ from house

[26]The list of control variables includes the log area, year of construction, indicators for parquett flooring, floor heating, an elevator, a fitted kitchen, a second bathroom, a balcony, garden access, and variables that indicate the type of the unit and its quality.

Table D3: Estimating the Housing Rents–Earnings Elasticity

Dependent variable:	Log Rent per sqm			
	(1) OLS	(2) OLS	(3) OLS	(4) OLS
Log Earnings per Worker	0.2476*** (0.0410)	0.2214*** (0.0471)	0.0669** (0.0231)	
Log Employment		0.0131* (0.0054)		
Lagged Log Earnings per Worker				0.1665*** (0.0372)
Controls	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Postcode FE	yes	yes	yes	yes
Weighted	no	no	yes	no
Observations	6,083,920	6,083,920	6,083,920	7,101,264

Note: Standard errors are clustered by postcode; *: $p < .05$, **: $p < .01$, ***: $p < .001$. The control variables are log area, year of construction and its square, indicators for parquett flooring, floor heating, an elevator, a fitted kitchen, a second bathroom, a balcony, garden access, and variables that indicate the type of the unit and its quality. The regression in column (3) weighs observations from district k by $1/\sqrt{n_k}$, where n_k is the total number of observations from district k.

price-earnings elasticities. Finally, the regressions in Hilber and Vermeulen (2016) are based on a panel of local authorities, whereas the regressions in column (1) are based on individual housing units. That is, they implicitly put much more weight on larger cities than on rural areas—simply because the rental housing markets of larger cities are also larger.

Changes in earnings could be correlated with other demand factors. The Rosen-Roback model suggests that agents are willing to accept a lower wage rate if a location offers more local amenities. According to this argument, wages could have decreased (relatively speaking) in places that got more attractive otherwise. I therefore control for log employment in column (2) so that the wage effect is conditional on the number of people working in a region. This does not affect the earnings coefficient much, suggesting that other demand factors do not correlate strongly with changes in local earnings.

In column (3), in order to make the regression more comparable to a district-level panel regression, I weigh each observation from a district k by $1/\sqrt{n_k}$, where n_k is the total number of observations from district k. This shifts weight away from bigger cities, towards smaller housing markets. The estimated coefficient shrinks to 0.067, suggesting that the places with smaller rental markets (in absolute terms), housing supply is more elastic. Moreover, the average elasticity of housing supply in Germany seems to be much higher than the supply elasticity in England.

If replacing the current log earnings by its lag in column (4), the corresponding elasticity decreases to 0.167, potentially because local supply needs some time to react to a positive shock on the local labor market.

E. Filtering and Optimal Length of Tenure

Here, I consider briefly the household problem of optimal moving choices in a filtering framework with moving costs.

The main empirical analysis is concerned with the impact of a newly built housing unit on the distribution of rents. This unit will be occupied by a household that leaves vacant another housing unit. In order to understand the effects on the distribution of rents, it is important to develop an idea about the quality level of the household's previous housing unit. Consider a housing unit in a building of age t. The flow of housing quality is $q(t)$, and it deteriorates over time ($q' < 0$). Assume that infinitely-lived households discount the future by a factor β. If they want to move house, they have to expense a utility cost c. In order to focus the discussion, I assume that households expect income y and housing costs r to remain constant. Moreover, let the household's instantaneous utility from living in a house of quality $q(t)$ be $v(q(t), y-r)$.

The household's problem is to choose the optimal moving time t^*. Denoting by V_{t_m} the value function of a household who just moved into a new house and always moves house after staying in a housing unit for a period t_m, we have

$$V_{t_m} = \int_0^{t_m} e^{-\beta t} v(q(t), y - r) dt + e^{-\beta t_m} \left(V_{t_m} - c\right). \tag{12}$$

The integral represents the value of living in the current housing unit for t_m units of time. Then, the household loses utility equal to $ce^{-\beta t_m}$ due to the move, and finds herself in the same position as at time 0, but utility needs to be discounted by $e^{-\beta t_m}$. Solving for V_{t_m},

$$V_{t_m} = \frac{1}{1-e^{-\beta t_m}} \left(\int_0^{t_m} e^{-\beta t} v(q(t), y-r)dt - ce^{-\beta t_m} \right). \tag{13}$$

Thus,

$$t^* = \arg\max_{t_m} \left\{ \frac{1}{1-e^{-\beta t_m}} \int_0^{t_m} e^{-\beta t} v(q(t), y-r)dt - c\frac{1}{e^{\beta t_m}-1} \right\}. \tag{14}$$

This term is useful for deriving some very basic predictions. The first term inside the curly brackets represents utility of staying in the dwelling for t_m units of time. Clearly, longer tenures lead to more utility, but this has to be contrasted with potential utility gains from moving into a new unit (where the service flow is higher). This decision depends on the path of $q(t)$. The fraction before the integral represents the summation factor, i.e. the discounted number of individual tenures. The number of tenures goes down if individual tenures are longer.

The second term inside the curly brackets represents the impact of moving costs. Longer tenures imply that households have to pay moving costs less often. In line with this argument, the negative impact of c on utility decreases with t_m.

F. Linear Instrumental Variable Regressions with Quantiles of the Rent Distribution as Outcomes

The regression results described in this appendix correspond to the results from Table 2, column (1). The difference is that the dependent variable, the log hedonic index, refers to the τ-th conditional quantile of the district-level rent distribution (instead of its mean). The hedonic index for district d and quantile τ is derived from an ordinary τ-quantile regression of log rents observed in year t and district d on housing characteristics and year fixed effects (see Appendix Section B). The resulting log index is given by the coefficients on the year-fixed effects.

The panel regression for the conditional τ-quantile equation is

$$\ln \text{Index}^{\tau}_{d,t} = \gamma \frac{S^{(12)}_{d,t-1}}{H_d} + \psi_d + \phi_t + \varepsilon_{d,t}, \tag{15}$$

where the notation the same as for equation 6. The results are displayed in Table F1. They show that the whole rent distribution shifts in response to the supply shock, with slightly smaller movement at the lower end. However, the differences across the different quantiles are never (substantially) larger than the standard error. This corroborates the results from the IVQR approach.

Table F1: Impact of New Housing Supply the District-Level Rent Distribution

Dependent variable:	Log Rent Index								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Index representing quantile $\tau =$	.1	.2	.3	.4	.5	.6	.7	.8	.9
Units completed in Dec of year $t-1$	-23.9*	-29.1**	-27.1**	-26.4**	-26.5**	-27.4**	-30.6**	-29.9*	-33.7*
(share of the stock 2011)	(10.0)	(10.5)	(10.2)	(10.1)	(10.2)	(10.4)	(11.5)	(11.7)	(13.2)
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
District FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Kleibergen-Paap F	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1
Observations	3,136	3,136	3,136	3,136	3,136	3,136	3,136	3,136	3,136

Note: The dependent variables are log indices for the τ-th conditional quantile of the district-level rent distribution. They are based on quantile hedonic regressions, see Appendix Section B for a more detailed description of the indices. The instrument is the rainfall shock in the summer of year $t-1$. Standard errors are clustered by district; *: $p < .05$, **: $p < .01$, ***: $p < .001$.