Titel der Arbeit:
”Individual Housing Decisions, Mortgage Supply and Housing Market Regulations”

Schriftliche Promotionsleistung

zur Erlangung des akademischen Grades

Doctor rerum politicarum

vorgelegt und angenommen

an der Fakultät für Wirtschaftswissenschaft

der Otto-von-Guericke-Universität Magdeburg

Verfasser: Antonios Mavropoulos, M.Sc.

Geburtsdatum und -ort: 19.01.1991, Patras

Arbeit eingereicht am: 23.11.2020

Gutachter der schriftlichen Promotionsleistung:

Prof. Michael Koetter, PhD

Prof. Dr. Felix Noth

Datum der Disputation: 02.03.2021

OTTO VON GUERICKE UNIVERSITY MAGDEBURG

Doctoral Thesis

Individual Housing Decisions, Mortgage Supply and Housing Market Regulations

---

Author:
Antonios Mavropoulos

Supervisors:
Professor Michael Koetter, PhD
Professor Dr. Felix Noth
Junior Professor Huyen Nguyen, PhD

Otto-von-Guericke University Magdeburg

Halle Institute for Economic Research
Member of the Leibniz Association

Department of Financial Markets

Acknowledgments

The last four years have been one of the most enjoyable and positively challenging chapters of my life. And it wouldn't be the same without the people around me.

First and foremost, I am extremely grateful and thankful to my supervisors, Michael Koetter, Qizhou Xiong and Huyen Nguyen. As I started my PhD in 2016, I was only entering a world that I had (and still have) so much to learn. Their guidance, supervision and compassion were essential to my research, my interests and well-being. Michael, Qizhou and Huyen, I am deeply indebted for the opportunities you provided me, the discussions we had, your help and resilience.

Furthermore, I would like to thank the wonderful people of the Halle Institute for Economic Research (IWH). The administrative staff that was always there willing to help me with my lack of comprehension of administrative documents, the technical staff who endured my constant requests for new Python libraries and of course, my fellow PhD students. I would like to especially thank Konstantin Wagner, Joao Carlos Claudio, Talina Sondershaus, Hannes Böhm, Felix Pohle, Moritz Stieglitz, Michael Ghisletti, Stefano Colonnello, Felix Noth, Thomas Krause, Carolla Müller, Oliver Rehbein and Philipp Marek for their support, the coffees, the drinks and the laughs.

I want to express my gratitude to the Institution where I conducted my PhD, the IWH, a place for factual scientific research and discussions, which shaped me significantly as a researcher for the last four years. I am also very grateful for the financial support that was provided by the IWH, which allowed me to travel, to discuss research and to meet incredibly accomplished and interesting people. Furthermore, together with OvGU, they provided me with the opportunity to teach in tutorials, which is something that I really enjoyed. I could not wish for a better place.

Lastly, I want to thank my family. My sister Anna, my mother Christina and my father Nikos. Thank you for your support, your patience and the liters upon liters of olive oil. I would not be who I am without you.

Foreword

Housing is an essential durable consumption good and oftentimes the largest and most important investment a household makes. The way households finance their housing is important not only for expenditure patterns but also for asset accumulation. As Chambers et al. (2009) explain, housing investment, for both residential and nonresidential structures comprises about half of all private investments and the liabilities from home mortgages are approximately equal to two-thirds of gross domestic product. It is very closely linked with the financial market via the housing backed mortgages taken by the majority of home-owners and it is intrinsic to real economic activity.

An unfortunate paradigm of such interdependence was the subprime mortgage crisis which took place in 2007. In the years that ensued we experienced one of the most significant downturn of economic events in the last century, known as the Great Recession, with its adverse macroeconomic effects spreading well beyond the United States. In its aftermath, conventional monetary policy in the Euro area introduced a low interest rate environment, aiming to boost economic activity. The increased lending incentives induced from low interest rates consequently raised concerns as a contributing factor for over-heated real estate markets (Del Negro and Otrok (2007), Taylor (2009)).

The goal of this dissertation is to investigate the evolution of house prices and housing consumption choices in post crisis Europe, under the effects of macroprudential, fiscal and other unconventional interventions on the real estate markets from a borrower-based perspective. An intrinsic aspect of such analysis entails the detailed monitoring of real estate markets. House price over-heatings can also be attributed to scarcity of housing supply and lack of developable land (Glaeser et al. (2008), Saiz (2010), Paciorek (2013), Hilber and Vermeulen (2016)) which is more prevalent in large urban areas and thus, heterogeneous effects of policy intervention can range from a successful deceleration of upward housing trends to adverse welfare outcomes on credit constrained households in rural areas. Therefore, among others, the dissertation contributes to the established literature on investigating local real estate fluctuations with the implementation of novel, very granular and frequent housing price data.

As the global financial crisis reminded policymakers that traditional macroeconomic polices were less effective in the management of boom-bust cycles in real estate markets (Crowe et al. (2013), Claessens et al. (2013), Mendicino and Punzi (2014)), macroprudential policy and regulation became more relevant than ever, both for financial institutions (Basel III) with the introduction of counter-cyclical capital requirements and systemic risk buffers, as well as borrower-based measures, in the form of loan-to-value and loan-to-income ratios among others. Researchers and policymakers started to discuss the implementation of such policies in order to curb the housing demand, slow the housing price increase and mitigate the probability bubble build-ups (Igan and Kang (2011), Craig and Hua (2011), Detragiache et al. (2012), Kuttner and Shim (2016) and Poghosyan (2020)). However, the effectiveness of such regulations remains open. Not only in the ability to reduce the ascending trend of house prices as Arena et al. (2020) discuss, but also in their ability to reduce the levels of financially troubled households.

This dissertation consists of four independent scientific contributions. The paper *"Housing Consumption and Macroprudential Policies in Europe: An Ex Ante Evaluation"* is the empirical and theoretical analysis of housing consumption, tenure choice, financing decisions and investment opportunities deriving from counterfactual macroprudential policies on the housing market. Following on, the paper *"Real estate transaction taxes and credit supply"* investigates the endogenous relationship between house price growth and mortgage growth by using novel granular data on house prices and exploiting the cross-sectional dependence of mortgage lending of German savings banks. It also provides empirical evidence of fiscal policy on bank lending, and compares the findings with potential macro-prudential instruments targeting the deceleration of house prices. The paper *"Monitoring Real Estate Markets using Lag-Free House Price Indices"* introduces high frequency, lag-free house price indices for European housing markets. Furthermore, it provides evidence on the common house price index methodologies and aims to cover the one to two quarter lag of Eurostat's house price indices. This paper aims to show the importance of lag-free monitoring of real estate markets across countries, using harmonized house price indices as a tool useful for researchers and policy-makers alike. Finally, the paper *"To rent or not to rent: A household finance perspective on Berlin's short term rental regulation"* investigates potential adverse effects of regulation on the sharing housing economy (or else the short-term rental housing market) on the household finance decisions of commercial landlords. Whereas the aforementioned papers investigate the effects of macroprudential and fiscal policy on housing decisions and mortgage lending respectively, the final one investigates whether an unconventional regulation aiming to decrease housing scarcity interfered with household finances and introduced long-term adverse effects on the housing market.

The paper *"Housing Consumption and Macroprudential Policies in Europe: An Ex Ante Evaluation"*, which is co-authored with Qizhou Xiong[1], builds a parsimonious model of dynamic housing and mortgage decision, that allows households to optimize both their mortgage and housing size. We exploit the second cross sectional survey of the HFCS[2] to create a panel component of households, in order to measure changes in home-ownership status. The underlying assumption is that households choose to become an owner and take a mortgage when the optimized utility of owning is higher than renting. While they face uncertainty from labor income, housing purchase and rental prices and make decisions based on their expectation and preferences, the research question is how macroprudential policy, in the form of LTV and LTI ratios can affect their housing tenure choice, consumption adjustment and welfare.

This paper provides a comprehensive account of the housing decisions of European households in multiple countries during the post-crisis period. We also contribute by structurally estimating the housing demand using a partial equilibrium life-cycle model in the European context. With the households deriving utility both from non-durable consumption and housing service, and while following in the theoretical footsteps of Campbell and Cocco (2015), we innovate by introducing a much more flexible budget constraint for renters, as they can optimize their utility each period, by reducing or increasing their housing consumption. Third, the ex-ante policy evaluations offer valuable insights on the possible financial and welfare consequences to the households if certain housing market policies are implemented.

Our research question remains in-between the realm of the literature on the determinants of over-indebted households (Bloom et al. (1997), Stango and Zinman (2009), Lusardi and Tufano (2009), Lusardi and Mitchell (2011), McCarthy (2011), Wong et al. (2011) and Albacete et al. (2013)) and the literature strand on the tenure choice of households ((Henderson et al. (1983), Campbell and Cocco (2003), Sinai and Souleles (2005) and Chambers et al. (2009)).

[1] Saïd Business School, University of Oxford, United Kingdom.
[2] Household Finance and Consumption Survey, ECB.

Our findings suggest that tighter regulations have a significant effect on households' choice of housing tenure and size as they limit the entry to the housing market at an early stage of the households' life cycle and slow their wealth accumulation. We also find that the two macroprudential instruments yield similar results: tighter regulation forces the households to take smaller housing units to circumvent the tighter budget constraints.

The paper *"Real estate transaction taxes and credit supply"*, which is co-authored with Michael Koetter[3] and Philipp Marek[4], investigates the endogenous interdependence between the housing and the financial market for credit through fiscal policy regulation. Financial crises are frequently preceded by real estate booms (Reinhart and Rogoff, 2008; Brunnermeier and Schnabel, 2015), which are more likely to emerge in times of very loose monetary policy at the zero-lower bound. The role of unconventional fiscal policy to stimulate economic activity instead of monetary policy has been studied (Correia et al., 2013; D'Acunto et al., 2018), but whether it can also help to smooth credit cycles is unclear. Likewise, the scope of macroprudential policy to contain asset price bubbles is increasingly well understood (Aikman et al., 2019), but the question if and to what extent fiscal policy can assist in mitigating exuberant mortgage credit growth remains open.

Our approach to answer if fiscal policy can contain mortgage lending by its effect on real estate prices exploits the staggered introduction of Real Estate Transaction Taxes (RETT, *"Grunderwerbsteuer"*) across the 16 federal states of Germany on quality-adjusted regional house price indices (HPI). Using instrumented real estate prices, we combine regional HPI with detailed bank-level data to isolate mortgage credit supply adjustments due to fiscal policy shocks. This setting of autonomous tax changes paired with granular bank and real estate market data overcomes the notorious challenge that real estate prices and credit supply are jointly determined (Gerlach and Peng, 2005; Gimeno and Martinez-Carrascal, 2010; Hott, 2011).

This paper lies between the literature on the effects of fiscal policy on housing transactions and price depreciation (Dachis et al. (2011), Fritzsche and Vandrei (2016), Petkova and Weichenrieder (2017)) and the studies on the effects of asset price changes on the financial decisions by households and firms (Mian and Sufi (2011), Cvijanović (2014), Adelino et al.

[3]Otto-von-Guericke University Magdeburg, Halle Institute for Economic Research (IWH) and Deutsche Bundesbank.

[4]Deutsche Bundesbank.

(2015)). We contribute to the established literature by providing the linkage of house price fluctuations on mortgage lending demand through the prism of fiscal policy.

Our results show that fiscal policy hikes can contain mortgage lending through a preceding deceleration of purchasing and rental prices. We also find that these effects are more profound in rural regions and that urban ones are more inelastic to changes in the fiscal regime. We also find a homogeneous reaction of mortgage lending based on bank capitalization, which is an important finding from a financial stability perspective.

In the single-authored paper *"Monitoring Real Estate Markets using Lag-Free House Price Indices"*, I aim to complement the already existing European house price indices using novel, granular and frequent data. The main contribution of this paper is to allow less costly and lag-free monitoring of real estate markets from the national (NUTS1) to the regional (NUTS2 and NUTS3) level.

I do so by constructing cross country harmonized house prices indices through the collection of web-scraped data across 14 Euro area countries. The indices are then compared (in a similar fashion to Fernald (2014)) with the Eurostat indices for each country.

Whereas the established house price indices from Eurostat, due to their large time span, offer the chance of observing house price cycles, and are useful for inflation targeting and economic forecasting among others, they fall short on two aspects. First, collection and harmonization processes among the various sources of national data are costly in terms of time, resulting in a one to two quarter lag of the published indices. Second, they do not capture regional discrepancies, which are important for understanding heterogeneous housing trends and policy implications. My aim is to fill the gap by providing researchers and policy-makers with harmonized indices that allow timely interventions both on the national as well as the regional context.

In the single-authored paper: *"To rent or not to rent: A household finance perspective on Berlin's short term rental regulation"*, I investigate a dense and appreciating regional housing market, where due to high demand and housing scarcity, regulators imposed an unconventional measure on the sharing economy for housing in order to increase long-term housing supply.

Complementing the empirical literature on the sharing economy for housing and its effects on housing purchase and rental prices (Guttentag (2015), Coldwell (2017), Horn and Merante

(2017), Ioannides et al. (2018) and Koster et al. (2018)), I examine the effects of a ban of short-term housing listings on household finance decisions from a commercial home-owner's perspective. More specifically, I investigate whether Berlin's ban of Airbnb listings in 2016 increased the short-term rental income for non-conforming commercial home-owners, due to a reduced supply of offers and steady demand. I consequently ask as whether these higher motives of participating in the sharing economy for housing due to increased income realized from the short-term market can consequently accelerate rental and purchase prices.

The paper contributes to the literature by combining data on the short and long-term housing markets in order to create a novel counterfactual measure of sharing economy intensity and by looking at the resulting evidence of policy from a household finance perspective.

The results suggest that within the first year of the regulation short-term rental income for commercial home-owners increased by 50%, which was mainly attributed to the decreased supply. I furthermore find that this result did not have a strong effect on house purchase and rental prices, but it increased the incentive of participating in the sharing economy for housing. Although I find that the regulation was not able to curb an ever increasing trend in house prices, I supplement the established literature by illustrating the causal positive effect of the supply of short-term housing units on rental and purchase prices.

References

Adelino, M., A. Schoar, and F. Severino (2015). House prices, collateral, and self-employment. *Journal of Financial Economics 117*(2), 288–306.

Aikman, D., J. Bridges, A. Kashyap, and C. Siegert (2019). Would Macroprudential Regulation Have Prevented the Last Crisis? *The Journal of Economic Perspectives 33*(1), 107–130.

Albacete, N., P. Lindner, et al. (2013). Household vulnerability in Austria – A microeconomic analysis based on the household finance and consumption survey. *Financial stability report 25*, 57–73.

Arena, M., T. Chen, S. M. Choi, N. Geng, C. A. Gueye, T. Lybek, E. Papageorgiou, Y. S. Zhang, et al. (2020). Macroprudential policies and house prices in europe. Technical report, International Monetary Fund.

Bloom, M., M. Burrows, B. Lafleur, and R. Squires (1997). The economic benefits of improving literacy skills in the workplace. ERIC.

Brunnermeier, M. and I. Schnabel (2015). Bubbles and central banks: Historical perspectives. Technical report, CEPR Discussion Papers.

Campbell, J. Y. and J. F. Cocco (2003). Household risk management and optimal mortgage choice. *The Quarterly Journal of Economics 118*(4), 1449–1494.

Campbell, J. Y. and J. F. Cocco (2015). A model of mortgage default. *The Journal of Finance 70*(4), 1495–1554.

Chambers, M. S., C. Garriga, and D. Schlagenhauf (2009). The loan structure and housing tenure decisions in an equilibrium model of mortgage choice. *Review of Economic Dynamics 12*(3), 444–468.

Claessens, S., S. R. Ghosh, and R. Mihet (2013). Macro-prudential policies to mitigate financial system vulnerabilities. *Journal of International Money and Finance 39*, 153–185.

Coldwell, W. (2017). First venice and barcelona: Now anti-tourism marches spread across europe. *The Guardian 10*, 2017.

Correia, I., E. Farhi, J. P. Nicolini, and P. Teles (2013). Unconventional Fiscal Policy at the Zero Bound. *American Economic Review 103*(4), 1172–1211.

Craig, M. R. S. and M. C. Hua (2011). *Determinants of property prices in Hong Kong SAR: Implications for policy.* Number 11-277. International Monetary Fund.

Crowe, C., G. Dell'Ariccia, D. Igan, and P. Rabanal (2013). How to deal with real estate booms: Lessons from country experiences. *Journal of Financial Stability 9*(3), 300–319.

Cvijanović, D. (2014). Real estate prices and firm capital structure. *The Review of Financial Studies 27*(9), 2690–2735.

Dachis, B., G. Duranton, and M. A. Turner (2011). The effects of land transfer taxes on real estate markets: Evidence from a natural experiment in toronto. *Journal of economic Geography 12*(2), 327–354.

D'Acunto, F., D. Hoang, and M. Weber (2018). Unconventional fiscal policy. *American Economic Review Papers & Proceedings 108*(5), 519–523.

Del Negro, M. and C. Otrok (2007). 99 luftballons: Monetary policy and the house price boom across US states. *Journal of Monetary Economics 54*(7), 1962–1985.

Detragiache, E., U. Vogel, J. Vandenbussche, and I. M. Fund (2012). *Macroprudential Policies and Housing Prices–A New Database and Empirical Evidence for Central, Eastern, and Southeastern Europe*. International Monetary Fund.

Fernald, J. (2014). A quarterly, utilization-adjusted series on total factor productivity. Federal Reserve Bank of San Francisco.

Fritzsche, C. and L. Vandrei (2016). The german real estate transfer tax: Evidence for single-family home transactions. Technical report, ifo Working paper.

Gerlach, S. and W. Peng (2005). Bank lending and property prices in hong kong. *Journal of Banking & Finance 29*(2), 461–481.

Gimeno, R. and C. Martinez-Carrascal (2010). The relationship between house prices and house purchase loans: The spanish case. *Journal of Banking & Finance 34*(8), 1849–1855.

Glaeser, E. L., J. Gyourko, and A. Saiz (2008). Housing supply and housing bubbles. *Journal of Urban Economics Volume 64, Issue 2, September 2008, Pages 198-217 Journal of Urban Economics 64*(2), 198–217.

Guttentag, D. (2015). Airbnb: disruptive innovation and the rise of an informal tourism accommodation sector. *Current issues in Tourism 18*(12), 1192–1217.

Henderson, J. V., Y. M. Ioannides, et al. (1983). A model of housing tenure choice. *American Economic Review 73*(1), 98–113.

Hilber, C. A. and W. Vermeulen (2016). The impact of supply constraints on house prices in england. *The Economic Journal 126*(591), 358–405.

Horn, K. and M. Merante (2017). Is home sharing driving up rents? Evidence from airbnb in boston. *Journal of Housing Economics 38*, 14–24.

Hott, C. (2011). Lending behavior and real estate prices. *Journal of Banking & Finance 35*(9), 2429–2442.

Igan, D. and H. Kang (2011). Do loan-to-value and debt-to-income limits work? Evidence from korea. *IMF Working Papers*, 1–34.

Ioannides, D., M. Röslmaier, and E. van der Zee (2018). Airbnb as an instigator of 'tourism bubble' expansion in Utrecht's lombok neighbourhood. *Tourism Geographies*, 1–19.

Koster, H., J. van Ommeren, and N. Volkhausen (2018). Short-term rentals and the housing market: Quasi-experimental evidence from airbnb in Los Angeles.

Kuttner, K. N. and I. Shim (2016). Can non-interest rate policies stabilize housing markets? Evidence from a panel of 57 economies. *Journal of Financial Stability 26*, 31–44.

Lusardi, A. and O. S. Mitchell (2011). Financial literacy and planning: Implications for retirement wellbeing. Technical report, National Bureau of Economic Research.

Lusardi, A. and P. Tufano (2009). Debt literacy, financial experiences, and overindebtedness. Technical report, National Bureau of Economic Research.

McCarthy, Y. (2011). Behavioural characteristics and financial distress.

Mendicino, C. and M. T. Punzi (2014). House prices, capital inflows and macroprudential policy. *Journal of Banking & Finance 49*, 337–355.

Mian, A. and A. Sufi (2011). House prices, home equity-based borrowing, and the US household leverage crisis. *American Economic Review 101*(5), 2132–56.

Paciorek, A. (2013). Supply constraints and housing market dynamics. *Journal of Urban Economics 77*, 11–26.

Petkova, K. and A. J. Weichenrieder (2017). Price and quantity effects of the german real estate transfer tax.

Poghosyan, T. (2020). How effective is macroprudential policy? Evidence from lending restriction measures in eu countries. *Journal of Housing Economics*, 101694.

Reinhart, C. M. and K. S. Rogoff (2008). Is the 2007 US sub-prime financial crisis so different? An international historical comparison. *American Economic Review 98*(2), 339–344.

Saiz, A. (2010). The geographic determinants of housing supply. *The Quarterly Journal of Economics 125*(3), 1253–1296.

Sinai, T. and N. S. Souleles (2005). Owner-occupied housing as a hedge against rent risk. *The Quarterly Journal of Economics 120*(2), 763–789.

Stango, V. and J. Zinman (2009). Exponential growth bias and household finance. *The Journal of Finance 64*(6), 2807–2849.

Taylor, J. B. (2009). The financial crisis and the policy responses: An empirical analysis of what went wrong. Technical report, National Bureau of Economic Research.

Wong, T., T. Fong, K.-f. Li, and H. Choi (2011). Loan-to-value ratio as a macroprudential tool - Hong Kong's experience and cross-country evidence.

Housing Consumption and Macroprudential Policies in Europe: An Ex Ante Evaluation[*†‡]

Antonios Mavropoulos [§]   Qizhou Xiong [¶]

Abstract

In this paper, we use the panel of the first two waves of the Household Finance and Consumption Survey by the European Central Bank to study housing demand of European households and evaluate potential housing market regulations in the post-crisis era. We provide a comprehensive account of the housing decisions of European households between 2010 and 2014, and structurally estimate the housing preference of a simple life-cycle housing choice model. We then evaluate the effect of a tighter LTV/LTI regulation via counter-factual simulations. We find that these regulations limit homeownership and wealth accumulation, reduce housing consumption but may be welfare improving for the young households.

Keywords: Housing Consumption, Macroprudential policies, LTV/LTI regulation
JEL Classifications: D14, D31, D91

[*]We thank Michael Koetter and Michael Halliassos for their comments and benefited from feedback received at the IWH Doctoral seminar, the FWW Research Seminar at OvGU Magdeburg and the Topics in Accounting and Financial Economics workshop from Goethe university.

[†]This paper is the current version of the working paper: Mavropoulos and Xiong (2018). All co-authors contributed equally.

[‡]Conference Presentations: Italian Society of Public Economics XXX Annual Meetings, Padova, Italy, September 2018; Society of Economics of the Household (SEHO) 2019 meetings, Lisbon, Portugal, May 2019.

[§]Halle Institute for Economic Research (IWH), Germany. E-mail: antonios.mavropoulos@iwh-halle.de

[¶]Saïd Business School, University of Oxford, United Kingdom. E-mail: qizhou.xiong@sbs.ox.ac.uk

1 Introduction

After the painful and costly lesson from the Great Recession, loan-to-value (LTV) and loan-to-income (LTI) regulations have become part of the widely accepted macroprudential policy toolkits. There has been worldwide application of such policies to ensure healthy micro level household indebtedness and prevent the housing bubble from resurfacing by curbing the housing demand through borrowing constraints. The necessity and effectiveness of such regulations depend on the household housing demand and how much they are credit constrained. While some cases have been proven successful by ex post policy evaluation (Mitra et al., 2015), we still need to be cautious in claiming universal effectiveness. Moreover, most of the policy evaluations heavily focus on the market aggregate and macroeconomic indicators but remain silent on the micro level impacts on household finance and welfare.

Some European countries, especially Germany, have witnessed substantial housing price increase and low mortgage rates in the past few years. The concern of overly leveraged household portfolios and housing market bubble building has led to the discussion of implementing housing related macroprudential policies in the euro-zone countries. For instance, the German Ministry of Finance had proposed a draft law aimed at tightening residential mortgage lending market regulations in late 2016. The essential information we need to make sensible policy decisions is the answer to the following two questions: first, how do European households make housing consumption decisions; second, what will be the consequences of specific policy implementation? We answer the first question by documenting the housing consumption choices at both intensive and extensive margin and structurally estimate the parameters that primarily govern the housing consumption preferences: the consumption share and the elasticity of substitution between the housing and non-durable good consumption. We then answer the second question by simulating multiple policy scenarios of potential LTV/LTI regulations to conduct ex ante policy evaluation.

First, we provide novel empirical evidence of the recent housing demand (2010-2014) in Europe at the household level. According to the European Systemic Risk Board statistics, in this period, most of the western European countries do not have any LTV regulation in place[1] We investigate the untethered housing consumption decision at both intensive and extensive margins using the observed housing size change and tenure transition between 2009/2010 and 2014 in European countries. The two-wave short panel of the Household Finance and Consumption Survey (HFCS) of the ECB enables us to accurately identify the renters who transitioned to home-owners between 2010/2009 and 2014. We use this well-identified housing tenure transition to study the main driving forces of housing

[1] `https://www.ecb.europa.eu/pub/fsr/html/measures.en.html` The only exception is the Netherlands, which reduced tax exemption for the mortgage payment. Ireland started implementing LTV/LTI regulation after 2014.

demand change, similar to Blickle and Brown (2016) which use the Swiss Household Panel (SHP) to study the treatment effect of an exogenous wealth transfer on homeownership. The panel part of HFCS includes the observations from Belgium, Cyprus, Germany, Italy, Malta, and the Netherlands. It contains information on 7449 households in two waves, the first one in 2010 and the second in 2014.

We find that around 60 percent of both renters and owners adjust their housing consumption at the intensive margin between 2010 and 2014 without changing their tenure status. Although the total percentage of intensive adjustment accumulates during the four year period, the share of households adjusting their housing consumption is high, especially for owners. We also observe that there are more households trading-up than trading-down for both renters and owners, which is in line with the prediction of a typical life cycle model in which as households amass their wealth, they start to consume more both non-durable goods and housing. Moreover, on top of a substantial proportion of intensive housing consumption adjustment, many households also report housing size adjustment without changing their primary residence. This suggests that European households actively adjust their housing consumption without incurring the costly sale and repurchase process.

We then look into the housing consumption decision at the extensive margin through the lens of a standard optimal portfolio model with a focus on housing and mortgage. We employ a highly stylized model, in line with the classic housing choice theories (Henderson et al., 1983; Grossman and Larogue, 1990; Campbell and Cocco, 2003), to guide the empirical exercises. We find that housing preference shocks, such as family size and marital status, have a positive impact on the home-ownership transition as predicted in the previous studies. However, the background risk like income growth and volatility do not have a significant impact. These findings confirm the previous theoretical and empirical results in the literature. To our surprise, we find that households are less willing to transition to home-ownership where the house prices are increasing quickly, which suggests that the future housing value may not be the primary driving force of housing decision.

Second, we build a life-cycle housing decision model in partial equilibrium setting à la (Campbell and Cocco, 2015; Li et al., 2016; Landvoigt, 2017) and then structurally estimate the housing preference. We assume the housing market conditions (housing supply and housing prices), labor income and financial market conditions (mortgage rates, return on liquid financial assets) are exogenous and stochastic. At each period, households make forward-looking decisions on whether to purchase a residence and how much housing to consume for both renters and owners. We allow the households to breach their borrowing constraint only through mortgage taking and obtain a significant terminal value at the end of the mortgage. We also impose that the households face transaction costs when purchasing a new property and also aim to reduce their outstanding loans when remaining stable on housing tenure. We apply a two-step process proposed by Bajari et al. (2007) to numerically solve the model and structurally estimate the parameters of interest. We find that European households have almost unit elasticity of substitution between housing and non-durable goods and have a relatively high consumption share compared with the previous results found in the US data.

Finally, we investigate the possible change of a tighter LTV and LTI regulation: the LTV decreases from 80% to 60% and LTI ratio at 4.5.[2] We find that those tighter regulations have a significant effect on households' choice of housing tenure and housing size. We choose two representative households for the ex ante evaluation of the policies: a 25-year-old household with average income and no wealth and a 30-year-old household with average income and average wealth. We find that the regulations limit the entry to the housing market at an early stage of their life cycle and slow their wealth accumulation. Moreover, the regulation also forces the households to choose smaller housing units as they finally transition to home-ownership. The wealth difference caused by the tighter regulation can be as substantial as 20,000 euros by the LTV regulation and 40,000 euros by LTI regulation. However, the welfare level computed using the empirical utility estimated from the structural model suggests that such regulation tightening may be welfare improving. This is likely due to the fact that those regulations prevent households from prematurely invest in risky housing assets.

We believe that this paper makes a few empirical contributions to the housing service demand literature and the ongoing discussion of housing market regulations. First, this paper provides a comprehensive account of the housing decisions of European households in multiple countries during the post-crisis period when the mortgage rates and real interest rates are low. The comparison shows that households in Europe make significantly different housing choices than American ones. Second, we are among the first to structurally estimate the housing demand using a partial equilibrium life-cycle model in the European context. Third, the ex ante policy evaluations offer valuable insights on the possible financial and welfare consequences to the households if certain housing market policies were implemented.

The remainder of the paper is structured as follows. Section 2 presents the empirical evidence on housing size adjustment, and Section 3 presents the empirical results of the renter to owner transition. Section 4 builds a simple theoretical framework to illustrate household housing demand and the housing tenure transition conditions. In Section 5, we discuss the strategy of the numerical solution and structural estimation. Section 6 presents the ex-ante evaluation of LTV and LTI regulation. Finally, Section 7 concludes.

[2]The LTI is defined as the ratio of loan to the annual gross income of the household.

2 Data

We use the Household Finance and Consumption Survey (HFCS) by the European Central Bank, which is a centralized effort in collecting European household finance data via the national central banks of the Euro-system and many national statistical institutes. The HFCS collects detailed financial and consumption information at a household level.[3] The first wave of the data was surveyed in 2008, 2009 and 2010[4] and the second wave was surveyed in 2014. There are six countries whose central banks follow up the households in the first wave and construct a short panel data with a unique household identifier. These are Belgium, Cyprus, Germany, Italy, Malta, and the Netherlands. A total of 7.449 households show up in both waves.

The main driving force of housing demand usually comes from the new purchase of houses from either owner buying the second house or renters becoming owners. As the data shows a deficient percentage of multiple home-ownership, we only break down the demographics of the renter-to-owner transition to answer the question who are becoming new owners in Europe. In Table 1, we present the summary statistics of the most relevant variables for housing tenure transition decisions. With only 8.88 percent of the initial renters purchasing houses, about half of our sample comprises of male heads of households, which makes this sample gender balanced. Almost 25 percent of them have finished tertiary education, which is also consistent with the overall sample average. Almost half of the sample are employees or have their own business, and almost 50 percent of the whole sample is married. Our proxy variables for a preference shock display that 3.8 percent had a positive employment shock, 6.7 percent got married in between the two waves and that in 6.9 percent of the households, family size increased. For background risk, we observe an average 1.04 percent income growth from the previous period, and that income expectations are truncated to the positive side, as we formulate this discrete variable 1 when income expectations have positively changed, 2 when they remained the same and 3 when there was an adverse expectation change. For our future value or the investment motive of a transition to home-ownership, we have distinguished between the assigned mean and volatility values between the two waves. We observe that originating from 2011 (the year the first wave of the survey was conducted) and three years before that; house prices were on average ascending but were quite volatile. Then the average values from 2011 till 2014 for growth and volatility display a small on average decrease in house prices and a much lesser variance than in the first wave.

–Table 1 here –

[3]Some individual information is also collected, which enables intra-household economic analysis as well.

[4]The first wave of the data was surveyed at a different time in different countries. The data from Spain was surveyed in 2008, Finland and the Netherlands in 2009, and the rest of the countries in 2010.

The data also shows a significant country difference in home-ownership and housing consumption. In EMU countries, despite the single currency and integrated financial market, there is significant country heterogeneity in home-ownership. Figure 1 shows the cross-country housing tenure differences in details [5]. We can see that due to cultural or institutional differences, the housing tenure composition is highly heterogeneous within the euro-zone. For instance, complete ownership of residence is particularly low in Austria and Germany compared to other developed economies in western Europe like France, Belgium, and the Netherlands.

– Figure 1 here –

Moreover, free use of the residence, presumably from parents and relatives, takes around 10 percent of the possible source of housing with Italy having the highest among developed western European economies. Furthermore, we look at the transition of housing tenure on a country-average level to provide more evidence on the cross-country difference in Table 2. We can see that the overall home-ownership in Europe has increased mildly between the two waves, but the dynamics of housing tenure are profoundly different. For instance, Malta has seen a 33.3 percent increase in home-ownership likely due to speculative investment from the Russian wealthy, while Austria and Netherlands witnessed a slight decline in home-ownership. We unfortunately cannot investigate country heterogeneity in terms of housing options and decisions. The summary statistics, however, suggest that simply ignoring it would be an oversight.

– Table 2 here –

This illustrates the importance of taking the institutional differences, especially tax incentives, into account when we study the housing decisions. Another explanation for the high Dutch home-ownership rates is the availability of credit[6]. As Clapham et al. (2012) explain, after the financial crisis of 2007 there was a decline in the formation of new households in the UK. They argue that the main reason for this decline was the lack of credit availability. Stricter macro-prudential regulation on the housing market, via tighter credit supply standards and lower thresholds for LTV ratios, averts households from transitioning into home-ownership. In the U.S. 82.1 percent of home-owners have acquired their primary residence through a mortgage[7]. In Europe, the percentage is significantly lower (Household Finance and Consumption Survey, ECB). Unfortunately, we do not observe any changes in the macro-prudential regulation within the period of

[5]To have a bigger picture of the housing tenure in Europe, we also include the countries that are not in the panel.

[6]The loan-to-value threshold in the Netherlands for new homeowners is 106 percent and will change in 2018 to 100 percent of the total value of the household

[7]The Census Bureau Residential Finance Survey (RFS).

the data. We take a simplified approach to account all the cultural and institutional differences by adding a country fixed effect to the baseline empirical exercise.

We first look at the extensive margin of housing decisions of the six countries in the panel data. The transition matrix in Table 3 illustrates how the households in those six countries adjust their housing demand at the extensive margin between 2010 and 2014. We do not the consider free use as an option of housing consumption for the moment, and focus on the trade-off between renting and owning. For each household, the HFCS reports the housing tenure status (i.e., whether the household owns or rents the dwelling). A household which was a renter in the first wave and a home-owner in the second wave is defined as $Renter - to - Owner = 1$. On the other side, those who rent in both waves are classified as $Renter - to - Owner = 0$. Out of these households, 8.79 percent became home-owners in the second wave; while only 1.71 percent of home-owners became renters in the second wave. Such asymmetry in housing tenure transition confirms the theoretical prediction of the life cycle of household finance, that households slowly move towards ownership as they age. However, despite the four-year gap between two waves of the panel data, we do not see a significant fraction of households make housing tenure change. We can see that the majority of renters and owners retain their housing tenure status.

– Table 3 here –

As housing decisions have a strong life-cycle pattern and age cohort effect, we then look at the country difference in housing tenure transition in different age groups. Figure 2 depicts the age profile of households which transition in different countries. Interestingly, young Dutch households have a high propensity to home-ownership thanks to mortgage tax deductions, as well as one of the loosest loan-to-value regulations. We observe a highly irregular pattern of housing tenure transition in Cyprus, where many senior citizens decided to own houses. Therefore we remove Cyprus data in the principal analysis to avoid bias.

– Figure 2 here –

Moreover, we break down the housing tenure transition concerning age, income and wealth in Table 4. With the limited young households in the survey, we can still see that the younger households are more likely to move across all categories. Overall there is no substantial difference between movers and stayers regarding income and wealth. However, we do observe that higher income owners are more likely to move to a new property, and young households with more wealth are more likely to move (likely to become a new homeowner).

– Table 4 here –

The expectation of appreciating housing prices in the near future also encourages people to purchase houses according to standard housing tenure choice models (Landvoigt, 2017; Davidoff, 2006). We, therefore, collect house price growth data from Eurostat to investigate whether there has been a substantial housing bubble in recent years. Figure 3c shows the self-evaluated house price per squared meters according to the owners in our survey data. We observe a substantial house price heterogeneity across countries. It is most evident that households in the Netherlands have much higher average house price evaluations compared with other western European countries. Another important aspect of the housing price is that the subjective belief of price can be self-realizing just like the inflation in classic macroeconomic models. Thus we use the self-evaluated house prices reported in the survey to see whether they deviate from the realized house price growth. This may shed light on how optimistic the European households are about the house prices. Figure 3d shows that the self-evaluated housing price shows a moderate level of optimism. Households perceive higher growth when the housing price is on the rise and lower drop when it decreases. However, there is no misconception about the housing market from the households, whereas the self-evaluated house prices are reasonably close to the levels observed on the market. More interestingly, the three countries that experienced a house price decline from 2010 to 2014 are also the countries we see a decline in home-ownership rates as illustrated in Table 2.

– Figure 3c and Figure 3d here –

We now look at the intensive margin to see how households adjust their housing consumption regarding housing size. Figure 4 shows housing consumption with regard to the actual size measured by square meters in four different tenure transition groups. We can see that most of the household who do not change their tenure status do not adjust their housing consumption size often; and for those who do, the adjustment for renters and owners seem to be symmetric. The renters-to-owners are more likely to trade up, and owner-to-renters are more likely to trade down. Table 5 summarizes housing size change in different housing tenure groups. The owners seem almost to have the identical percentages when trading up, trading down and maintaining their housing size consumptio.

– Figure 4 here –

In the classic housing choice theory, we often assume that the homeowners are locked in with their house and mortgage contract and are much less likely to adjust housing size. However, it seems that European homeowners are as active in adjusting housing consumption as renters even when we only look at the owners with one property. Owners of just one property do not have the option of moving back and forth between their properties, which makes the change in housing size more likely to be sales and repurchase of houses.

– Table 5 here –

3 Empirical Evidence on Housing Consumption

In this section, we investigate the driving forces of housing tenure transition and housing consumption level in the post-crisis Europe. We first look at the extensive margin of the housing tenure choice – the housing tenure status change, in order to test the impact of the candidate factors guided by the previous researches. Then we move on to study the intensive margin of housing consumption adjustment when households do not switch their housing tenure status. However, this does not mean that the households do not change their primary residence. Home-owners moving to a new property with a different level of housing service also count as an intensive margin of housing consumption adjustment.

3.1 Housing Tenure Choice

Housing tenure choice plays a critical role in inter-temporal consumption smoothing and has great asset pricing implications (Grossman and Larogue, 1990; Chambers et al., 2009; Flavin and Yamashita, 2002; Flavin and Nakagawa, 2008; Chetty et al., 2017). The marginal owners (renter-to- owner transition) offer us great insight into the precise decision-making process of housing tenure choice. Our empirical analysis of housing tenure transition follows the housing choice models lead by Henderson et al. (1983), which illustrate the complexity and principal drivers of housing decision: household characteristics, ownership preference/premium, housing price and future price expectation, capital market conditions (i.e. mortgage rates, risk-free return), labor income growth, and down-payment restrictions (Gete and Reher, 2016; Cameron and Tracy, 1997; Blickle and Brown, 2016; Davidoff, 2006; Fuster and Zafar, 2016). We employ a simple reduced form logistic model for the baseline analysis of the determinants of transition to home-ownership in the six euro-zone countries. We set the dependent variable Renter-to-Owner $RtO = 1$ if a household transitioned from renting to owning and to $RtO = 0$ if the household remains a renter in the second wave. Similarly, we construct the binary variable $OtR = 1$ as owners transition to renters. We then apply a probit model to analyze the determinants of housing tenure choices:

$$Pr(RtO = 1 \quad \text{or} \quad OtR = 1|X) = [1 + e^{-(X'\beta+\epsilon_i)}]^{-1} \tag{1}$$

$$X'\beta = \beta_0 + \beta_1 h_i + \beta_2 PS_i + \beta_3 BR_i + \beta_4 FV_i + \beta_5 c_i \tag{2}$$

We categorize the main independent variables into three groups: preference shocks, background risks and future value of housing. For instance, family size is one of the most significant predictors in favoring home ownership for a household. We categorize that as the preference shock, which affects the relative weight between housing consumption and non-durable good consumption. Meanwhile, we include household income and the subjective income growth expectation in background risk group as an income growth indicator variable in the theoretical models. Moreover, we also include households' self-evaluated housing price change in two waves as the indicator of housing price growth in the near future. Finally, we also include country fixed effects to control for institutional and cultural differences. To sum up, the latent variable is described in Equation 2 as a function of country effects c_i and a vector of household characteristics h_i. Furthermore, PS_i is a vector of variables which indicate changes in preferences, and BR_i and FV_i are vectors of variables regarding background risk and future value respectively.

We report the results of the logistic regression in Table 6 and the average marginal effects in Table 7. Both tables report four specifications of the above model, progressing from a simpler version (Column I) where we regress the dependent variable on household characteristics to a more elaborate one (Column IV) where we fully extend the regression to cover equation (2). From a simplistic model with only household characteristics as regressors, we progress to the testing of the factors of interest. We first regress with our preference shock variables, then we add background risk, and finally, we extend our specification to check whether house price growth and volatility constitute drivers of a transition towards homeownership.

Household characteristics matter for the housing tenure transition choices. First, it is evident that the wealthier the households are, the more likely they are to transit towards home-ownership. Second, age and gender do not have any significant impact on the decision. This deviates from the empirical results found in American household surveys. For instance, Han (2010) and Davidoff (2006) both find a significant impact of age on home-ownership using US data; Davidoff (2006) also finds the female head of households are more likely to transition from renting to owning. Our results are, however, in line with the UK evidence found in Battu et al. (2008).

From Table 6 we extract that households on the 2nd tertile of each country's income distribution are 25 percent more likely to become home-owners than the ones which lie on the first tertile, whereas those in the upper tertile have an approximate 10 percent propensity towards home-ownership than the baseline level. The reason that the coefficient of the 2nd wealth tertile is larger than the 3rd one is purely descriptive, as most of the new homeowners in our sample derive from this specific tertile. A possible answer could be that affluent households have already become homeowners (Di and Liu (2007)) or even the positive correlation between job mobility and wealth as in Holmlund (1984) and Cameron and Tracy (1997).

In columns II, III and IV of Table 6, we obtain that households which got married between the first and the second wave have more chances to change their housing tenure status which is in line with Lauster and Fransson (2006). A positive employment shock, on the other hand, is not correlated with increased home-ownership probabilities. An increase in family size is also associated with a tendency towards home-ownership. An increase in the members of the household should signify a need for an increase in all consumption levels. This rise in housing consumption provides the insentive of rethinking the decision to buy a house. The marginal effects for these variables show that they are not significant at the 5 percent level. This is not something that should induce doubt for the effects of the marriage and family size shock on the dependent variable. They continue to be significant at the 10 percent level of significance, but we cannot converge on the coefficient to extract the exact effect that this shock has on the probabilities of a transition to home-ownership.

In column III, we add our indicators of background risk. The three variables that we use is income growth from the previous period, where its coefficient is almost 0 and not significant. We assume that the households might consider an income shock as transitory as they might not be able to foresee if their income will hold stable in the future. This leads us to check on the other two variables, which indicate a positive and a negative income expectation change. These self-assessed measures, which contribute to our volatility proxy, fail to produce a statistically significant result as the one that the literature suggests.

Regarding our last factor of interest, the future value or the investment motives of a transition to home-ownership, we find that when house prices portray a decreasing tendency, then home-ownership becomes more attractive. The housing sector, along with its importance as an economic factor, entails a paradoxical market behavior. From our experience, we have observed that as house prices grew in the past, investing in a housing unit becomes more attractive. According to this result, we argue that housing consumption is not responsible for bubble build-up, as new home-ownership become less attractive when house prices grow.

– Table 6 here –

From Table 7, we also observe that house price growth and volatility coefficients remain robust, whereas other variables lose their significance. Furthermore, we extract that households undervalue the utility they extract from the terminal housing value in our model, which translated into the fact that the transition to home-ownership is taking place for housing consumption motives and investment incentives. Our measure of house price volatility is also significant and negative, in line with Turner and Seo (2007). In this case, we argue that since the house owning decision is made for housing consumption purposes, it nevertheless entails some investment risk. Overall these results suggest that households are drawn into homeownership when prices are low, thus making the initial investment in housing cheaper, and also when house price variance is also on low levels, overall reducing the risk of their investment.

– Table 7 here –

As we can see from the descriptive results within age groups in Figure 2, young age cohorts are more likely to transition to home-ownership. The standard life-cycle prediction is that as households age and grow in size, the housing consumption also increases, thus we will observe a probably hump-shaped growing trend of housing consumption increase (in size). It is also likely that the middle-aged households are more likely to transition to homeownership. However, we do not observe this pattern here. A possible explanation is that some households have a very low premium in owning, thus choose to rent the housing service for their entire life cycle. Alternatively, they have already transitioned to ownership at a younger age, which is likely the case in Netherlands.

3.2 Housing Consumption: Size Adjustment

We turn to the intensive margin of the housing consumption by examining carefully the households who remain renters or owners in both waves. The primary motivation of this empirical exercise is to investigate how freely or frequently household adjust their housing consumption. Due to the moving and mortgage termination cost for the owners, the theories often assume that the housing size of home-owners remains constant or much less flexible compared with the renters, which leads to the constant housing consumption in many of the previous papers in the literature. (Grossman and Larogue, 1990; Flavin and Yamashita, 2002) Moreover, the empirically observed housing adjustment inflexibility has more significant implications for asset pricing and macroeconomics, as pointed out by Flavin and Nakagawa (2008) that a non-convex adjustment cost of housing consumption can deliver similar implications as habit persistence model without invoking unreasonably high risk aversion.

Since the European Sovereign debt crisis, the European Central Bank has maintained a relatively loose monetary policy, which leads to a steady decline of average mortgage rates in the majority of European countries (see Figure 3b). The conventional wisdom in housing tenure transition predicts that when households face a lower mortgage rate, they are more likely to transition to home-ownership. However, the results in Figure 1 seems to suggest otherwise. In the four years interval between 2010 and 2014, we do not observe a significant home-ownership increase in European countries, except for Portugal and Slovakia. To make it even more puzzling, countries like Austria, Belgium and Netherlands, we even observe a mild decline in home-ownership. Given the magnitude of mortgage reduction (from 5% annualized interest rate to around 2%) is substantial, we have to consider that some other major constraining factors prevent Europeans from owning homes.

We know that the housing decision is never a static choice that only maximizes the utility of a current period. Instead, it affects the future choices and wealth accumulation of the households. Papers like Campbell and Cocco (2003) have emphasized the importance of housing as means of saving and hedging against future housing consumption and non-durable good consumption. This requires us to look into the investment alternatives and the housing market, which has been assumed exogenous in this paper. First, for the financial market conditions, due to the lax monetary policy, the financial return on safe assets has been low since the sovereign debt crisis. The financial assets as an alternative to store value do not seem to be very attractive during the recent years. Second, the housing markets in Europe has been booming, but not without volatility. We look at the housing price index based on the ECB data warehouse series in Figure 3a.

– Figure 3a and Figure 3b here –

There is another puzzling empirical finding that households report in the survey. They remain in the same property for longer than the period between 2010 to 2014. Meanwhile, a substantial amount of households also report a different level of housing size between the two waves. To clear that concern of reporting errors, we consider all the housing size changes which are less than ten square meters as reporting mistakes. However, even after controlling for reporting errors, we still observe a significant amount of households adjusting their housing size without selling and repurchasing. It is understandable that households living in single unit detached houses can construct an additional room or re-purpose an old garage and replace it with a garden. However, for households living in apartments and attached houses, it is tough to adjust their housing by construction or lack of maintenance. We thus believe that they are engaged in the exercise of renting part of the property out or getting it back between two waves.

4 A Parsimonious Model of Dynamic Housing Decisions

We build a parsimonious partial equilibrium model based on Landvoigt (2017), Campbell and Cocco (2003, 2015), Li et al. (2016) and Corradin (2014) to illustrate how do households make housing decisions in terms of housing size, tenure choice, and mortgage decisions in a life-cycle consumption smoothing model. Our model is closely related to the stylized model in Bajari et al. (2013). However, there are a few key differences from the previous models. First, we introduce the asymmetric structure of financial asset holding – indebted households are more likely to reduce the outstanding loan. This also coincides with many recent European regulations focusing on amortization requirements, such as Netherlands where a law making mortgage payment only tax deductible when households amortize at a minimum rate is introduced. In general, households also have the incentive to reduce their indebtedness, as indebtedness has a certain negative psychological effect on household psychological well-being. (see (Brown et al., 2005; Gathergood, 2012)) Second, we adopt a more flexible utility function of constant relative risk aversion instead of a log utility function without assuming constant elasticity of substitution between housing and non-durable good consumption. Third, we emphasize the housing adjustment friction from adjustment costs and borrowing constraints, but abstain from mortgage default, home equity extraction and different mortgage contracts for the tractability and lack of corresponding empirical cases observed in the data.

4.1 Household Preferences

Consider a representative household, which derives utility from both non-durable consumption C_t and housing service H_t with a finite horizon. We assume that households live for finite T periods and make dynamic decisions to maximize the life time utility and then obtain a terminal value based on their total wealth, W_T. We do not consider mortality for the moment. When we think about households as a unit, there are usually multiple members. The possibility of all them perishing is very low. There are two ways for the households to obtain housing service: renting housing service with the market rental price and owning residential property (possibly) with collaterized mortgage loans. In the baseline model, we do not consider the rare case when households own housing in places they do not reside and rent their main residence. However, it is possible for people organizing their housing investment as such.

We denote $\tau_t = \{0, 1\}$, as the tenure choices of the household at time t, and let h_t^o and h_t^r be the housing service obtained from owning and renting respectively. The total housing service is as follows:

$$H_t = \tau_t e^{\kappa_t} h_t^o + (1 - \tau_t) h_t^r \tag{3}$$

where κ_t is the housing ownership preference. When κ_t is positive, households derive more housing service from owning than renting at the same housing size. Observing the life cycle of housing consumption, we also assume that this housing preference parameter is age dependent – $\kappa_t = f(a_t) + \varepsilon_t^\kappa$.

The household's preference is given by:

$$U(\{C_t, h_t^o, h_t^r\}_{t=0}^T) = \mathbb{E}_t \sum_{t=1}^{T} \beta^{t-1} u(C_t, H_t) + b\beta^T V(W_T), \tag{4}$$

where β is the time discount factor, and b is the parameter that governs the bequest motive. We interpret the bequest motive as the combined utility one derives from altruistic bequest motive and consumption value of the remaining wealth after the terminal period. Housing can serve as both consumption and saving thanks to its durability and resale value. Therefore the terminal wealth comprises of both cash at hand and the resale price of the housing equity the households holds at the terminal period.

4.2 Labor Income and House price

We assume that each household has a level of endowment wealth W_0 entering period $t = 1$. Such wealth is the sum of cash at hand and real estate value abstracted by total outstanding of mortgage loan. Through the life cycle, households also receive constant streams of labor income at each period, which cannot be traded nor used as collateral. The labor income grows with age and follows a life cycle that is well studied in the labor economics literature. We thus assume that the natural log of labor income, $l_t \equiv log(L_t)$, follows a random walk with a life cycle trend:

$$l_t = l_{t-1} + f(a_t) + \varepsilon_t^L \tag{5}$$

where $f(a_t)$ is the deterministic age profile of labor income, and $\varepsilon_{i,t}$ is the individual random shock with zero mean and variance $\sigma^2_{\varepsilon^L}$. For the simplicity of the baseline model, we do not consider the heterogeneity of the labor income age profile, which is dependent on time-invariant individual characteristics such as gender and education.

Similar to labor income, the natural log of house prices, $p_t^H \equiv log(P_t^H)$, follows a random walk with a common housing market drift:

$$p_t^H = p_{t-1}^H + \omega^H + \varepsilon_t^H \tag{6}$$

where ω^H is the average expected house price growth in the housing market, and ε_t^H has zero mean and variance $\sigma^2_{\varepsilon^H}$. However, house owners and potential owners, may have different expectations in house price growth, which causes heterogeneous extensive and intensive housing consumption decisions *ceteris paribus*. Moreover, we assume that $\varepsilon = (\varepsilon^L, \varepsilon^H)$ is independently distributed over time, but they may be correlated contemporaneously — $\sigma_{\varepsilon^H\varepsilon^L,t} > 0$.

We assume that rental prices are pegged with the housing value for simplicity. We follow Poterba (1984) and Díaz and Luengo-Prado (2008) to assume that the rental price is a function of the safe asset interest rate and expected housing price growth. However, we leave the maintenance cost out of the equation for the simplicity of the structural model. Therefore we have the rent-to-price ratio as follows:

$$\rho_t = \frac{P_t^r}{P_t^H} = r_t - \mathbb{E}[exp(\Delta p_t^H)] - 1 = r_t - exp(\omega^H) - 1 \tag{7}$$

where r_t is the one period risk-free interest rate. The ratio is deterministic for the baseline model, but we can introduce time-varying aspect later to make it more realistic.

5 Mortgage Contracts

We assume a tight borrowing constraint, where a household cannot obtain a loan against its human capital. Therefore, the only way to circumvent the borrowing constraint is mortgage debt with the house value as the collateral. At any period of t, one can borrow as much as:

$$X_t \geq -(1-d)P_t^H h_t^o. \tag{8}$$

where d is the minimum home equity requirement by mortgage providers or regulation at period t. The down-payment requirement is the thus the main friction for young households transitioning from renting to owning. They will have to save up to cover the down-payment and the transaction fees. Gete and Reher (2016) also point out that even without a down-payment requirement by the regulator, mortgage providers will also internalize the default risk (to a certain extent) and exercise an endogenous down-payment rate due to the costly foreclosure and process of housing assets.

However, households do not necessarily have to borrow to the limit. We model the level of mortgage debt as one of the endogenous choice by the households. We only consider the net debt position here. When $X_{i,t} < 0$, the debt is mortgage collateralized by the housing value. Thus the loan-to-value ratio (LTV) and loan-to-income ratio (LTI) are defined as follows:

$$LTV_t = \frac{|X_t|}{P_t^H h_t^o} \tag{9}$$

$$LTI_t = \frac{|X_t|}{L_t}. \tag{10}$$

We only consider adjustable rate mortgage (ARM) here for simplicity. Furthermore, we assume that households aim to repay all the mortgage principle between the end of period T. This assumption corresponds to the recent regulations in Europe that incentivizes households to amortize instead of constantly holding very high levels of debt. For instance, in the Netherlands, mortgage payment was fully tax deductible, which has led to the observation that Dutch households have much higher mortgage debt even at very senior age. Since 2016, new regulations in Netherlands aim to offset that incentive by reducing the tax deductible mortgage payment conditional on a minimum amortization rate. We therefore incorporate such regulation trend in the model. We assume that the net debt position of household cannot be decreasing and the principle pay back is at least an even installment that pays back all the debt at period T:

$$X_t \geq X_{t-1} \quad \text{when } X_{t-1} < 0 \quad \& \quad \tau_t = \tau_{t-1} \tag{11}$$

However, households have the refinancing option to renegotiate the outstanding principle with the mortgage provider. Thus the net debt position X_t is an endogenous choice by households. We do not consider mortgage default in this paper simply due to the fact that we do not observe any defaults in the HFCS panel data.

5.1 Housing Tenure Choice and Wealth Accumulation

Household wealth evolves differently depending on their tenure choice and net asset position. At each period, households receive cash at hand from labor income, investment in the previous period, and potential house sale value if she decides to sell. The wealth of households at period t is as follows:

$$W_t = L_t + X_{t-1}[1 + r_{t-1} + \lambda_{t-1}\mathbf{1}(X_{i,t-1} < 0)] + \tau_{t-1}P_t^H h_{t-1}^o, \tag{12}$$

where λ is the mortgage premium. Households then allocate the wealth among non-durable consumption, investment, housing expense and other costs. Thus we have the following budget constraint:

$$W_t = C_t + X_t + (1 - \tau_t)P_t^H \rho_t h_t^r + \tau_t P_t^H h_t^o(1 + \delta) \tag{13}$$

Recall that ρ_t is the rent to price ratio, and δ is the housing service adjustment cost. We assume that it is costly to adjust housing service, especially when owning. Consider that households often have to pay a significant percentage of total housing value for the commission fee and notary fee to complete the transaction, it is natural to assume that when households decide to own a new property, they have to face the transaction cost. For the simplicity of the model, we do not consider the moving cost of renters and owners since it can be highly heterogeneous and not necessarily proportional to the housing value. We can also afford to ignore the moving cost given that it is usually much less than the transaction costs of property purchase. Moreover, the housing service adjustment cost also captures the cost of housing service adjustment without moving. Households can have the property partly reconstructed to adjust the total housing service, such as building an extra storage room in the garden or turning the basement into an additional living room. We assume that this type of adjustment also incurs housing service adjustment cost similar to repurchasing. To summarize, households have to pay a housing service adjustment cost proportional to the housing value if one chooses to a different level of housing service.

5.2 Owners and Renters

Denote the households which do not own a house at period $t-1$ as renters. Without housing as collateral, renters cannot have any mortgage, which makes the wealth at period is easy to compute:

$$W_t^R = L_t + X_{t-1}(1 + r_{t-1}). \tag{14}$$

Therefore the budget constraint of renters becomes

$$W_t^R = \begin{cases} C_t + X_t + P_t^H h_t^r \rho_t & \text{continues as a renter } \tau_t = 0 \\ C_t + X_t + P_t^H h_t^o (1+\delta) & \text{becomes an owner } \tau_t = 1 \end{cases} \tag{15}$$

For the households entering period t owning a house make similar decisions on housing tenure and size. Their budget constraints are therefore defined as follows:

$$W_t^O = \begin{cases} C_t + X_t + P_t^H h_t^o & \text{owning without adjustment: } \tau_t = 1,\, h_t^o = h_{t-1}^o \\ C_t + X_t + P_t^H h_t^o (\delta + 1) & \text{owning with adjustment } \tau_t = 1,\, h_t^o \neq h_{t-1}^o \\ C_t + X_t + P_t^H h_t^r \rho_t & \text{moving to renting } \tau_t = 1 \end{cases} \tag{16}$$

We can therefore update the budget constraint Equation 13 as follows:

$$W_t = C_t + X_t + (1 - \tau_t) P_t^H \rho_t h_t^r + \tau_t P_t^H h_t^o [1 + \delta \mathbf{1}(h_t^o \neq h_{t-1}^o \text{ or } \tau_t > \tau_{t-1})] \tag{17}$$

In this partial equilibrium housing demand model, the state variables of the household's problem are: age (a_t), the net debt position (X_{t-1}), ownership (τ_{t-1}), labor income (L_t), house prices (P_t^H), and risk-free interest rate (r_t). The state variable vector is $S_t = \{a_t, X_{t-1}, \tau_{t-1}, L_t, P_t^H, r_t\}$. Households then make decisions at each period about: non-durable consumption (C_t), housing consumption (H_t), tenure choice (τ_t), net asset position (X_t). The decision vector is therefore $D_t = \{C_t, H_t, \tau_t, X_t\}$. The time varying parameters of the problem are: rent-to-price ratio (ρ_t), risk-free interest rate (r_t), mortgage premium (λ_{t-1}), and the minimum home equity (d_t). Denote $\theta_t = \{\rho_t, r_t, \lambda_{t-1}, d_t\}$ the vector of time-varying parameters. Finally, the time invariant parameters are $\Theta = \{\sigma_{\varepsilon^\kappa}, \omega^H, \sigma_{\varepsilon^H}, b, \beta\}$.

Given the state variables, choice space, and all the parameters, we can rewrite the household's optimization problem as the following Bellman equation:

$$V_t(S_t; \theta_t) = \max_{D_t} \{U(C_t, H_t) + \beta \mathbb{E}_t [V_{t+1}(S_{t+1}; \theta_{t+1})]\} \quad \text{for } t < T \tag{18}$$

Where $V_{t+1}(S_{t+1}; \theta_{t+1})$ is the continuation value at time T+1. For different types of households, their optimization problem are subject to different budget constraint as described in Equation 8, 11 and 17.

6 Estimation and Simulation

In this section, we describe the estimation procedure of the model. This model cannot be solved analytically. We, therefore, have to solve the dynamic optimization problem for different households using numerical solutions. One possible estimation method is to estimate the structural parameters using the simulated methods of moments similar to Landvoigt (2017) and Li et al. (2016). However, due to a large number of state variables and decision variables, it is very computationally costly to adopt the simulated methods of moments to map out the life cycle of decision trees conditional on all possible state variable realizations. We, therefore, adopt the method proposed by Bajari et al. (2007) to alleviate the computational burden. Moreover, Bajari et al. (2013) also show that the dynamic discrete and continuous choice of housing service and housing tenure fits the requirement of this relatively new method of estimating a dynamic model with higher computational efficiency.

The estimation takes two stages. In the first stage, we need to estimate the policy functions of endogenous state variables and the transition functions of the exogenous variables. Then we estimate the empirical value function by varying the realizations of state variables. In the second stage, we then apply the equilibrium condition of the optimal decision and vary the policy functions to estimate the parameter of interest by minimizing the violation of optimality in the observed sample.

6.1 Reduced Form Policy Functions of Endogenous State Variables

The first stage of Bajari et al. (2007) requires the estimation of reduced form policy functions of the decision variables. It is optimal to use non-parametric estimation to allow the maximal flexibility for the choices based on the state variables. However, due to the curse of dimensionality and lack of economic interpretation, we follow Bajari et al. (2013) and choose a semi-parametric approach to balance flexibility and economic interpretation. We assume, at time t, that the tenure choice can be formed into four categories for owners and renters, which are governed by an unobservable latent variable $y^*_{i,t} = f(S_{i,t}, Z_{i,t}) + \varepsilon_{i,t}$. $S_{i,t}$ are the state variables, and $Z_{i,t}$ are the variables that affect the housing preference parameter. Depending on the realization of the latent variable, households make ordered discrete choices as follows:

$$D^O_{i,t} = \begin{cases} \text{Transition to renting} & \tau_{i,t} = 0 \\ \text{Owning but trade down} & \tau_{i,t} = 1 \text{ and } h^o_{i,t} < h^o_{i,t-1} \\ \text{Owning the same housing size} & \tau_{i,t} = 1 \text{ and } h^o_{i,t} = h^o_{i,t-1} \\ \text{Owning but trade up} & \tau_{i,t} = 1 \text{ and } h^o_{i,t} > h^o_{i,t-1} \end{cases} \tag{19}$$

This specification resembles the real world housing tenure choice that any type of adjustment in housing size is costly, especially when owning. If housing can be smoothly adjusted without cost, the discrete choices among the owning options would make no sense. We do not further specify the precise housing size adjustment once owners decide to transition to renting due to the rare occurrence in the data and limited additional contribution to the discussion. Similarly, we can derive the ordered discrete choices for renters as follows:

$$D_{i,t}^{R} = \begin{cases} \text{Keep renting} & \tau_{i,t} = 0 \\ \text{Owning but trade down} & \tau_{i,t} = 1 \text{ and } h_{i,t}^{o} < h_{i,t-1}^{r} \\ \text{Owning the same housing size} & \tau_{i,t} = 1 \text{ and } h_{i,t}^{o} = h_{i,t-1}^{r} \\ \text{Owning but trade up} & \tau_{i,t} = 1 \text{ and } h_{i,t}^{o} > h_{i,t-1}^{r} \end{cases} \tag{20}$$

Once again, due to the adjustment cost, housing tenure choice is often lumpy, and households expect the change of future housing consumption needs. Therefore looking into the housing size transition on top of the extensive tenure choice can help us identify that aspect of housing decision making. Moreover, since we assume zero adjustment cost in renting, we ignore the housing size adjustment when renters keep renting, similar to the case of owners. However, in practice, due to a meager number of observations in renter-to-owner transition, we simplify the ordered probit model by binary probit model between owning and renting for the renter and entirely rely on the housing size choice estimation to capture the detailed transition decisions.

We estimate the reduced form policy function of the intensive adjustment margin, housing size choice, conditional on the tenure choice. Since the tenure choice has already defined the direction of adjustment for the owners, we therefore only look at the absolute value of housing size adjustment. Meanwhile, we allow full flexibility in housing size adjustment for renters.

Finally, we specify the reduced form policy function for the remaining endogenous state variables – net asset position. For the renters, it is savings accumulation, and for the owner, it is either savings or the total debt outstanding. When a housing tenure transition happens, the net asset position often adjusts by a large margin by the downpayment and the mortgage contract. In the baseline model, for the simplicity of the estimation procedure, we only consider the net asset position and assume that households can adjust their net asset position relatively freely. By doing so, we reduce the dimension of endogenous state variables and keep the model parsimonious and traceable.

We acknowledge that this is a rather strong simplifying assumption to allow flexible adjustment of net asset position. Once the household has taken a mortgage contract, it is often tough to adjust the amortization speed or a lump-sum home equity increase in one period. This is the result of a significant mortgage adjustment cost. Households do not hold both positive financial assets and mortgage debt in theoretical models due to the non-negative and significant mortgage premium. While, in reality, we do observe a large number households holding both. We, therefore, consider that households view these two as separate accounts and let them evolve relatively independent to one another. In other words, households make savings to a financial asset account and passively follow the initial structure of the mortgage contract regarding amortization and interest payment. However, when households are hit with a substantial shock on the housing value, income, liquidity, and mortgage interest rate, it is optimal to re-negotiate or default (Campbell and Cocco (2015)). We do not further complicate the model with those specific discrete choices for the moment. Moreover, we believe it is a rare circumstance especially in the European context since we observe zero mortgage defaults in more than 7000 observations. Nonetheless, those practical complications can reconcile with the simplifying assumption by viewing the non-mortgage assets being invested in a balanced portfolio that has the same rate of return as mortgage interest.

Once we have the policy functions for housing tenure choice, housing adjustment size, savings to the financial assets and mortgage account reduction, we can easily calculate the consumption using budget constraints.

6.2 Transition Functions of Exogenous State Variables

We also estimate the rules of transition for the exogenous state variables such as housing price, real interest rate, mortgage premium, and income growth path. However, due to the limited time span of the HFCS and the lack of information on actual housing prices, we turn to external macroeconomic time series for the transition functions of the exogenous state variables.

We assume that the income process follows the typical life-cycle pattern with independent and identical income shocks every period.[8] We take the country-specific labor income evolution from different sources. We do not have a long-standing panel of households to estimate the life-cycle of labor income. Instead, we use the findings from previous papers like Iacoviello and Pavan (2013) and Campbell and Cocco (2015) to calibrate the parameters of the income growth process that cannot be estimated.

We use the Eurostat times series on the country level housing price index and monthly mortgage rate index to formulate the transition functions of housing prices and mortgage premium process. We assume that all the euro-zone countries face the same real interest rate for simplicity and take the country average deposit rate time series to formulate the real interest rate process. The house price growth and real interest rate may be

[8]For the baseline model, we do not consider the persistence of labor income shocks, but we can easily extend the model to incorporate the lasting effect of labor income shocks.

correlated. We, therefore, consider the following VAR process:

$$r_t = b_{r0} + b_{r1} r_{t-1} + b_{r2}\omega_{t-1} + \varepsilon_{rt} \tag{21}$$

$$\omega_t = b_{\omega 0} + b_{\omega 1}\omega_{t-1} + b_{\omega 2} r_{t-1} + \varepsilon_{\omega t}. \tag{22}$$

The results of the vector autoregressive approach between the real interest rate and house price growth show a minimal correlation between the error terms of the two. We, therefore, move forward with independently distributed error terms for the real interest rate and house price index growth. Moreover, the country fixed effect appears to be absorbed by the first order time difference and explain the growth path of neither interest rate nor house price growth. Therefore, we consider real interest rate and house price growth as independent auto-regressive to the power of one processes.

6.3 Empirical Utility Function

Before we estimate the utility function, it is necessary to specify a functional form. The essential trade-off is to decide whether housing service is a separate consumption stream irrelevant from the non-durable consumptions. (Campbell and Cocco, 2015; Cocco, 2004; Chetty et al., 2017). A simple utility function specifications is as follows:

$$U(C_t, H_t) = \frac{(C_t^{1-\xi} H_t^{\xi})^{1-\gamma}}{1-\gamma} \tag{23}$$

where γ is the relative risk aversion of the household, and ξ is the relative weight of non-durable consumption and housing consumption. However, this Cobb-Douglas utility between the housing and non-durable consumption leads to a simplified and strong assumption that the elasticity of substitution is constant and unit. As demonstrated in Bajari et al. (2013), such simplification can lead to a substantial differences in housing adjustment timing and size. Nonetheless, we also want to have a more traceable parameter in relative risk aversion unlike the log utility function in Bajari et al. (2013). Therefore, we follow Flavin and Nakagawa (2008) and adopt a relatively more complicated utility function as follows:

$$U(C_t, H_t) = \frac{[(1-\xi)C_t^{\varphi} + \xi H_t^{\varphi}]^{\frac{1-\gamma}{\varphi}}}{1-\gamma} \tag{24}$$

where the additional parameter φ governs the elasticity of substitution between non-durable consumption and housing.

We randomly select 100 households in the sample of the first wave and forward-simulate 200 paths of exogenous state variables for 10 periods for each household. According to Bajari et al. (2007), by computing the discounted present value of all the periods of the forward simulation, we obtain a consistent estimate of the empirical value function. Note that all these forward simulations are conditional on the estimated reduced form policy functions and the parameter of interest nested within the utility function.

6.4 Estimation of the Structural Parameters

The primary interest of this paper is to measure the housing preference accurately. We thus focus on the following two parameters in the utility function: 1) the elasticity of substitution between the housing and non-durable good consumption; 2) the weighting parameter between the two consumptions. It is also beneficial to limit the dimension of parameters we structurally estimate with such a simulation-based value function. As every realization of the value function requires a substantial amount of simulation, it is very time-consuming to optimize the objective function over higher dimensions. We calibrate the rest of the parameters as 20% for the weight of housing consumption and 0.98 for the discount factor (Campbell and Cocco, 2015).

The estimation criterion of Bajari et al. (2007) is similar to the minimum distance approach in the simulated method of moments. However, instead of minimizing the distance between observed and simulated moment, this approach uses the optimality of the equilibrium choices and minimizes the occurrence of the violation of optimality. Therefore, we construct the objective function by randomizing the parameters of the reduced form policy function using a uniform distribution random number generator and varying the parameters in the 15% interval around the point estimation. We have the following estimates of the parameters of interest.

– Table 8 here –

We can see that the estimated parameters suggest that the European households have very balanced housing consumption as part of their total consumption and the elasticity of substitution between the housing and non-durable consumption is almost unit, which corresponds to Cobb-Douglas utility function. Notice that our results are significantly different from Bajari et al. (2013); Li et al. (2016), but closer to the results in Flavin and Nakagawa (2008), who employ a similar utility function setup. However, most of the results in the previous papers are estimated using United States data. It is thus not too surprising that European data shows a different housing consumption preference given that Europe has a much lower average home-ownership and a more stable rental market. The results are, however, sensitive to the discount factor and relative risk aversion parameter calibration due to the limit of the objective function of the method employed in the paper. We will explore more options to estimate the model using backward induction and simulated methods of moments, which we discuss in the technical Appendix section 1.10.

7 Counterfactual Simulations and Results

As mentioned in the introduction, we are mainly interested in the regulations that would directly affect households' tenure choice at the micro level: the loan-to-value regulation and the amortization restrictions. These are the popular housing market regulations that target the mortgage contracts between banks and households. The simple model we build in this paper has already incorporated the channels of how those two regulations can affect the households' tenure choice. We now simulate the counterfactual outcomes given different levels of regulations on both fronts to see how effective they are and whether do they cause households substantial welfare loss.

7.1 Loan-to-Value Regulations

We forward simulate the dynamic choices of the young households (age 25 and age 30) to see what are the likely outcomes of different LTV regulation policy. For instance, in the baseline estimation, we assume that the banks set the minimum downpayment at 20% on average voluntarily due to foreclosure cost (see Gete and Reher (2016)). According to BIS Financial Stability Policy Indicator, similar LTV regulations have been implemented in many countries around the world such as South Korea, Singapore, China, Hungary, Turkey, Norway, Sweden, and Canada. We look into the possible regulation change that increases the down-payment to 40% to see what is the impact on household housing tenure choice, housing size choice, and eventually welfare level.

Before we looked into the difference that the policy change brings. For the sake of clarity, it is necessary to check the simulated path of those representative young households before the regulation change. In Figure 5, we report the simulated results of the two representative households in five different countries with an average income in the respective countries. Younger households do not have any wealth, while the older ones have accumulated wealth to the average level of households in their 30s. We simulate the same type of households for 500 times and calculate the average probability of owning a house and housing size in the different stage of their life cycle. We can see that households slowly increase their probability of owning a house as they accumulate wealth. It is also evident that households want to live in larger units as they age. Note that we are simulating using the parameters estimated using the sub-sample of family size 2. The results indicate the life cycle of such households without an increase of family size. We acknowledge that it is a very restrictive limitation that we although later address. However, this shows that even without family size increase, there is also an evident lifecycle of housing tenure and size choice. Moreover, due to different income, housing market and financial market conditions, such life-cycle of housing choices are not homogeneous across western euro-zone countries. For instance, Dutch households are more likely to own a house in all stages of life compared with households in Belgium. However, we need to take those results with a grain of salt since we only two waves of panel data to identify many of the time-varying parameters. Therefore, we focus on looking at the difference regulation change brings to the households instead of the levels.

– Figure 5 here –

We now look at the changes brought about by the regulation tightening on LTV ratios from 80% to 60%. In Figure 6, we present the effect of the regulation change in housing tenure and size choices. In the upper half of the figure, we can see that the regulation makes it harder for young households to own a house in all stages of life. The difference is more significant among the young wealth-less households in Italy than the 30-year-old median wealth level households in Belgium. This is related to the housing price and income process in those countries. On the other hand, the regulation does not have much impact in Cyprus and the Netherlands. Overall, we can see that the regulation tightening can make it harder for the transition from renting to owning for young households. However, it is worth pointing out that the marginal effect on the housing market would be limited: there is only 2% to 5% difference in the probability of owning across all the countries, and the young renters are not likely to be the primary source of housing demand as discussed in the previous empirical section. One of the apparent logic of facing a tighter LTV regulation is to go for a smaller house so that the same amount of saving ensures the minimum requirement of downpayment. The lower panel of Figure 6 confirms such conjecture with a mostly positive difference in the housing size at most stages of the life cycle in most countries with Cyprus being the clear outlier. It is worth noting that as households age, they are much more likely to own a house. This makes the downward pressure of a tighter LTV regulation on housing size more evident.

– Figure 6 here –

We also look at the wealth accumulation and welfare level of households when the LTV regulation gets tighter in Figure 7. We can see that by disallowing households to invest in real estate, the tighter regulation costs households a significant opportunity cost of wealth accumulation. As indicated by Belgium and young German households, the wealth loss for those households can be as significant as 100 thousand euros in their later stages of life cycle. Finally, we look at the welfare difference in the bottom panel of Figure 7. It is fascinating that even if the LTV regulation denies the households' entry to the housing market in their early stages of life cycle, it does not necessarily hurt the welfare level largely thanks to the increased non-durable consumption in the early years, which is rational behavior according to the standard life-cycle model in household finance. Moreover, the higher risk in the housing market, especially in markets like Italy and Germany, makes the forced choice of not entering housing investment rationale and welfare improving.

– Figure 7 here –

7.2 Loan-to-Income Regulations

In addition to LTV regulation, LTI regulations are often implemented to ensure that households have sufficient liquidity for the mortgage debt and to avoid unnecessary and costly foreclosures. For instance, to secure the financial stability of households and avoid over-indebtedness, the Financial Policy Committee in the United Kingdom has implemented an LTI flow limit recommendation that restricts the number of mortgages extended at LTI ratios at or above 4.5 to 15% of a lender's new mortgage lending. The Central Bank of Ireland recently renewed such flow restriction that "20% of the value of new mortgage lending to first-time buyers and 10% of the value of new mortgage lending to second and subsequent time buyers can be above the LTI cap of 3.5, effective since January 2018". Norges Bank also temporarily implemented an LTI cap at 5 from 2015 to 2016.

We implement an LTI regulation with the cap at 4.5 on top of the existing LTV regulation at 20% downpayment requirement to investigate the same line of counterfactual results as in the previous section. We focus on the same two types of representative young households. We can see in Figure 8 that some proportion of the households will be blocked from switching to home-ownership due to the additional regulation. However, the magnitude is much smaller than the LTV regulation change. There will be almost no difference for the wealth-less young households and around 2% of rejected potential owners for average wealth households. It is expected since the younger representative households are more likely to be wealth constrained instead of liquidity constrained. We can also see that the regulation has similar results as in the tighter LTV regulation: tighter regulation forces the households to take smaller housing units to circumvent the limitation imposed by the regulation.

– Figure 8 here –

We now look at the wealth and welfare effect of the LTI regulation. In Figure 9, we can see that LTI regulation also prevents households from accumulating wealth via home-ownership. The wealth difference exhibits a hump shape along the life cycle, which means that the wealth accumulation difference stabilizes as the households age. It is understandable that household income grows with age and they are much less likely to be blocked from investing in housing by LTI regulation after middle-age. The drastic reverse wealth difference among the younger households without wealth in Cyprus is probably due to the volatile housing prices. The lower panel of Figure 9 shows the welfare difference due to the additional LTI regulation. Our results suggest that the LTI regulation might be welfare improving for the young households. The reason is similar to the previous case, LTI regulation prevents the young households from prematurely investing in housing at their young age with limited labor income and financial savings. Since the households do not have a perfect expectation of the house prices, the LTI may help the households smooth the consumption better by blocking them from taking on housing price risk too early.

– Figure 9 here –

8 Conclusion

In this paper, we investigate the home-ownership transition for households in 6 Countries in the EU, right after the financial crisis of 2007. Through a similar conceptual, theoretical framework with the one of Campbell and Cocco (2003) we identify three different factors which might have a potential impact on the home-ownership decision, namely a preference shock, background risk, and the future expectation. We then, empirically test these factors in a more organized econometric setting that tests the previous findings of the literature on the transition to home-ownership. Moreover, we build a parsimonious partial equilibrium model on housing demand to structurally estimate the housing preference of the European households. We find that European households have significantly different housing preferences compared with American ones. Finally, we investigate the possible change towards a tighter LTV and LTI regulation and find that it does have a significant effect on households' choice of housing tenure and size choice. It limits the entry to the housing market and slows the wealth accumulation. However, the welfare level computed using the empirical utility estimated from the structural model suggests that such regulation tightening may be welfare improving. However, our results heavily rely on the identification power of an extremely short panel data. We do not make any strong normative policy suggestion. Instead, we show in this paper that given ample survey panel data, we can credibly estimate the housing tenure and size choice and then evaluate policy change at a micro level using simple partial equilibrium models.

Tables

Table 1: Sample summary statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Renter-to-Owner	0.088	0.283	0	1	1775
Gross income	39543.3	49397.7	0	740400	1761
Net wealth	90093.2	306315.1	-452500	4957000	1762
Household Demographics					
Male	0.514	0.5	0	1	1775
Age	55.7	15.7	22	85	1643
Tertiary Educ.	0.246	0.431	0	1	1775
Working	0.457	0.498	0	1	1775
Married	0.491	0.5	0	1	1775
Preference Shocks					
Employment Shock	0.038	0.0192	0	1	1039
Marriage Shock	0.067	0.250	0	1	968
Family Size Increase	0.069	0.254	0	1	1775
Background Risks					
Income Growth	1.0483	1.278	-66.0	148.53	1539
Income Expectations	1.278	1.244	0	1	1775
Housing Market					
House Price Growth 1st Wave	1.838	2.300	0.2	7.225	4970
House Price Growth 2nd Wave	-0.493	2.848	-3.475	3.3	7449
House Price Volatility 1st Wave	31.560	41.693	2.82	125.0358	4970
House Price Volatility 2nd Wave	4.872	4.032	0.0533	12.409	7449

This table reports the summary statistics of our sample. The gross income and net wealth are aggregated at the household level instead of individuals. The household contols, preference shocks and background risk variables are the personal characteristics of the reported head of the household. The preference shocks report the status change between the two waves of the survey. On the bottom part of the table, we report the moving average of the housing market history in the past 10 years.

Table 2: Homeownership rates in the HFCS

Country	2010 (%)	2014 (%)	Δ
Austria	46.4	41.7	-4.7%
Belgium	72.7	70.8	-1.9%
Cyprus	79.1	76.9	-2.2%
Germany	54.0	56.1	+2.1%
Spain	84.9	84.0	-0.9%
Finland	77.0	75.9	-1.1%
France	64.0	66.6	+2.6%
Greece	65.3	67.1	+1.8%
Italy	68.7	69.7	+1.0%
Luxembourg	69.3	71.8	+2.5%
Malta	43.5	76.8	+33.3%
Netherlands	74.1	70.8	-3.3%
Portugal	67.4	78.9	+11.5%
Slovenia	75.8	71.2	-4.6%
Slovakia	73.4	82.9	+9.5%
EU(Average)	69.4	70.9	+1.5%

This table reports the percentage of homeowners in all countries surveyed in the HFCS in two waves and the change of ownership percentage in four years.

Table 3: Housing tenure transition matrix

Tenure	**Renters (2nd)**	**Owners (2nd)**	**Total (2nd)**
Renters (1st)	1.619 91.21%	156 8.79%	1775 100.00%
Owners (1st)	97 1.71%	5577 98.29%	5674 100.00%
Total (1st)	1716 23.04%	5733 76.96%	7449 100.00%

This table details the housing tenure transition from the first wave to the second wave of the survey. The first wave housing tenure status is reported in the column, and the second wave is reported in the row.

Table 4: Characteristics of movers and stayers

	2010		2014		Owner-to-Owner		Renter-to-Renter	
	Movers	Stayers	Movers	Stayers	Movers	Stayers	Movers	Stayers
Fraction of households								
aged ≤ 35	-	-	5.9%	3.5%	65.2%	34.8%	73.2%	26.8%
aged > 35	-	-	94.1%	96.5%	60.1%	39.9%	58.1%	41.9%
Median income (EU thousands)								
aged ≤ 35	36.1	42.0	40.1	39.1	43.7	50.0	32.7	31.2
aged > 35	39.5	38.0	35.1	42.6	38.1	46.2	26.1	30.9
Median wealth (EU thousands)								
aged ≤ 35	62.0	147.7	92.5	68.1	191.0	214.0	17.3	9.2
aged > 35	186.0	252.5	233.4	240.9	288.2	291.2	18.6	26.4

This table reports summary statistics for stayer and mover households in the HFCS panel component for the 1st and 2nd wave of the data. The table has two age bins for household heads; aged 35 and younger, older than 35 years as in Landvoigt (2015). For both income and wealth statistics we have dropped outliers below the 5th and above the 95th percentile. Total income and total household wealth are reported.

Table 5: Housing consumption shifts across housing tenure groups

	Full Sample			Own One Property Only		
	Trade-Down	No Diff	Trade-Up	Trade-Down	No Diff	Trade-Up
Owners	26.21%	39.70%	34.09%	25.48%	42.23%	32.29%
Renters	25.39%	40.27%	34.34%	-	-	-
Renters-to-Owners	16.67%	12.82%	70.51%	-	-	-
Owners-to-Renters	55.67%	12.37%	31.96%	52.63%	13.16%	34.21%

This table reports the percentage of households who adjust their housing consumption in term of residence size with respect to their housing tenure status in two waves of the panel data between 2010 and 2014. The left part titled "full sample" reports all observations, while the right panel reports the owners in the first wave with only one property.

Table 6: The determinants of housing tenure transition

Household Characteristics	Column I	Column II	Column III	Column IV
Income (2nd Tertile)	0.169	0.015	0.569	0.563
	(0.699)	(0.974)	(0.203)	(0.214)
Income (3rd Tertile)	0.857 *	0.732	1.566***	1.480***
	(0.094)	(0.188)	(0.003)	(0.005)
Wealth (2nd Tertile)	2.984***	3.102***	2.275***	2.605***
	(0.000)	(0.000)	(0.000)	(0.000)
Wealth (3rd Tertile)	1.941***	1.988***	1.408**	1.714**
	(0.000)	(0.000)	(0.009)	(0.004)
Preference Shock				
Marriage		1.287**	1.386***	1.466***
		(0.023)	(0.010)	(0.009)
Employment		-0.400	-0.421	-0.352
		(0.567)	(0.495)	(0.581)
Family Size Growth		0.975 *	0.884	0.947 *
		(0.087)	(0.106)	(0.087)
Wealth/Gift Transfer		1.442	1.555**	1.679**
		(0.054)	(0.037)	(0.034)
Background Risk				
Net Wealth Growth			0.005	0.003
			(0.313)	(0.498)
Income Growth			-0.377	-0.266
			(0.363)	(0.528)
Positive Income Expectations			-0.106	-0.062
			(0.849)	(0.914)
Investment Motives				
House Price Growth				-0.436***
				(0.007)
House Price Volatility				-0.184 *
				(0.072)
Demographic Controls	Yes	Yes	Yes	Yes
Country Effects	Yes	Yes	No	No
Observations	1430	1430	1430	1430
F	5.985	6.723	5.997	7.478
Prob>F	0	0	0	0

This table reports the results of a logistic regression where the dependent variable is housing tenure transition from renter to home-owner between the two waves of the data. The investment motive variables are country specific and taken from the "Eurostat Database".
Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: The determinants of housing tenure transition: Marginal effects

Household Characteristics	Column I	Column II	Column III	Column IV
Income (2nd Tertile)	0.007	0.000	0.022	0.022
	(0.696)	(0.974)	(0.192)	(0.205)
Income (3rd Tertile)	0.049	0.040	0.087***	0.078**
	(0.111)	(0.207)	(0.010)	(0.013)
Wealth (2nd Tertile)	0.252***	0.248***	0.154***	0.177***
	(0.000)	(0.000)	(0.000)	(0.000)
Wealth (3rd Tertile)	0.111**	0.107**	0.069	0.086**
	(0.021)	(0.017)	(0.057)	(0.041)
Preference Shock				
Marriage		0.082	0.088**	0.092**
		(0.074)	(0.047)	(0.043)
Employment		-0.017	-0.017	-0.014
		(0.530)	(0.455)	(0.552)
Family Size Growth		0.057	0.049	0.052
		(0.160)	(0.177)	(0.153)
Wealth/Gift Transfer		0.097	0.105	0.113
		(0.157)	(0.137)	(0.133)
Background Risk				
Net Wealth Growth			0.000	0.000
			(0.315)	(0.500)
Income Growth			-0.017	-0.012
			(0.361)	(0.527)
Positive Income Expectations			-0.010	-0.008
			(0.705)	(0.771)
Investment Motives				
House Price Growth				-0.019***
				(0.006)
House Price Volatility				-0.008 *
				(0.071)
Demographic Controls	Yes	Yes	Yes	Yes
Country Effects	Yes	Yes	No	No
Observations	1430	1430	1430	1430
F	5.985	6.723	5.997	7.478
Prob>F	0.000	0.000	0.000	0.000

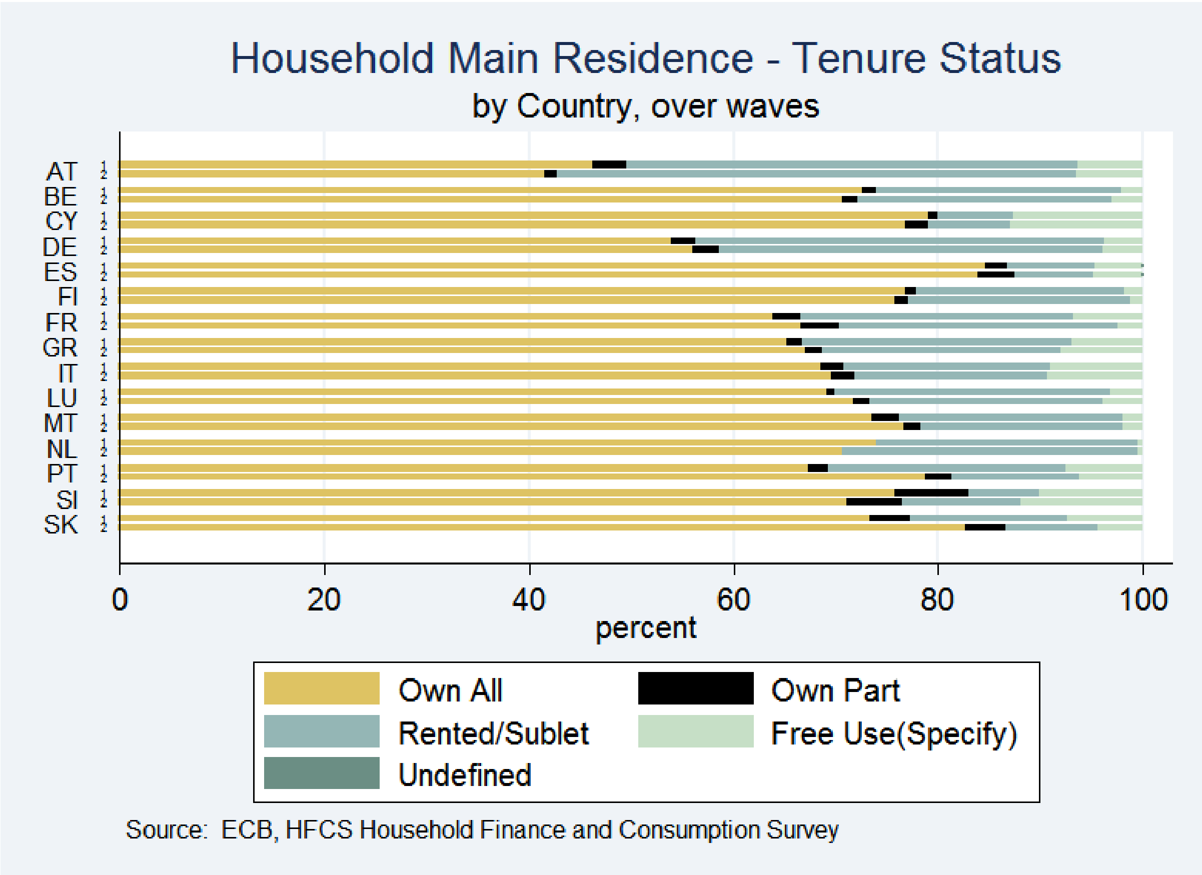
This table reports the marginal effects of Table 6. The dependent variable is the housing tenure transition from renter to home-owner between the two waves of the data. The investment motive variables are country specific and taken from the "Eurostat Database".
Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Estimated parameters of interest

Parameter	Value	s.d.
Elasticity of Substitution: φ	-0.0639	(0.0126)
Housing consumption share: ξ	0.4888	(0.0018)
Relative risk aversion: ρ	4.0	calibrated
Bequest motive: b	3.0	calibrated
Discount rate: β	0.97	calibrated

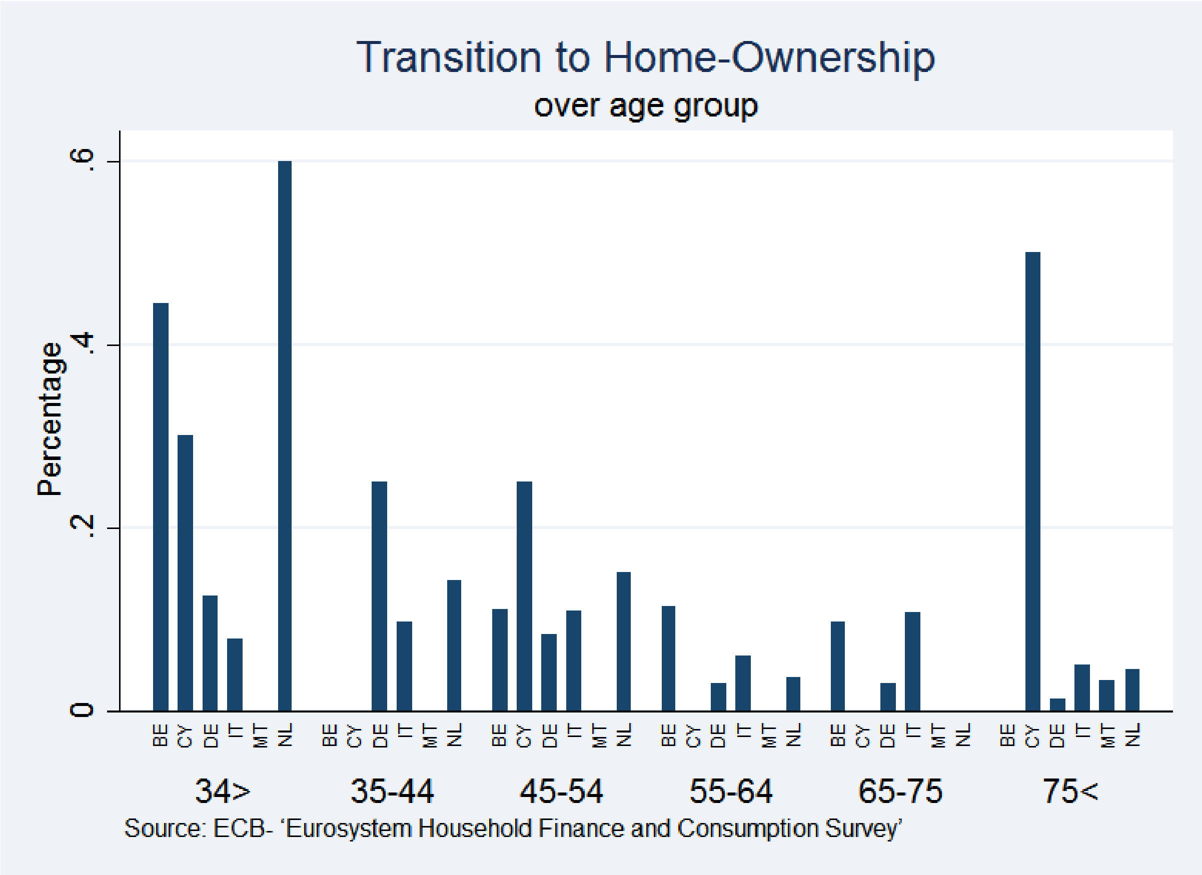
The standard errors are computed via bootstraping 49 times

Figure 1: Housing tenure status over Country and survey cross-section



We present the housing tenure profile of all the participating countries of HFCS in Europe. For each country, we show the profile of the average tenure choice at both waves of the survey. Tenure choices are categorized in the following five types: complete ownership (own all), partial ownership (own part), renting (rented/sublet), Free use and undefined

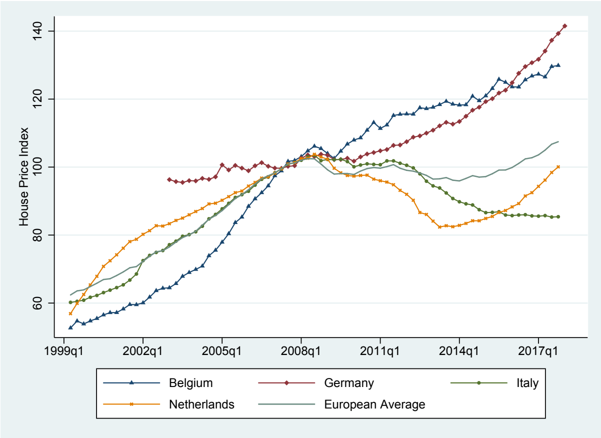
Figure 2: Percentage transtition to homeownership over Country and age group



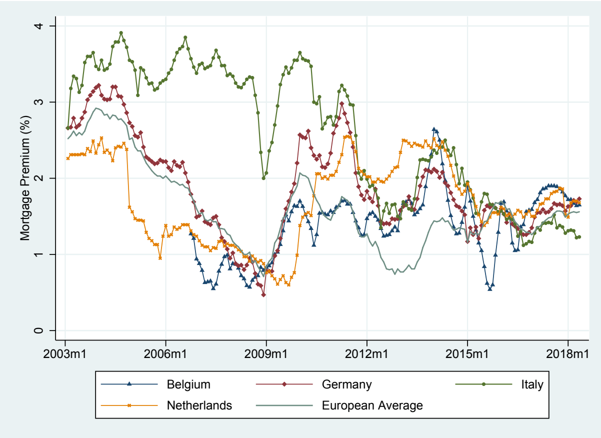
This figure plots the share of tenure transition with respect to different age groups and in different countries.

Figure 3: Housing price index and mortgage rates

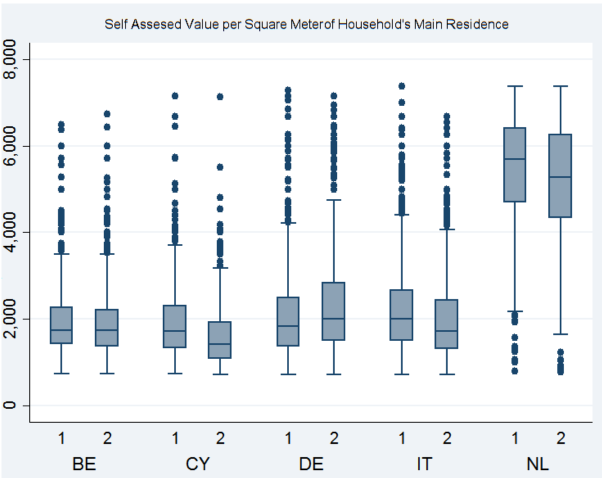
(a) Housing price index in selected countries



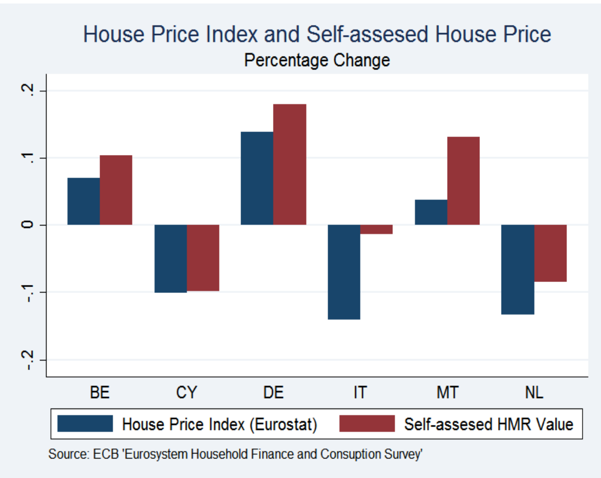
(b) Mortgage premium in selected countries



(c) Self-evaluated prices

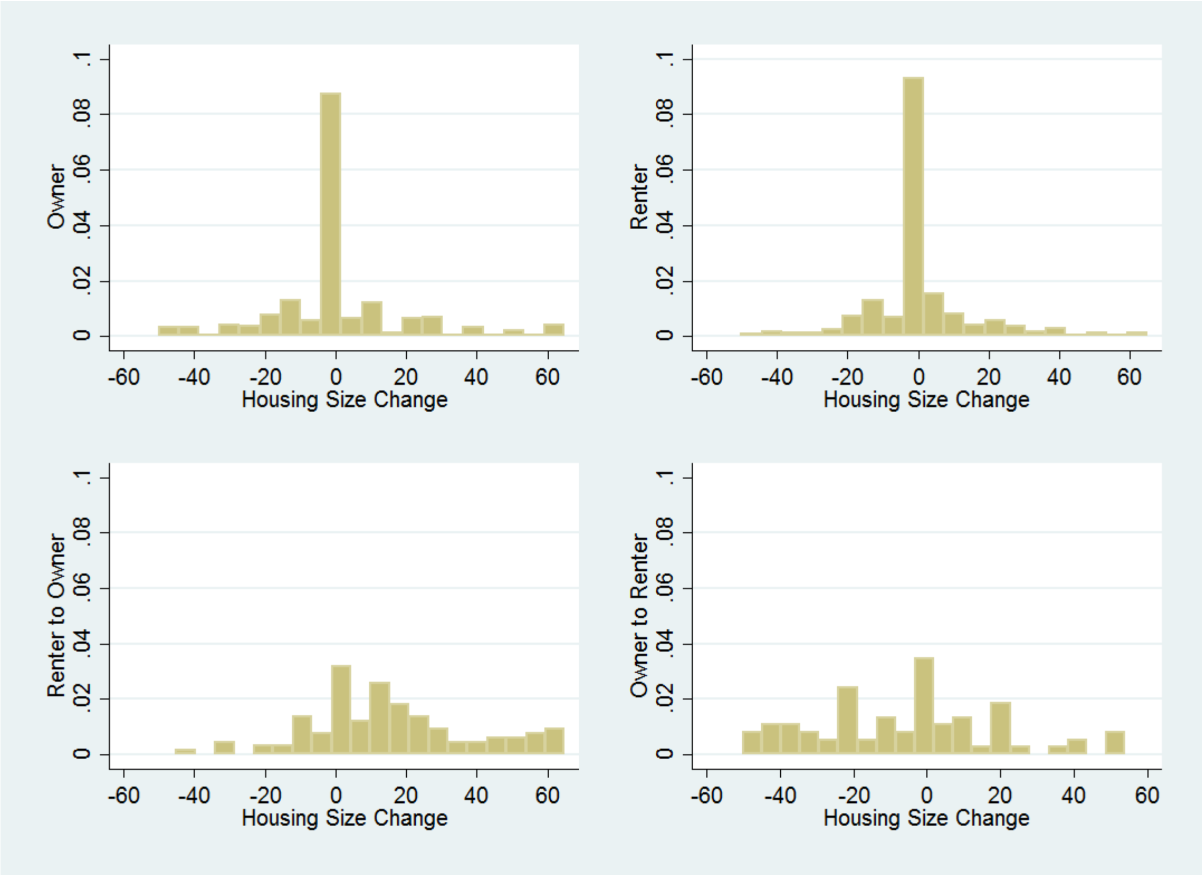


(d) House price index v.s. self-evaluated price



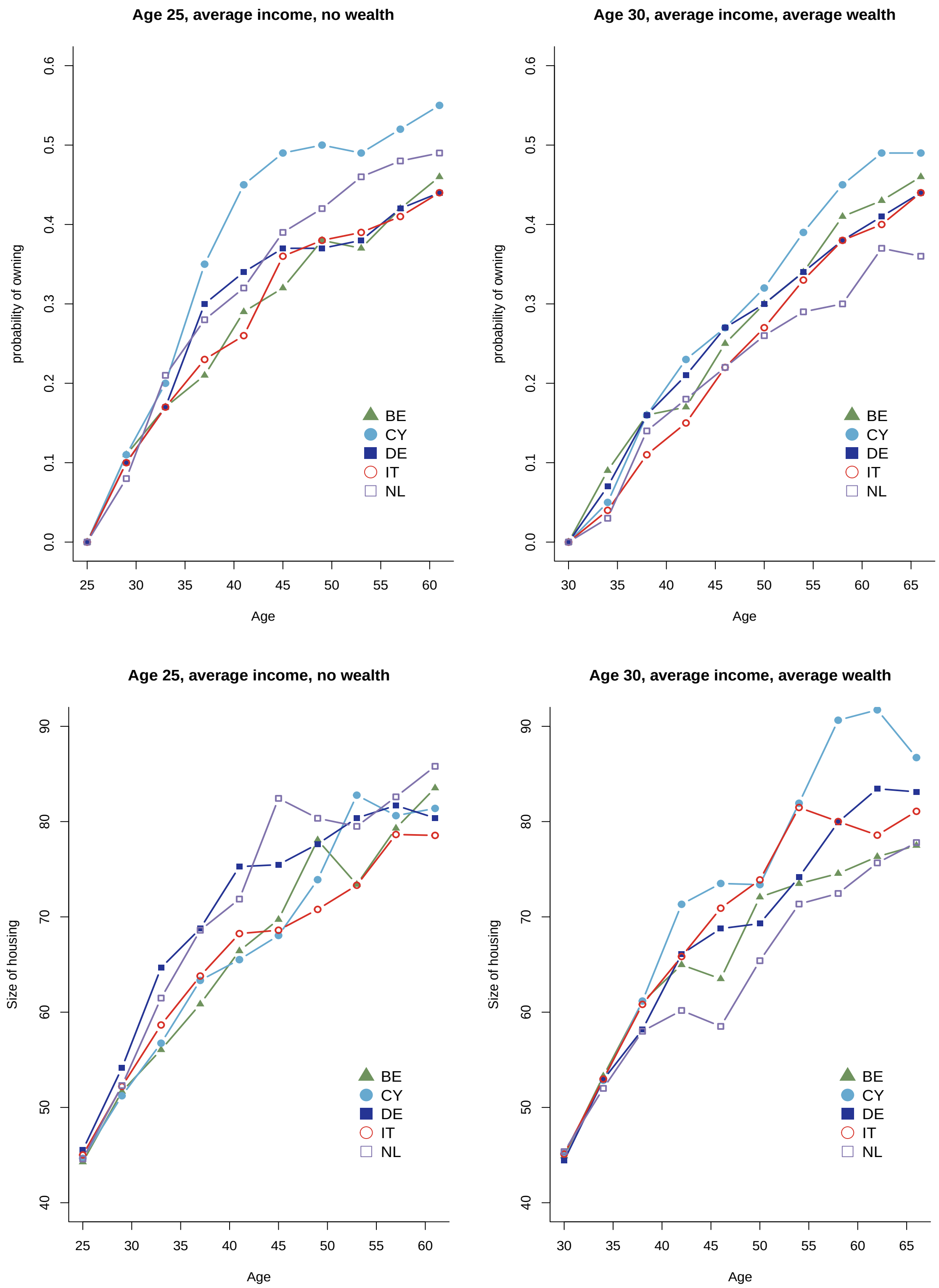
We present the macroeconomic and housing market conditions in this figure. Panel (a) plots the housing price index in recent history of the selected countries according to the Eurostat database; panel (b) plots the average mortgage premium in the selected countries at a monthly frequency according to the Eurostat database; panel (c) depicts the price difference of the average self-reported housing prices in the selected countries in the two waves of HFCS; panel (d) shows the difference between the average housing index price and the self-reported housing price.

Figure 4: Housing size change



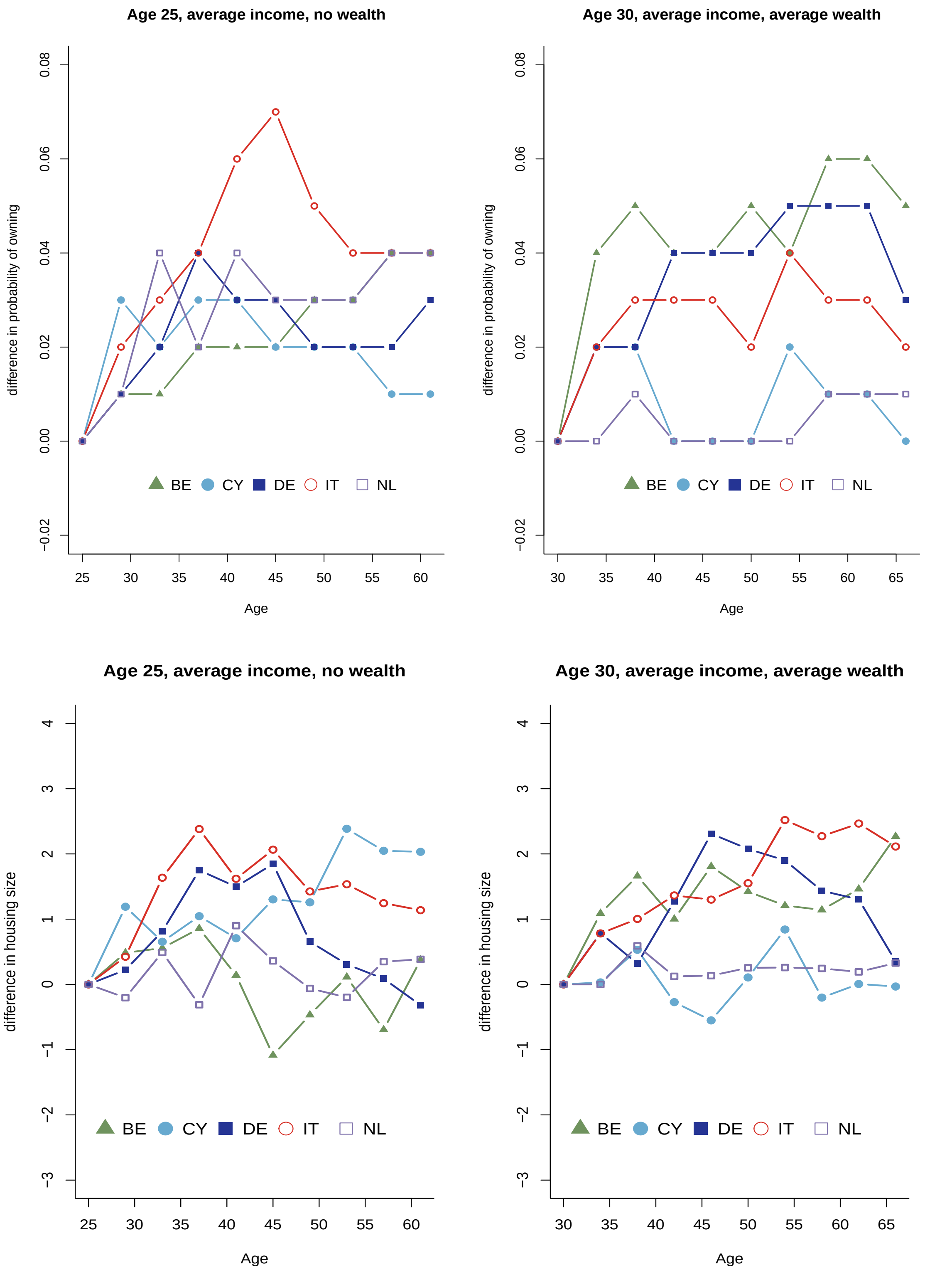
This figure plots the density of housing size adjustment with respect to four housing tenure groups: owner at both waves (top left), renter at both waves (top right), renter to owner transition (bottom left), and owner to renter transition (bottom right); source: ECB – "Eurosystem Household Finance and Consumption Survey"

Figure 5: Simulated housing choice: Ownership (up) and size (down)



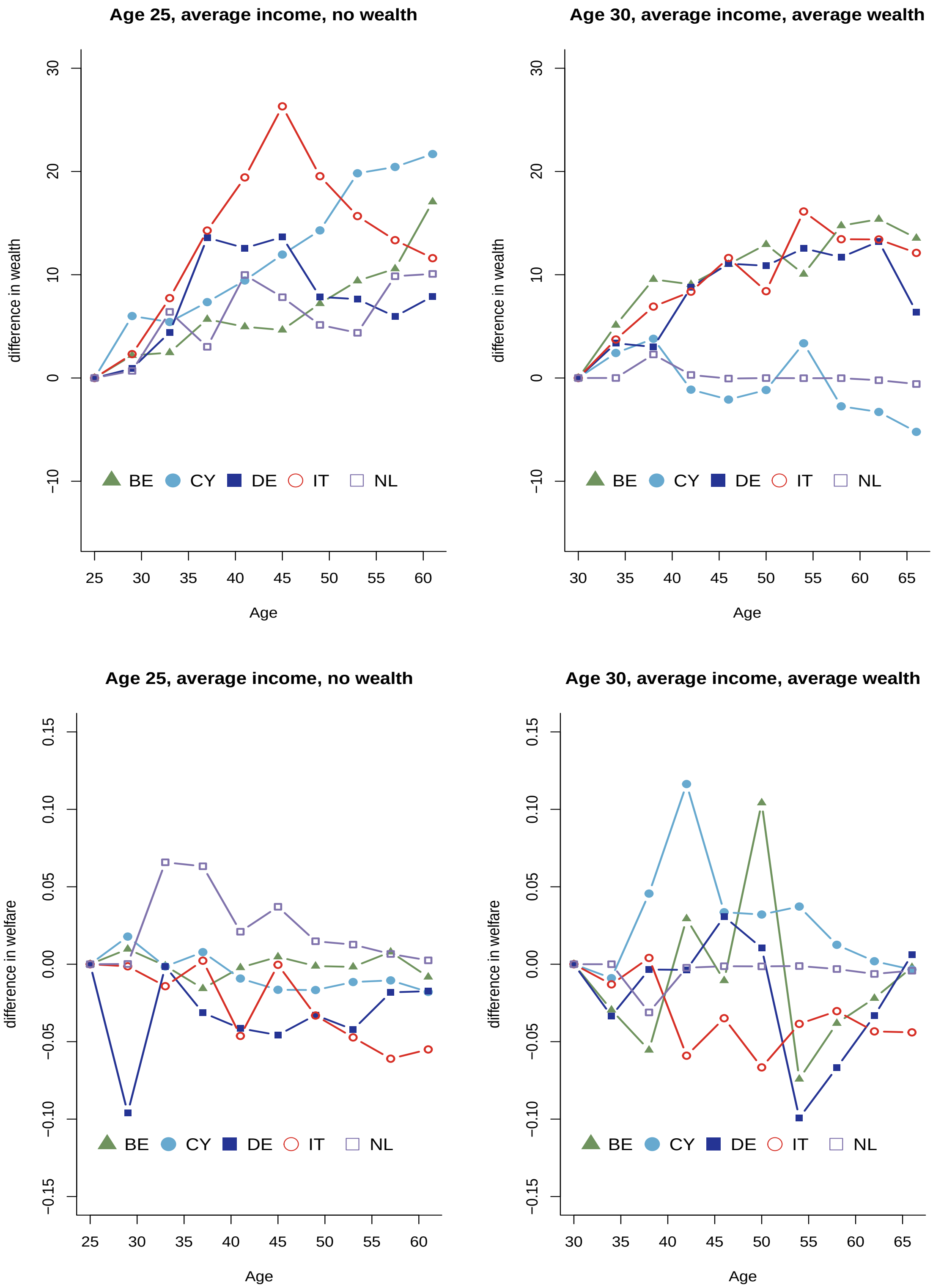
This figure plots the simulated housing tenure (upper panel) and size choices (lower panel) of two types of representative young households: a 25-year-old average income household without wealth (lefthand side) and a 30-year-old average income household with average wealth (righthand side). We plot the country specific life cycle of housing choices with regard to different levels in income growth, financial market conditions and housing market evolution. We present the results in five countries: Belgium(BE), Cyprus(CY), Germany(DE), Italy(IT), and the Netherlands(NL).

Figure 6: Effect of LTV regulation – 80% to 60%: Ownership (up) and size (down)



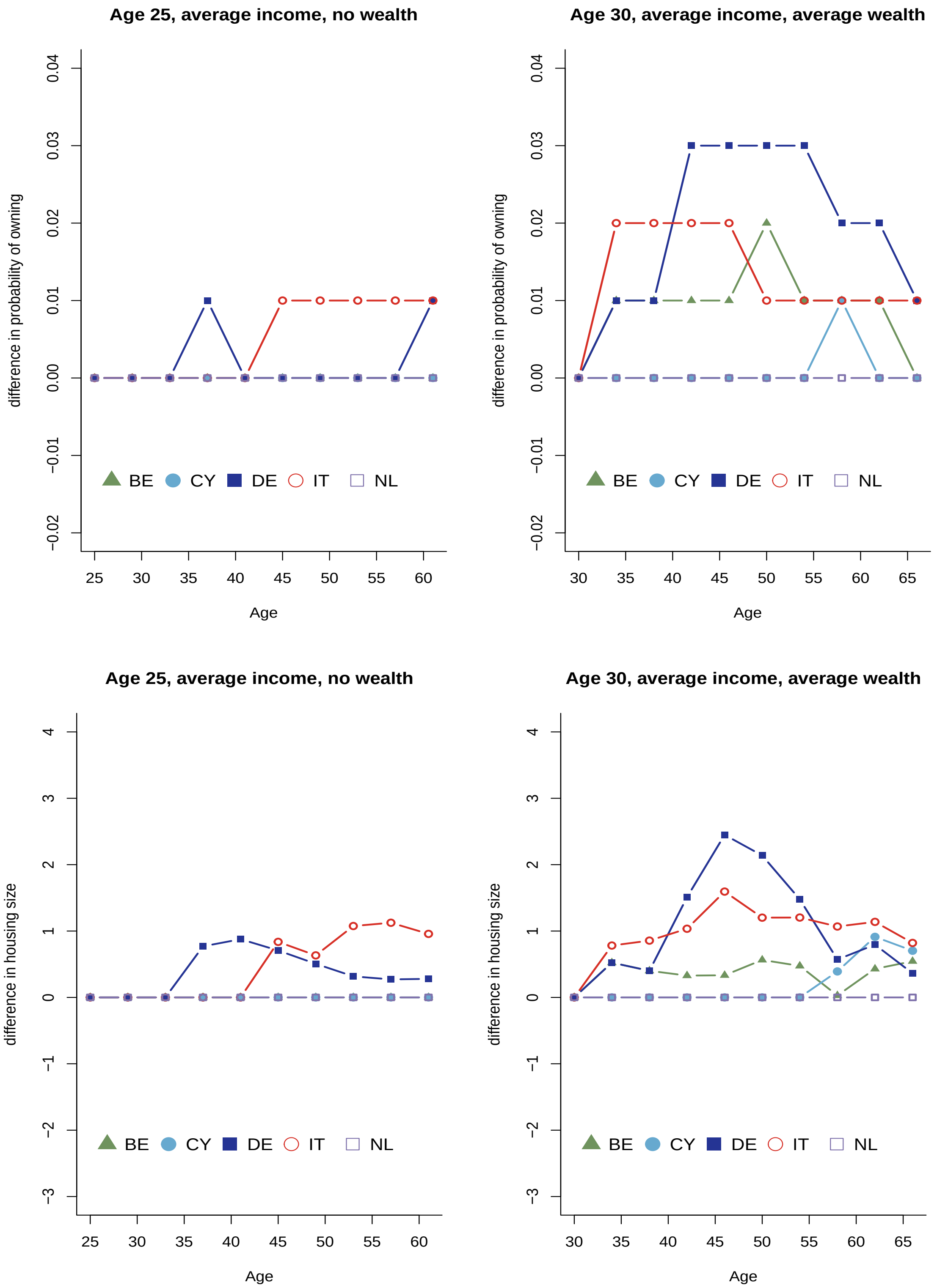
This figure plots the simulated effect of LTV regulation (80% to 60%) on housing tenure (upper panel) and size choices (lower panel) of two types of representative young households: a 25-year-old average income household without wealth (lefthand side) and a 30-year-old average income household with average wealth (righthand side). We plot the country specific life cycle of housing choices with regard to different income growth, financial market conditions and housing market evolution. We present the results in five countries: Belgium(BE), Cyprus(CY), Germany(DE), Italy(IT), and the Netherlands(NL).

Figure 7: Effect of LTV Regulation – 80% to 60%: Wealth (up) and welfare (down)



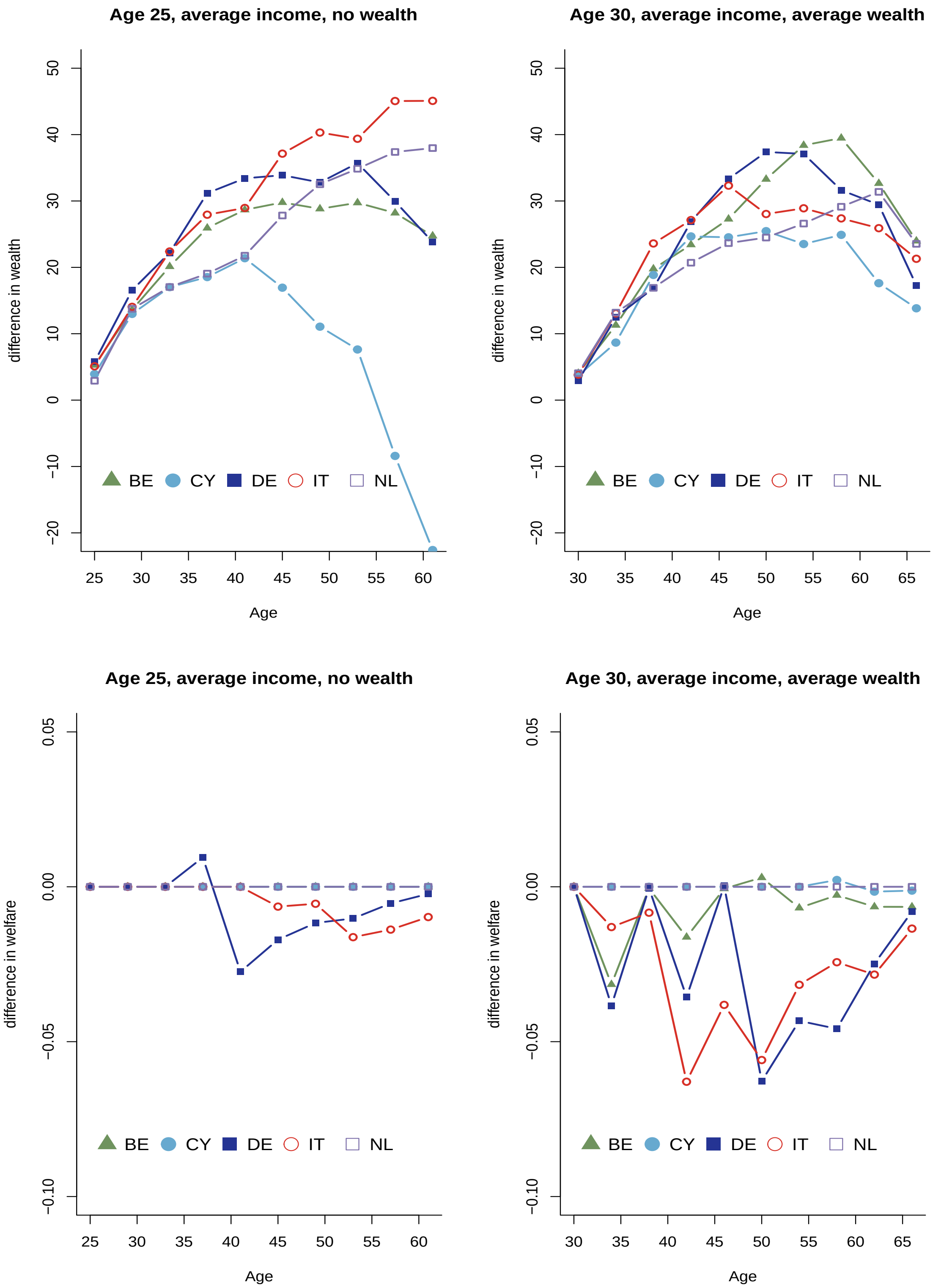
This figure plots the simulated effect of LTV regulation (80% to 60%) on wealth accumulation (upper panel) and welfare (lower panel) of two types of representative young households: a 25-year-old average income household without wealth (lefthand side) and a 30-year-old average income household with average wealth (righthand side). We plot the country specific life cycle of housing choice swith regard to different income growth, financial market conditions and housing market evolution. We present the results in five countries: Belgium(BE), Cyprus(CY), Germany(DE), Italy(IT), and the Netherlands(NL).

Figure 8: Effect of LTI regulation – 4.5 cap: Ownership (up) and size (down)



This figure plots the simulated effect of LTI regulation (4.5 to annual income) on housing tenure (upper panel) and housing size (lower panel) of two types of representative young households: a 25-year-old average income household without wealth (lefthand side) and a 30-year-old average income household with average wealth (righthand side). We plot the country specific life cycle of housing choices with regard to different income growth, financial market conditions and housing market evolution. We present the results in five countries: Belgium(BE), Cyprus(CY), Germany(DE), Italy(IT), and the Netherlands(NL).

Figure 9: Effect of LTI regulation – 4.5 cap: Wealth (up) and welfare (down)



This figure plots the simulated effect of LTI regulation (4.5 to annual income) on wealth accumulation (upper panel) and welfare (lower panel) of two types of representative young households: a 25-year-old average income household without wealth (lefthand side) and a 30-year-old average income household with average wealth (righthand side). We plot the country specific life cycle of housing choices with regard to different income growth, financial market conditions and housing market evolution. We present the results in five countries: Belgium(BE), Cyprus(CY), Germany(DE), Italy(IT), and the Netherlands(NL).

Appendix

An Alternative Numerical Solution of the Model

We can also use an alternative numerical solution that follows the classic dynamic programming approach. The model proposes a finite horizon model with mixed endogenous variables: tenure choice, housing consumption, and net debt position, among which the first one is discrete, and the other two are continuous. We use the Tauchen (1986) to discretize the continuous endogenous variables. We construct a fine grid for both housing size H_t and outstanding net debt position X_t to run the backward induction. We construct the grid range broad enough to ensure that all the results of the policy function lie within the grid range.

For the stochastic process of the house price and labor income growth, we use Gaussian quadrature numerical integration method to construct the expected continuation value EV. For the baseline model, we use a two-dimensional quadrature with seven nodes for each dimension, and we assume zero covariance between the house price shock and labor income shock. However, we can introduce covariance to the process if it is later proven necessary.

It is worth noting that with mixed type endogenous variables, using multivariate interpolation to update the value function through the iteration is computationally complicated and burdensome. However, if we use the exact grid point to grid point projection, difficulties arise for the points that do not fall on grid points. Therefore, we circumvent this issue by constructing the wealth at the beginning of period as the single endogenous variable that dictates the optimal continuation value. We then use the univariate interpolation to approximate the value function of wealth w_t. For the baseline model, we use a spline interpolation with 30 nodes and the order of three. According to John Rust 2006, this is sufficient to capture most of the curvature of the function and deliver global maximum.

The exogenous state variables evolution in the model, the labor income, moving shock and house prices, are computed using the real world data we have obtained from the HFCS. Labor income is age and household characteristic dependent. We, therefore, run a simple OLS regression of total non-financial income of the households on their age cohort and personal characteristics. Then we use the regression results to compute the deterministic part of the permanent labor income part of the households at a different period. For instance, at period t= 20, the household's age is 40 and has 40 periods left until the final period. We thus have the labor income equal to the average labor income at the age of 40: $L_20 = L(40) + shock$. For the house price, we look at the average housing purchase price per square meter at 2010 in Europe as our benchmark price. We ignore the granular difference between rural and urban housing, location differences, the construction quality and the garden space. We only use the price per square meter as the indication of the housing price. We finally look at the households who move between 4 years to construct the likelihood of receiving a moving shock. However, it is more complicated because households also voluntarily move without receiving the shock. We assume that among all the households who move, half of them receive moving shock. We would like to construct a more accurate measure of the moving shock given more information.

Once we obtain the numerical solution of the model, we move on to simulate the moments of interests, i.e., tenure choice transition, average housing consumption, the average debt outstanding, the loan to value ratio, the loan to income ratio and so on. We take the wave of 2010 as the given state variable and then simulate the stochastic shocks, combined with the observed decision choices, we can predict the state variable for the next period, and their choices as well. We then simulate for four periods to obtain the simulated moments at the year 2014. Given the parameter of interest in the numerical solution as an input, we can construct the simulated methods of moments as $m(\theta, \theta_K)$. We then construct the corresponding moments in the data $\hat{m}$. The minimum distance estimator of $min(m(\theta) - (\hat{m}))$ will give us the estimation of the parameters of interest.

References

Bajari, P., C. L. Benkard, and J. Levin (2007). Estimating dynamic models of imperfect competition. *Econometrica 75*(5), 1331–1370.

Bajari, P., P. Chan, D. Krueger, and D. Miller (2013). A dynamic model of housing demand: Estimation and policy implications. *International Economic Review 54*(2), 409–442.

Battu, H., A. Ma, and E. Phimister (2008). Housing tenure, job mobility and unemployment in the UK. *The Economic Journal 118*(527), 311–328.

Blickle, K. and M. Brown (2016). Borrowing constraints and home ownership.

Brown, S., K. Taylor, and S. W. Price (2005). Debt and distress: Evaluating the psychological cost of credit. *Journal of Economic Psychology 26*(5), 642–663.

Cameron, S. and J. Tracy (1997). The transition to homeownership: The importance of early career concerns. *General Information 84*(2), 151–163.

Campbell, J. Y. and J. F. Cocco (2003). Household risk management and optimal mortgage choice. *The Quarterly Journal of Economics 118*(4), 1449–1494.

Campbell, J. Y. and J. F. Cocco (2015). A model of mortgage default. *The Journal of Finance 70*(4), 1495–1554.

Chambers, M. S., C. Garriga, and D. Schlagenhauf (2009). The loan structure and housing tenure decisions in an equilibrium model of mortgage choice. *Review of Economic Dynamics 12*(3), 444–468.

Chetty, R., L. Sándor, and A. Szeidl (2017). The effect of housing on portfolio choice. *The Journal of Finance 72*(3), 1171–1212.

Clapham, D., P. Mackie, S. Orford, K. Buckley, and I. Thomas (2012). Housing options and solutions for young people in 2020. *population 16*, 19.

Cocco, J. F. (2004). Portfolio choice in the presence of housing. *The Review of Financial Studies 18*(2), 535–567.

Corradin, S. (2014). Household leverage. *Journal of Money, Credit and Banking 46*(4), 567–613.

Davidoff, T. (2006). Labor income, housing prices, and homeownership. *Journal of urban Economics 59*(2), 209–235.

Di, Z. X. and X. Liu (2007). The importance of wealth and income in the transition to homeownership. *Cityscape*, 137–151.

Díaz, A. and M. J. Luengo-Prado (2008). On the user cost and homeownership. *Review of Economic Dynamics 11*(3), 584–613.

Flavin, M. and S. Nakagawa (2008). A model of housing in the presence of adjustment costs: A structural interpretation of habit persistence. *American Economic Review 98*(1), 474–95.

Flavin, M. and T. Yamashita (2002). Owner-occupied housing and the composition of the household portfolio. *The American Economic Review 92*(1), 345–362.

Fuster, A. and B. Zafar (2016). To buy or not to buy: Consumer constraints in the housing market. *The American Economic Review 106*(5), 636–640.

Gathergood, J. (2012). Debt and depression: causal links and social norm effects. *The Economic Journal 122*(563), 1094–1114.

Gete, P. and M. Reher (2016). Two extensive margins of credit and loan-to-value policies. *Journal of Money, Credit and Banking 48*(7), 1397–1438.

Grossman, S. and G. Larogue (1990). Asset pricing and optimal portfolio choice in the presence of illiquid durable consumption goods. *Econometrica 58*(1), 25–31.

Han, L. (2010). The effects of price risk on housing demand: Empirical evidence from US markets. *The Review of Financial Studies 23*(11), 3889–3928.

Henderson, J. V., Y. M. Ioannides, et al. (1983). A model of housing tenure choice. *American Economic Review 73*(1), 98–113.

Holmlund, B. (1984). Income prospects and job mobility: the case of sweden. *European Economic Review 24*(3), 383–400.

Iacoviello, M. and M. Pavan (2013). Housing and debt over the life cycle and over the business cycle. *Journal of Monetary Economics 60*(2), 221–238.

Landvoigt, T. (2017). Housing demand during the boom: The role of expectations and credit constraints. *The Review of Financial Studies 30*(6), 1865–1902.

Lauster, N. T. and U. Fransson (2006). Of marriages and mortgages: The second demographic transition and the relationship between marriage and homeownership in sweden. *Housing Studies 21*(6), 909–927.

Li, W., H. Liu, F. Yang, and R. Yao (2016). Housing over time and over the life cycle: A structural estimation. *International Economic Review 57*(4), 1237–1260.

Mavropoulos, A. and Q. Xiong (2018). Housing consumption and macroprudential policies in Europe: An ex ante evaluation. *IWH Discussion Papers 17/2018*.

Mitra, M. S. et al. (2015). *LTV and DTI limits - Going granular*. Number 15-154. International Monetary Fund.

Poterba, J. M. (1984). Tax subsidies to owner-occupied housing: An asset-market approach. *The quarterly journal of economics 99*(4), 729–752.

Tauchen, G. (1986). Finite state markov-chain approximations to univariate and vector autoregressions. *Economics letters 20*(2), 177–181.

Turner, T. M. and D. Seo (2007). Investment risk and the transition into homeownership. *Journal of Regional Science 47*(2), 229–253.

Real estate transaction taxes and credit supply*†‡

Michael Koetter § Philipp Marek ¶ Antonios Mavropoulos ‖

Abstract

We exploit staggered real estate transaction tax (RETT) hikes across German states to identify the effect of house price changes on mortgage credit supply. Based on approximately 33 million real estate online listings, we construct a hedonic house price index between 2008:q1 and 2017:q4, which we instrument with state-specific RETT changes to isolate the effect on mortgage credit supply by all local German banks. First, a RETT hike by 1% reduces HPI growth by 1.2%. This effect is driven by listings in rural regions. Second, a contraction of HPI by 1% induces a 1.4% decline in mortgage lending. This transmission of fiscal policy to mortgage credit supply is effective across almost the entire bank capitalization distribution.

Keywords: Fiscal Shocks, Real Estate Markets, Mortgage Lending, Financial Stability
JEL Classifications: H30; R00; R31

*We thank Peter Egger for comments and benefited from feedback received at the Bundesbank Research seminar, the 9th ifo Dresden Workshop on Regional Economics, the Household Finance and Consumption Network (HFCN) meeting at the ECB, the Macro and Money Brown Bag Seminar at Goethe University, and the DPE seminar series at the IWH. The views expressed are those of the authors and do not necessarily represent the views of Deutsche Bundesbank or the Eurosystem. All errors are our own.

†Conference Presentations: 9th ifo Dresden Workshop on Regional Economics, Dresden, Germany, September 2019; Household Finance and Consumption Network (HFCN) meeting, Frankfurt, Germany , November 2019.

‡All co-authors contributed equally.

§Otto-von-Guericke University Magdeburg, Halle Institute for Economic Research (IWH) and Deutsche Bundesbank, Germany. E-mail: michael.koetter@iwh-halle.de

¶Deutsche Bundesbank, Germany. E-mail: philipp.marek@bundesbank.de

‖Halle Institute for Economic Research (IWH), Germany. E-mail: antonios.mavropoulos@iwh-halle.de

1 Introduction

We investigate if and how real estate transaction tax hikes (RETT) slow down mortgage credit supply by banks. Understanding the (in)ability of fiscal policy to curtail mortgage lending is important because many historical financial crises were preceded by housing booms (Reinhart and Rogoff, 2008; Brunnermeier and Schnabel, 2015). Whereas these events triggered the launch of novel macroprudential policy tools to contain excessive (mortgage) borrowing, such as loan-to-value caps, their effectiveness remains ambiguous (Grodecka, 2020). This paper provides evidence on an alternative mechanism to impact mortgage credit as a historically important driver of financial instability: fiscal policies.

Many scholars attribute the real estate bubble that preceded the Great Financial Crisis (GFC) of 2007 to mortgage lending hikes due to deteriorating lending standards and poor securitization practices in the US (Mian and Sufi, 2009, 2011; Keys et al., 2010; Favara and Imbs, 2015; Justiniano et al., 2019). Real estate bubbles may disguise excessive household borrowing against overvalued collateral (Cloyne et al., 2019), thereby displacing corporate lending (Farhi and Tirole, 2012).[1] The loose monetary policy stance since the GFC fueled again mortgage lending in the US (Rodnyansky and Darmouni, 2017; Mian and Sufi, 2018; Chakraborty et al., 2020), but also in large European economies like Germany (Koetter, 2019), and concomitant house price hikes concern guardians of financial stability once more.[2]

Our approach to answer if fiscal policy can contain mortgage lending by its effect on real estate prices exploits a unique empirical setting that combines spatial heterogeneity of fiscal shocks, granular information on real estate prices, and administrative data about the population of regional mortgage lending. We use the staggered introduction of Real Estate Transaction Taxes (RETT, *"Grunderwerbsteuer"*) across the 16 federal states of Germany on quality-adjusted regional house price indices (HPI). To isolate mortgage credit supply adjustments due to fiscal policy shocks, we regress instrumented regional HPIs on bank-level mortgage lending by regional banks. This setting of autonomous tax changes paired with granular bank and housing market data overcomes the notorious challenge that real estate prices and credit supply are jointly determined (e.g. Gerlach and Peng, 2005; Hott, 2011).

The main upshot of our empirical exercises is that fiscal policy hikes can contain mortgage lending. An increase of RETT by 1 percentage point reduces purchase prices contemporaneously by 20 basis points and on average by 17.5 basis points in each of the subsequent six quarters. Rental prices, in turn, respond only mildly in the quarter of the RETT hike, exhibiting a fall by 6.8 basis points. An important qualification emerges

[1] Either during the build-up of imbalances if banks re-allocate lending (Chakraborty et al., 2018) or after drastic price corrections (Peek and Rosengren, 2000; Gan, 2007) that cause sudden lending stops.

[2] Deutsche Bundesbank (2018) estimates an excess pricing in large German cities of 15-30% since 2017.

from the separation of urban and rural regions. Both exhibit eventually declining purchase prices and price-to-rent ratios, but the effect is driven by rural regions. Urban real estate markets exhibit a substantially smaller and later purchase price impact. The effect in rural and urban rental markets oppose another: rents decline in the former, but increase in the latter. These responses suggest that potential buyers of real estate are forced to rent due to the RETT, thereby exerting upward pressure on rental prices in urban regions. Using increases in the RETT as a predictor for changes in HPI, results from a instrumental variable regression yield that a 1 percentage point drop in predicted HPI leads to a 1.2% decline in mortgage lending by regional banks in rural areas. Mortgage supply by banks in urban regions, in turn, does not exhibit a statistically significant response. Except for the very tails of the bank capitalization distribution, these effects remain significantly positive. Hence, the effectiveness of fiscal policies to contain mortgage lending depends on the regional real estate market to which a bank caters rather than its capitalization.

The analyses in this paper proceed in three steps. First, we follow Bauer et al. (2013) and develop a quality adjusted, quarterly hedonic HPI at a granular regional level (NUTS-3, *"Kreis"*) to overcome the lack of according official statistics. HPI changes are based on approximately 33 million observations on residential properties offered online for sale or rent between January 2007 and October 2017 on the real estate portal *Immobilienscout24* (Boelmann and Schaffner, 2018).[3] We consider asking prices for residential dwellings (houses and apartments) that are offered for rent or sale between the first quarter of 2008 up to the last quarter of 2017. These granular information allow us to account for the well-documented spatial heterogeneity in house price dynamics between rural and urban regions (see e.g. Mian and Sufi, 2009; Holly et al., 2010) and resulting asymmetric reactions to policy interventions (Saiz, 2010).

Second, we gauge the effects of transfer tax hikes on the hedonic HPI by exploiting the staggered introduction of different RETT in 14 out of 16 federal states as shown in Figure 1, ranging from 3.5% in Bavaria and Saxony to 6.5% in four other states in 2018.

-Figure 1 here-

Staggered changes of the RETT across states make for an ideal quasi-natural experiment for a number of reasons. Most importantly, the mandate to set the tax was relegated from the federal level to the 16 states in 2006. This change was part of a larger effort to provide states with means to consolidate their public budgets so as to comply with a new fiscal rule—the so-called debt brake (“*Schuldenbremse*”)—that prohibited German states from running structural deficits as of 2020 (Heinemann et al., 2016). Fiscal policy choices set at the state-level to consolidate public finances so as to comply with new budget rules anchored in German Basic Law are arguably orthogonal to mortgage lending choices of

[3]Germany comprises 402 NUTS-3 regions that belong to one of the 16 federal states. *Immobilienscout24* is the largest real estate web platform, and covers 50% of all online residential listings in Germany.

regional banks at the county level. At the same time, RETT hikes exert strong direct effects on house prices due to more equity required for obligatory downpayments. These must not be part of the mortgage by German law. Hence, RETT hikes increase purchase prices and reduce demand for real estate directly without affecting mortgage supply in and of itself, which strongly suggest them as a valid instrument of HPI changes to identify mortgage supply responses.

In the third step, we instrument house price growth with RETT changes per region to explain mortgage lending supply by regional savings (*"Sparkassen"*) and cooperative banks (*"Genossenschaftsbanken"*). Regional banks are organized in associations and adhere to *de jure* or *de facto* rules to operate only in the NUTS-3 regions where they reside (Koetter and Popov, 2020). This feature of German banking creates an ideal setting to investigate whether regional house price fluctuations affect bank-specific mortgage lending supply.

Our paper speaks to the literature on the effects of fiscal policy on real estate markets. Most studies focus on the number of transactions in response to tax changes and document that tax hikes reduce market depth, depress prices, and reduce trading volumes. For example, Dachis et al. (2011) find that the introduction of a 1.1% land transfer tax in Toronto resulted in a 16% fall in sale transactions and a 1.5% drop in values. Similarly, Fritzsche and Vandrei (2016) find that a one percentage point higher transfer tax yields approximately 6% fewer transactions over the long run. Petkova and Weichenrieder (2017) also observe fewer transactions after the RETT introduction, but no significant price effects. We take a more granular approach regarding house prices and are the first to identify resulting mortgage lending effects, which is important form a financial stability perspective.

We also relate to studies that investigate how rising house prices affect household and firm choices, such as increasing consumption (Mian and Sufi, 2011), surging leverage because of higher collateral values that alleviate credit constraints Cvijanović (2014), or more self-employment (Adelino et al., 2015). We add to this literature how households respond in terms of housing demand to fiscal policy and the commensurate adjustments by banks' mortgage supply stance in response to changing housing demand.

Finally, we complement research on the bi-directional relationship between house prices and mortgage credit that often lack geographically granular data. Overcoming this limitation allows us, in turn, to exploit strictly local fiscal shocks and lending responses by the many local banks in this large, open economy (see, e.g. Gerlach and Peng, 2005; Cloyne et al., 2019). Thereby, we isolate the causal effects of fiscal policy on mortgage supply more directly.

2 Institutional Setting and Identification

The Real Estate Transaction Tax (RETT) is ideal to identify causal effects on mortgage lending via its effect on house price dynamics because state governments changed it independent of local banks' lending stances and because it is an important fiscal tool.

Until 2006:q3, the tax rate was identically set to 3.5% in all 16 federal states. In the wake of a general reform of the relationship between national and federal legislative responsibilities (*"Föderalismusreform"*), each state received the mandate to levy the RETT independently. Only the states of Bavaria and Saxony did not change the RETT rate after 2006 and therefore serve as control groups.[4] All remaining states increased the RETT between one and four times over the sample period in nodges between 0.5% and 1.5%. In 2018, the levels of the RETT ranged from 3.5% to 6.5%. These staggered tax changes across federal states entail tax rates that are identical within each of the 16 German federal states, but that differ considerably across states (see Figure 1). Changes in the RETT are thus arguably exogenous policy shocks to local banks and their local mortgage lending choices.

The tax is economically meaningful. In 2019, the German states collected €15.8 billion in RETT, which compares to a total state-level tax income of €329.1 billion in 2019. The RETT is thus not only the largest single tax income item for federal states in Germany, it is also the fastest growing component due to both rising tax rates and real estate valuation.[5] It is calculated and collected at the regional level, typically a county or city, by the tax and revenue authority to which the buyer of a property files.

The institutional setting of the tax is as follows. After the buyer and seller agree on a price for a dwelling, a notary drafts a contract between the two parties and certifies the purchase of the property. Besides the two parties, the tax office as well as the land registry receive copies of the contract (Fritzsche and Vandrei, 2016). The tax office calculates the RETT incurred by the buyer based on the total acquisition cost: the purchase price plus any encumbrances on the property, usage rights, abatement costs and broker fees.[6]. It is waived for transactions of less than 2,500 €, inheritances, and family transfers. Importantly, the taxed amount does not count towards the market value of the property that is considered by banks as collateral or constitutes the basis for subsequently due land taxes (*"Grundsteuer"*). Thus, the RETT has to be financed by the buyer's equity and the borrower must not include it as part of her mortgage. Consequently, the RETT directly affects house prices without being mechanistically correlated with mortgage loan

[4] In fact, we exploit this discontinuity in fiscal policy treatments and compare HPI responses in contiguous counties across these states' borders in Appendix 6.

[5] See German Statistical Office: www.destatis.de.

[6] The term "property" includes fractional shares of the property, land rights and condominiums.

demand. We estimate the impact of tax hikes on regional house prices as:

$$\Delta HPI_{r,t} = \alpha_r + \alpha_t + \sum_{j=-1}^{6} \beta_j TaxIncrease_{r,t-j} + \sum_{i=1}^{2} \gamma_i \Delta Unemployment_{r,t-i} + e_{r,t}, \quad (1)$$

where r indicates the administrative NUTS-3 county and t is the quarterly calendar date. The dependent variable $HPI_{r,t}$ denotes the quarterly growth rate of regional hedonic house price indeces (HPI) that we develop below. The variable of interest captures the intensive margin of the tax increase measured in percentage points, $TaxIncrease_{r,t-j}$. As the impact of the tax change may not only unfold contemporaneously, the variable is decomposed into a lead variable one quarter prior the tax change, the contemporaneous indicator, and lagged variables for each quarter up to 6 quarters after the tax change.[7] To avoid distorting effects of subsequent tax increases, we dismiss observations if a RETT increase was implemented less than 24 months after the previous tax hike, which would preclude the distinction of anticipation and post-activation effects. Hence, we exclude regions located in the federal states of Saarland from 2012 onwards, Berlin for the year 2014 or later, and Hesse for the year 2015 or later (see Figure 1).[8] To account for dynamics in regional demand, we also specify quarterly seasonally adjusted *Unemployment* rates per county provided by the federal employment agency (*"Bundesagentur für Arbeit"*). We also specify region α_r and quarter α_t fixed effects and cluster standard errors at the state-by-quarter level. Thus, our identification exploits within-state variation of house prices while holding constant observable macro conditions at the county level.

3 Data

3.1 Real Estate Data and Hedonic House Price Indices (HPI)

From the Research Data Center Ruhr at the RWI, we obtain data on all dwellings listed on the online platform Immobilienscout24.de, Germany's largest real estate portal (Bauer et al., 2013).[9] The data comprise granular information on all apartments and houses that were offered for sale or for rent on the website between January 2007 and October 2017. We observe approximately 33 million listings that include detailed geographical information in 98.5% of cases, and many qualitative traits of dwellings. The latter data allow the construction of quality adjusted hedonic regional house price indices (HPI).

Following Bauer et al. (2013), we keep dwellings with a living space ranging from

[7]Figure 2 shows that the effect of RETT changes on house price growth vanishes six quarters after the tax increase.

[8]In Appendix 6 we also specify an indicator variable of RETT changes instead of the intensive margin and also consider samples including multiple tax hikes. All results remain qualitatively unaffected.

[9]All variables used are described in Table A.1.

25m^2 to 500m^2 and with less than 12 rooms to reduce the influcence of extreme outliers. With respect to listed houses, we keep objects with a surface area between 50m^2 and $10{,}000\text{m}^2$. We sample dwellings for sale with asking prices between €100 and €20,000 per m^2 and rental properties with a monthly rent between €2 and €30 per m^2. Note that we observe asking, but not transaction prices. However, Dinkel and Kurzrock (2012) show for rural areas in Rhineland-Palatine that apart from a slight price markup there are no systematic differences between asking and transaction prices.

If sellers adjust a listing, it is treated as a new observation with the same object identifier. Whenever the same object is listed multiple times within six months, we consider only the latest traits that are closest to the realized transaction in the HPI estimation. Each listing features a start and an end date. Listings are assigned to the month of the starting date because this is when the ask price is set.

The geographical information provided comprise zipcodes, geo-coordinates (1 km^2 grid), and administrative municipality identifiers (*"Allgemeiner Gemeindeschlüssel"*, AGS). These data are crucial to devise hedonic HPIs as the location of real estate explains most of the observed variation in asking prices. Each NUTS-3 region is divided into strata based on the municipalities (*"Gemeinde"*) nested in NUTS-3 regions. Sufficiently large municipalities are further divided into multiple strata based on zipcodes.[10] Dwellings sharing the same zipcode are geographically close and prices should not deviate too much when adjusting for quality. We adjust the data for the very few missing values of geographical information and inconsistencies.[11] A caveat associated with this approach is that the size of municipalities varies across federal states. For example, Schleswig-Holstein consists of 1,116 municipalities with less than 3 million inhabitants, whereas Nordrhein-Westfalen consists of 396 municipalities with about 18 million inhabitants. Therefore, we merge members of a union of municipalities (*"Gemeindeverband"*) into one strata. Among the German NUTS-3 regions the number of strata varies between one (county-free cities such as Straubing or Eisenach with only one zipcode) and 189 in Berlin. Finally, we exclude duplicate observations that exhibit exactly identical traits across different object identifiers.

Hedonic models are ideal for the construction of quality-adjusted house price estimates (Hill et al., 2014). For dwelling i, we adjust for k observable characteristics (e.g. size, year of construction, balcony, etc.), $X_{k,i}$, and the location as gauged by the strata, S_i (Saiz, 2010). We pursue a three-stage approach to estimate regional hedonic price indices for each of the 402 German NUTS-3 regions (Gouriéroux and Laferrère, 2009). First, we estimate a price regression for a reference stock of dwellings in a reference period (2008q1:2009q4):

[10] Postal code districts are homogeneous in size and cover around 40,000 inhabitants (Mense et al., 2018).

[11] Missing data is rare, namely 1.5% of zipcodes and 3% of AGS-code and geo-coordinates, respectively. Postbox zipcodes are replaced by the dominant zipcode in the reported geo-coordinate.

$$\ln P_{i,y,q} = \alpha_0 + \sum_{k=1}^{K} \alpha_k X_{k,i} + \sum_{s=1}^{S} \beta_s S_{s,i} + \sum_{y=1}^{2} \gamma_y Y_y + \sum_{q=1}^{4} \delta_q Q_q + \epsilon_{i,y,q}, \tag{2}$$

where $P_{i,y,q}$ denotes the asked price or rent per m^2 of dwelling i in year y and quarter q. The model contains an intercept, α_0, a vector of housing characteristics with k elements –$X_{k,i}$–, a vector equal to the number of strata s in the NUTS-3 county –$S_{s,i}$–, annual fixed effects –Y_y– per year y and seasonal fixed effects –Q_q– in quarter q within the reference period. Second, we estimate the price of a reference dwelling –P_0–, at mean values of the covariates denoted in equation (2) during the reference period:

$$\ln \hat{P}_0 = \hat{\alpha}_0 + \sum_{k=1}^{K} \hat{\alpha}_k \bar{X}_{k,0} + \sum_{s=1}^{S} \hat{\beta}_s \bar{S}_{s,0} + \sum_{y=1}^{Y} \hat{\gamma}_y \bar{Y}_{y,0} + \sum_{q=1}^{4} \hat{\delta}_q \bar{Q}_{q,0}. \tag{3}$$

Third, we estimate the price of the reference dwelling in period τ by adjusting the observed price of dwelling i in period τ. Specifically, we account for differences in the characteristics between dwelling i and the reference dwelling given the average traits in the reference period. Whereas we omit year fixed effects, which are only specified within the reference period, quarterly indicators account for seasonality:

$$\ln \hat{P}_{i,\tau} = \ln P_{i,\tau} - \sum_{k=1}^{K} \hat{\alpha}_k (X_{k,i,\tau} - \bar{X}_{k,0}) - \sum_{s=1}^{S} \hat{\beta}_s (S_i - \bar{S}_{s,0}) - \sum_{q=1}^{4} \delta_q (Q_q - \bar{Q}_{q,0}). \tag{4}$$

The hedonic price index for period τ is derived from the average of adjusted estimated prices:

$$\hat{P}_\tau = \frac{1}{N} \sum_{i=1}^{N} \exp(\hat{P}_{i,\tau}). \tag{5}$$

The price-to-rent ratio index equals the ratio of the hedonic price and rent per m^2 at each period. The hedonic HPI are smoothed by cubic splines and each time series is divided by equidistant knots (see Mense et al., 2018). Between these knots, the time series is fitted by a function with three polynomials, which is estimated after each knot separately. The interval between the knots equals four quarters.

The two top panels in Table 1 underpin the importance to control for observable quality differences of real estate when aiming to assess the effect of tax changes.

- Table 1 here -

We describe the data one year before and after a tax hike in terms of means, standard deviations, and observations. The right-most columns show a t-test whether the means of variables exhibit statistically significant differences before and after an increase in the RETT. Consider the comparison of mean price-to-rent ratios and HPI in both purchase and rental markets between regions with (Treated) and without (Control) changes in the RETT. Mean hedonic HPI are statistically different from another whereas the moments of the raw price and rent data shown in the second panel of Table 1 are not statistically discernible from another. These differences in hedonic HPI bode well for the identification of mortgage lending responses to fiscal policy via the effect on real estate prices.

3.2 Banking data

To analyze the relationship between regional housing markets and mortgage lending, we obtain detailed financial data for the population of all banks operating in Germany from microprudential supervisory reports filed with Deutsche Bundesbank (*cf.* Table A.1.) We source balance sheet information from the Monthly Balance Sheet Statistics database (*"Monatliche Bilanzstatistik"*, BISTA Beier et al., 2017), which comprise end-of-month book values of assets and liabilities since 1999. We approximate mortgage lending to households by the amount of outstanding loans to individuals with a maturity of at least five years.

Since we focus on the relationship between regional housing markets and mortgage loan supply, we consider only regional savings cooperative banks. These banks are organized in pan-regional or national banking associations. They are obliged to operate on *de jure* or *de facto* delineated local markets[12]. Therefore, we follow existing literature and assign each bank based on the location of its headquarter to a unique NUTS-3 region (see e.g. Koetter and Popov, 2020). To control for observable bank traits that co-determine bank-lending choices, we account for size of the bank and specify also the natural logarithm of total assets. Additional bank-level covariates are the deposits and equity ratio (Chakraborty et al., 2020) as well as the liquidity and securities ratio (Koetter, 2019).

4 The Effect of RETT Hikes on House Prices

4.1 Headline Results

Table 2 shows the results when regressing the percentage point change in RETT rates on the growth rates of quality-adjusted purchase HPI, rent HPI, and price-to-rent ratios for the period 2008 to 2017.

-Table 2 here-

The first three columns report the results obtained for the entire sample of all NUTS-3 regions in Germany. Regarding potential HPI responses to RETT changes, we consider first regional markets to purchase real estate. Column (1) shows that the growth rate of

[12] See Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung (2013) p.232 for further details.

house prices declines by 21 basis points in the quarter of the tax increase. This effect remains constant up and until five quarters after the tax change, accumulating to around 120 basis points.

The over-proportionate price response in real estate markets is remarkable, but may reflect concerns voiced by policy makers about supply lags in selected urban regions that lead to overheated markets (Deutsche Bundesbank, 2018). Given the ample evidence on important regional differences (Himmelberg et al., 2005; Holly et al., 2010), we therefore consider urban and rural regions separately below.

Before, we turn to another concern related to policy measures that aim to mitigate real estate price hikes. A potential (unintended) consequence of requiring more equity in real estate transactions is to force potential buyers into rental markets, thereby exerting upward pressure on rents (Petkova and Weichenrieder, 2017). The according effect of the RETT on rents is shown in column (2) and does not support such concerns. It is statistically significant, but relatively small, summing up to a decline of around 10 basis points.

At the same time, column (3) shows that the price-to-rent ratio declines significantly by 67 basis points over the course of five quarters after the tax shock. To the extent that price-to-rent ratios gauge the returns that real estate investors expect to earn (Himmelberg et al., 2005), RETT changes appear to burden capital owners relatively more than consumers of rental housing.

Given that the effect of RETT changes on regional real estate purchase HPI vanishes after six quarters, as illustrated also by Figure 2, we specify in columns (4) to (6) of Table 2 a joint coefficient for this post-RETT change period instead of estimating quarter-specific responses.

– Figure 2 here –

This average response per quarter of purchase HPI, rent HPI and price-to-rent ratios exhibits qualitatively very similar results and almost idential goodness of fit measures. Column (4) shows that a one percentage point increase in the RETT induces negative growth rates of house prices on the order of 18 basis points per quarter, accumulating to 125 basis points six quarters after the RETT hike. The response of rents shown in column (5) resembles the relatively weak negative impact estimated in column (2), but is not statistically significant. Hence, the significant decline of price-to-rent ratios in column (6), accumulating to 85 basis point six quarters after a tax increase of one percent, is indeed mainly driven by a contraction of asset values rather than expected yields accruing to capital owners from renting.

This initial assessment whether RETT changes caused a change in house prices hinges crucially on the assumption that such policy changes were not anticipated by market participants. Otherwise, agents adjust their behavior prior to the shock, for instance by preponing transactions or by staying below tax thresholds as shown by Kopczuk and Munroe (2015), which would invalidate our identification strategy. Therefore, we specify a lead indicator equal to the value of the tax hike in the quarter before the actual policy shock in all six specifications. The coefficient of this lead indicator has no significant effect on house price growth, which bodes well for our identification strategy.

4.2 Rural versus urban regions

Real estate dynamics differ fundamentally between rural and urban agglomeration areas (Himmelberg et al., 2005; Saiz, 2010). The latter are smaller in size and more densely populated, which implies constrained housing supply due to scarce space, thus exerting price pressure given demand. Excess demand paired with development lags in the supply of new urban housing are often blamed to drive overvaluations on the order or 30% in urban regions according to Deutsche Bundesbank (2018). Therefore, we show estimation results for the cumulative RETT hike responses for sub-samples of urban and rural areas in Table 3.

-Table 3 here-

We define urban areas as cities that are not assigned to a NUTS-3 region, so-called county-free cities (*"Kreisfreie Stadt"*).[13]. Out of the 402 counties that correspond to NUTS-3 regions, around 107 are urban areas according to this definition.

The regional distinction between urban and rural regions reveals important additional insights. House price growth in rural real estate markets is substantially more affected by increases in the RETT than urban ones. Columns (1) and (4) clearly indicate that purchase prices contract in the former by 26 basis points contemporaneously whereas they do not respond significantly in more densely populated agglomeration areas. The dynamic effects are for both types of regional markets statistically significant, but the responses differ quantitatively. Purchase prices in urban regions contract by 93 basis points after six quarters, whereas the value depreciation in rural areas amounts to 15 basis points more. Taken together, these estimates support the notion that more densely populated agglomeration areas exhibit a substantially lower price elasticity of demand in real estate markets. This feature would render the effectiveness of small scale policies aiming to mitigate the emergence of real estate bubbles more limited compared to rural areas that face less tight demand conditions and fewer supply side frictions in the supply of housing.

Columns (2) and (5) highlight another important difference in the response of real estate markets to RETT hikes. Whereas rents in rural regions confirm the smaller decline in rental HPIs documented in Table 2, column (2) shows that urban rental markets suffer

[13]The status of county-free city is generally given to large cities with more than 100,000 inhabitants.

indeed from upward pressure in response to RETT hikes. Hence, requiring higher equity capital due to increased down-payments may have, especially in already tight regional real estate markets, unintended consequences by forcing potential buyers into renting real estate.

Columns (3) and (6) indicate that any crowding-out of potential buyers bears also important implications for different price-to-rent ratio responses to a given RETT hike. While negative in both types of regional real estate markets, the investment required to realize rental income contracts more in urban regions compared to rural areas. The cumulative effect amounts to 135 basis points in cities after six quarters, whereas it is only 72 basis points in less densely populated markets. On the margin, more substantial reductions in price-to-earnings ratios in urban regions may thus attract further investments in already tight regional markets. Clearly, such a re-allocation of real estate investment would depend on the availability of credit, to which we turn below. Before doing so, we discuss a number of scrutiny checks regarding the validity of identifying real estate market responses to fiscal policy changes.

4.3 Scrutiny checks

Figure 6 provides a range of tests to scrutinize the measurement of regional house prices, the specification of tax changes, and the identification of house price responses to RETT shocks. First, we test whether and how important it is to develop a regional HPI at quarterly frequency. We replicate the baseline results for all purchase prices, rents, and price-to-rent ratios in the full and for the regionally differentiated samples in Appendix 6. These results underscore the crucial importance to account for quality differences and unobserved regional macro conditions because simply specifying the moments based on the raw listing data yields virtually no significant relationship with fiscal policy changes.

Second, we consider in Appendix 6 the sensitivity of our headline results towards the specification of the intensity of tax changes rather than a simple indicator of possible changes of the fiscal stance of state governments towards their real estate market. Whereas the saturation of the specification with regional fixed effects as well as county-level unemployment as a proxy for regional macro conditions greatly enhances the explanatory power of the estimation, the choice of an indicator or continuous RETT change variable makes no qualitative difference for our main findings.

Third, we tackle the notorious challenge to identify causal effects of RETT changes on house prices in three ways. In Appendix 6, we replicate Table 2 using randomly generated tax treatments. These placebo shocks are all statistically insignificant. In Appendix 6, we conduct a panel regression at the level of federal states presented to test whether changes in the RETT are orthogonal to regional house prices. The results presented in Table A.5 clearly show that the RETT is not affected by the development of previous house prices. Both tests corroborate the validity of RETT changes as exogenous fiscal shocks to HPI. A more elaborate test to this end in Appendix 6 focuses on a subset of Bavarian regions, which did not experience any tax changes, and compare them to bordering regions in Baden-Württemberg that were subject to a RETT hike in November 2011. By ensuring otherwise equal (county) macro conditions, and by focusing on a single tax shock in otherwise identical regions, we can apply standard difference-in-difference techniques (Card and Krueger, 1994; Huang, 2008). Results are both qualitatively and quantitaviely strikingly similar.

5 Regional Real Estate Responses and Mortgage Credit

5.1 Empirical specification

The evidence so far establishes that fiscal policy has a causal effect on regional real estate markets, both regarding the purchase price of dwellings as well as rents. We turn now to the question, if fiscal policy can thereby also dampen the supply of mortgage credit. To that end, we estimate the impact of regional HPI growth on changes in regional mortgage lending of local banks. To account for endogeneity between both factors, we instrument house price growth with changes in the RETT. Predicted changes in purchase and rent HPI in regional real estate markets constitute the exogenous shock for credit demand to identify how house price changes affect bank-level mortgage credit supply. We apply a two-stage instrumental variable (IV) regression and use a GMM framework.

The first stage of the IV approach resembles Equation 1, where we specify the cumulative effect of RETT hikes up and until six quarters after state-specific tax policy changes (see columns 3-6 in Table 2) and the lead variable of the tax change. Banks located in Bavaria or Saxony are excluded from this analysis, since no tax increase was activated in these federal states throughout the whole sample period. In the second stage of the IV, we specify outstanding mortgage credit of bank b in quarterly date t, $MortgageLending_{b,t}$ as:[14]

$$\Delta MortgageLending_{b,t} = \alpha_b + \alpha_t + \beta_1 \Delta \hat{Price}_{r,t-1} + \gamma BankControls_{b,t-1} + e_{b,t}. \quad (6)$$

The predicted house price growth obtained from the first stage of the IV, $\Delta \hat{Price}$, is the main explanatory variable of interest. We specify also a set of lagged bank-specific control variables, $BankControls_{b,t-1}$: the natural logarithms of total assets, as well as deposit, equity, liquidity, and securities ratios (see also subsection 3.2). Bank and time fixed effects are included. Standard errors are clustered at the county by quarter level.

Note that the dimensionality of the dependent variable, estimated at the bank-level,

[14]Recall that we approximate mortgage lending by the amount of outstanding loans to private individuals with a maturity of more than 5 years of bank b at time t.

and the instrumented variable obtained from the first stage at the level of NUTS-3 regions, are different in the second stage of the IV. This difference can lead to inconsistent estimates of the standard errors. We account for the different dimensions within the IV framework using 1,000 bootstrapped iterations of the sample. Each bootstrapped sample is based on a random sample drawn at the level of NUTS-3 regions with replacement. The empirical results are obtained by deriving the mean and the standard deviation of the corresponding coefficient estimates. The validity of the instrumented variables is assessed through the Kleibergen-Paap and Cragg-Donald F-Statistics. As the distribution of the F-Statistics is not symmetric around the mean, the output provides information on the median value of the corresponding test statistics.

5.2 Headline results

Table 4 shows the estimation results to explain mortgage lending as a function of regional real estate price developments. Given the important regional differences in real estate price responses to taxes documented above, we show results for the total sample as well as for urban and rural regions only. The first three columns depict OLS estimates where we specify observed price changes as main explanatory variable. Real estate price hikes correlate positively with mortgage lending growth, although the quantitative effect is rather small and only statistically significant at the 10%-level for the total and the urban regions sample. But against the backdrop of the extensively documented interdependence between house prices and mortgage lending, these estimates may suffer from endogeneity and be inconsistent.

-Table 4 here-

Therefore, columns (4) to (6) of Table 4 provide the estimates obtained from the bootstrapped IV regressions using the predicted HPI growth rate as main explanatory variable. Price effect estimates according to OLS are much smaller compared to the IV regression. At the same time, the results for the validity of instruments strongly support an IV specification over the OLS estimator.[15] Specifically, the Kleibergen-Paap and the Cragg-Donald F-test support the validity of the RETT increase as instruments for house price growth for the total sample and for rural regions. The Kleibergen-Paap F-tests report values of 12.59 for the entire sample and 11.63 for the subsample of rural regions. These values are above the critical value of 10 suggested by Stock and Yogo (2005). Furthermore, the Cragg-Donald tests are above the 5% critical values for both samples. For the sub-sample of urban regions, the Kleibergen-Paap F-Statistic reports a value of 1.2, while the Cragg-Donald test statistics reaches a value of 4.8. Both values are far below

[15]In finite samples, the mean squared error of the biased OLS estimator of an endogenous variable can actually be smaller than the mean squared error of a correctly specified IV estimator. This is because of the efficiency loss (as described above) and because of the finite-sample bias of the IV estimator.

the corresponding critical value. Hence, changes in the RETT may be considered as a weak instrument assessing house price growth in urban region. These tests confirm our findings from section 4, which showed that increases in the RETT unfold stronger impact in rural housing markets.

For the entire sample, the instrumental regression results indicate that banks increase their mortgage lending by 1.4% in response to a house price increase of 1 percent in the previous period. Taking the previous result into consideration, that a 1 percentage point increase in the RETT leads to a 1.2% decline in house prices, the elastic reaction of house price growth on mortgage lending suggests that a 1%-increase in the tax rate reduces mortgage lending by about 1.7%. For rural regions, the estimates suggest an elasticity of 1.2 translating into a decline of 1.6% as a reaction to 1 percentage point increase in the RETT. The coefficient estimate for urban region is similar in the magnitude to the previous estimates, but it is exposed to a variance eight times higher in comparison to the other (sub)samples. Therefore, the estimates of the IV regression suggest that the effect of the changes in the RETT affect prices and quantities in regional housing markets.

In sum, the empirical findings strongly suggest that fiscal policy can be effective to contain mortgage credit supply through a dampening effect on the demand for dwellings to purchase. Importantly, this effect is only statistically significant in rural regions, which face less tight demand conditions and fewer construction lags to provide newly-built housing. To further assess whether and to what extent fiscal policies can serve as an instrument to contain a hallmark driver of financial instability—mortgage credit supply—we shed next more light on lending responses conditional on bank capitalization.

5.3 RETT lending responses conditional on bank capitalization

We consider mortgage lending responses conditional on the capitalization profiles because one of the main responses by policy makers after the GFC to enhance the resilience of the financial system was to require higher higher core capital buffers. Higher equity capital was deemed one if not the most important macroprudential tool because of the insight that insufficiently capitalized banks pose a threat to financial stability in case of systemic and sudden asset price deterioration–such as bursting housing market bubbles in the US in 2007.

Given the ample evidence on substantial spatial heterogeneity of these real estate asset price bubbles and their dissolution (see, e.g. Holly et al., 2010), we investigate banks' mortgage supply responses to this credit demand shock conditional on both their capitalization as well as their geographical location by means of interactions with instrumented house price growth. According to Goldsmith-Pinkham et al. (2018), the interaction of the instrument obtained from a single first stage IV regression may suffer from misspecification. Therefore, we use Bartik instruments for each covariate containing the instrumented variable. These covariates are the dependent variables in the first stage of the IV regression, which are estimated in separate regressions. These predicted values are then specified as explanatory variables in the second stage.

-Table 5 here-

Table 5 shows the results from these IV specifications using Bartik instruments. Column (1) features two instrumented covariates obtained from the first stage of the IV regression, namely the predicted purchase HPI growth and the interaction term between the HPI and the dummy for rural regions. The specification in column (2) contains a time-varying dummy, LOW_{t-1}, that indicates whether a bank's capitalization was in the bottom quartile (P25) of the equity ratio distribution across banks in each period t. Column (3) contains both interaction terms.

Column (1) of Table 5 confirms the headline finding that increasing house prices reduce the supply of mortgage credit by banks located in rural regions. We estimate an elasticity between house prices and mortgage lending equal to 0.65 in urban regions and 1.31 in rural ones. The elasticity of 1.37 between house prices and mortgage lending in Column (2) is significant. The interaction term between house price growth and the indicator of bottom quartile capitalization is insignificant though. This result suggests that location matters more than the capital ratio for a bank's reaction to house price changes, a finding confirmed when specifying indicators of location and poor capitalization in Column (3). For rural regions, the estimates suggest an elasticity of 1.35 for banks with a capitalization better than the bottom quartile and an elasticity of 1.33 for relatively poorly capitalized banks. In urban regions, the elasticity equals 0.79 for banks with a capitalization above the lowest quartile and an elasticity of 0.42 for low-capitalized banks. Except for the latter type of banks in urban regions, the elasticity estimates are signficantly different from zero.

The consideration of a continuous capitalization allows a more detailed assessment of the impact of capitalization on mortgage lending. Hence, column (4) builds upon the interaction term of the price indicator and the equity ratio of the bank, whereas column (5) combines terms for the price indicator interacted with the rural dummy and the equity ratio, respectively. At first glance, the estimation results do not seem to provide additional insights. When omitting the spatial component in column (4), the estimates for the predicted price and for the interaction term are positive but insignificant. The same holds for the specification reported in column (5) with a positive impact of the interaction between the price and the rural dummy being the only significant term.

-Figure 3 and Figure 4 here-

To put these interactions into perspective, we derive marginal effects of price growth changes on mortgage lending growth conditional on the bank capitalization distribution from regression results using the Delta-Method. Figure 3 illustrates these marginal effects of capitalization for the specification in column (4). Medium capitalized banks are more likely to respond to price fluctuations, whereas the effect is insignificant for banks at the top or at the bottom of the capitalization distribution. Figure 4 reflects the estimates from column (5) in Table 5. The effect of house price changes on mortgage lending in rural regions is significantly positive with an elasticity of around 1.3 across the entire capitalization distribution.[16] For urban regions, Figure 4 shows that only medium-capitalized banks significantly react to changes in house prices with an elasticity of about one.

6 Conclusion

In this paper, we exploit a unique combination of high-frequency, quality-adjusted house price index (HPI) responses to staggered real estate transaction tax (RETT) hikes to show that fiscal policy shocks can contain mortgage lending supply of regional banks. This insight is crucial in light of soaring real estate prices and the historically pivotal role played by credit-driven real estate bubbles in times of loose monetary policy that jeopardized financial resilience in many economies.

Tax changes at the state level are arguably exogenous to mortgage credit supply of regional banks, which allows us to isolate causal effects of RETT hikes via real estate price effects on housing demand on credit supply. A one percentage point increase in the RETT dampens house price growth in rural regions by more than 1%. As rents remain unaffected, changes in house prices also translate into changes in the price-to-rent ratio. Urban regions, which face tighter real estate markets, exhibit a slight increase in rental growth, which may indicate a an unintended crowding out of potential buyers towards renting markets. These results are robust to alternative empirical measurement methods of both house price indices and the tax shock, a randomized policy treatments, and when focusing only on contiguous regions in two states with very similar macro and banking market conditions.

To isolate the effect of fiscal policy changes on mortgage lending by regional banks, we specify RETT changes as an instrument in an IV setting to explain HPI growth. The results show that mortgage lending supply growth is elastic with respect to changes in HPI growth in rural regions, but not in urban ones. This result corroborates the subordinate importance of the RETT for real estate investment decisions in urban housing markets. Controlling for bank capitalization profiles, we do not find any differentiating evidence. Our analysis clearly shows that location matters more than the capital position of banks for its reaction to house price changes.

The irrelevance of the bank's capitalization on the transmission of house price changes

[16]Column (5) of Table 5 implies a negligibly small elasticity of mortgage lending of 2 basis point w.r.t. an additional percent in the capital ratio.

to mortgage lending has potentially important implications for the conduct of financial stability policies aiming to contain mortgage lending supply. Whereas higher capital requirements to generally strengthen banks' loss-absorbing capacities, we find little indication that it influences its stance on supplying mortgage credit. The effectiveness of fiscal policy that dampens housing demand, in turn, bodes well for borrower-based macroprudential instruments pertaining to ensuring stable mortgage lending practices, such as the loan-to-value (LTV) ratio. Similar to the LTV, changes in the RETT affect the down-payment requirements. Hence, our result may serve as indirect evidence that increased equity requirements due to LTV caps as a macroprudential instrument can mitigate housing demand and thereby mortgage issuance, mostly in rural regions. Regionally differentiated policies between urban and rural regions that account for the differences in demand elasticities documented in this paper may therefore be warranted.

Table 1: Regional and bank level summary statitics

	Total Sample						Treated Regions			Control Regions			T-test	
			Percentile				One year after tax increase							
	Mean	Std.Dev.	25th	50th	75th	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Delta	p-value
Dependent Variables - Hedonic Index														
Price to Rent ratio	0.3	2.0	-0.5	0.3	1.1	15276	0.3	1.6	1713	0.5	1.6	7049	0.24***	(0.00)
Price per sqm.	0.9	2.0	0.0	0.9	1.8	15276	0.9	1.5	1713	1.3	1.6	7049	0.45***	(0.00)
Rent per sqm.	0.6	0.8	0.2	0.6	1.0	15276	0.5	0.6	1713	0.8	0.8	7049	0.21***	(0.00)
Auxilliary Dependent Variables - Statistical Averages														
Price to Rent ratio	0.4	11.5	-5.3	0.4	6.0	15270	0.1	11.0	1713	0.4	11.8	7049	0.34	(0.27)
Price per sqm.	1.1	10.5	-4.0	1.1	6.2	15270	0.7	10.4	1713	1.2	10.6	7049	0.49	(0.08)
Rent per sqm.	0.7	5.0	-1.6	0.6	2.9	15276	0.7	4.5	1713	0.8	5.4	7049	0.15	(0.28)
Regional Controls - Only quarterly frequency														
Unemployment	-0.2	0.6	-0.5	-0.2	0.1	14070	-0.3	0.5	1713	-0.4	0.5	7049	-0.03*	(0.04)
Dependent Variables - Bank Lending														
Total household lending	0.1	0.2	-0.0	0.0	0.1	50685	0.1	0.3	6227	0.1	0.2	22941	0.01**	(0.01)
Mortgage Lending	-0.0	1.2	-0.2	-0.1	0.1	35434	0.1	1.4	5799	-0.1	1.0	17760	-0.15***	(0.00)
Collateralized household lending	-0.0	1.0	-0.1	-0.1	0.0	35961	0.0	1.1	5915	-0.0	0.7	17998	-0.05***	(0.00)
Collateralized household lending(w/o Mortgages)	-0.0	1.2	-0.2	-0.1	0.1	35631	0.1	1.4	5835	-0.1	1.0	17846	-0.14***	(0.00)
Unsecured household lending	0.2	1.2	0.0	0.1	0.3	33926	0.2	1.1	5653	0.3	1.3	16895	0.14***	(0.00)
Bank Controls														
Log size	18.8	1.4	17.9	18.8	19.8	53715	18.8	1.4	6227	18.8	1.4	22941	-0.00	(0.98)
Deposits ratio	72.4	8.9	67.7	73.5	78.4	53715	71.0	8.6	6227	73.9	8.6	22941	2.90***	(0.00)
Liquidity ratio	0.8	0.4	0.6	0.7	1.0	53715	0.8	0.3	6227	0.8	0.4	22941	0.01*	(0.03)
Securities ratio	27.0	11.9	18.5	25.8	34.0	53715	25.7	11.8	6227	29.1	12.0	22941	3.38***	(0.00)

This table shows descriptive statistics for the three main and auxilliary regional dependent variables and regional controls in growths from the previous quarter between q1:2008 and a4:2017. The second panel of the table respectively depicts summary statistics for the bank lending dependent variables and bank controls in growths from the previous quarter. Regions correspond to 392 counties ("Kreise") and there are 1516 regional banks in our sample. We show total sample statistics and distinguish between treated and non-treated regions for the following year after the tax increase. In the last two columns we perform a t-test of the mean differences between the two sub-samples and report the first difference and its statistical significance. Variables are defined in Table A.1. All values are percentages(i.e. $0,3 = 0,3\%$). *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: RETT Increase effects on the hedonic price indices for prices, rents and the price-to-rent ratio

	Event Analysis			Pre-Post Dummy Analysis		
	Price(%) (1)	Rent(%) (2)	Price-to-Rent(%) (3)	Price(%) (4)	Rent(%) (5)	Price-to-Rent(%) (6)
Tax Increase$_{t+1}$	-0.127	-0.047	-0.082	-0.117	-0.040	-0.078
	(0.097)	(0.029)	(0.104)	(0.098)	(0.030)	(0.104)
Tax Increase$_t$	-0.069^{*}	-0.141	-0.044	-0.203^{**}	-0.068^{*}	-0.137
	(0.100)	(0.037)	(0.100)	(0.101)	(0.037)	(0.100)
Tax Increase$_{t-(1-6)}$				-0.175^{***}	-0.037	-0.140^{***}
				(0.042)	(0.030)	(0.044)
Tax Increase$_{t-1}$	-0.232^{***}	-0.093^{*}	-0.142^{*}			
	(0.086)	(0.055)	(0.082)			
Tax Increase$_{t-2}$	-0.228^{***}	-0.115	-0.118			
	(0.071)	(0.089)	(0.095)			
Tax Increase$_{t-3}$	-0.209^{***}	-0.013	-0.198^{**}			
	(0.068)	(0.038)	(0.081)			
Tax Increase$_{t-4}$	-0.190^{***}	0.003	-0.195^{**}			
	(0.066)	(0.035)	(0.077)			
Tax Increase$_{t-5}$	-0.131^{**}	0.005	-0.138^{**}			
	(0.063)	(0.029)	(0.070)			
Tax Increase$_{t-6}$	-0.046	0.001	-0.047			
	(0.061)	(0.024)	(0.059)			
Regional Controls						
Unemployment Change$_{t-1}$	-0.135^{**}	0.024	-0.159^{***}	-0.139^{**}	0.020	-0.159^{***}
	(0.054)	(0.026)	(0.060)	(0.055)	(0.026)	(0.060)
Unemployment Change$_{t-2}$	0.062	-0.075^{***}	0.138^{**}	0.067	-0.069^{***}	0.137^{**}
	(0.051)	(0.025)	(0.057)	(0.051)	(0.025)	(0.057)
Observations	12,801	12,801	12,801	12,801	12,801	12,801
R-squared	0.223	0.231	0.117	0.222	0.230	0.117
TimeFE	Yes	Yes	Yes	Yes	Yes	Yes
RegionFE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster S.E. (State*Quarter)	Yes	Yes	Yes	Yes	Yes	Yes

This Table illustrates the regression assessing the impact of increases in the RETT on the growth of the HPI for Prices, Rents and Price-to-Rent ratio. The dependent variables are the quarterly growth rates of the quality adjusted prices per square meter, rents per square meter as well as the price-to-rent ratio. The variable of interest –Tax Increase– is an intensity measure capturing the level of changes in the RETT. We illustrate two different specifications using a cumulative dummy regression in Columns (1-3) and an event analysis in Columns (4-6). In the pre-post dummy analysis, variable Tax Increase$_{t-(1-6)}$ accounts for the 6 quarters following a change in the RETT. We also include quarterly unemployment change from year to year in order to account for the general macroeconomic trend at the regional level. Regional fixed effects at the county level and time fixed effects are included. Standard errors are clustered on the Federal State times quarter level. *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: RETT increase effects on the hedonic price indices for prices, rents and the price-to-rent ratio – Regional decomposition

	Urban Regions			Rural Regions		
	Price(%) (1)	Rent(%) (2)	Price-to-Rent(%) (3)	Price(%) (4)	Rent(%) (5)	Price-to-Rent(%) (6)
Tax Increase$_{t+1}$	0.069	-0.005	0.074	-0.184	-0.046	-0.140
	(0.069)	(0.030)	(0.061)	(0.131)	(0.037)	(0.143)
Tax Increase$_t$	-0.044	0.020	-0.065	-0.258^{**}	-0.090^{**}	-0.170
	(0.076)	(0.039)	(0.071)	(0.125)	(0.043)	(0.128)
Tax Increase$_{t-(1-6)}$	-0.155^{***}	0.069^{**}	-0.225^{***}	-0.180^{***}	-0.063^{*}	-0.120^{**}
	(0.060)	(0.031)	(0.060)	(0.049)	(0.034)	(0.057)
Regional Controls						
Unemployment Change$_{t-1}$	-0.187^{**}	0.016	-0.204^{**}	-0.100	0.034	-0.133
	(0.077)	(0.036)	(0.089)	(0.079)	(0.030)	(0.083)
Unemployment Change$_{t-2}$	0.078	-0.086^{**}	0.164^{*}	0.049	-0.058^{**}	0.108
	(0.081)	(0.039)	(0.094)	(0.069)	(0.029)	(0.074)
Observations	3,555	3,555	3,555	9,246	9,246	9,246
R-squared	0.227	0.260	0.131	0.227	0.237	0.122
TimeFE	Yes	Yes	Yes	Yes	Yes	Yes
RegionFE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster S.E. (State*Quarter)	Yes	Yes	Yes	Yes	Yes	Yes

This Table illustrates the regression assessing the impact of increases in the RETT on the growth of the HPI for Prices, Rents and Price-to-Rent ratio, respective to regional typology. The dependent variables are the quarterly growth rates of the quality adjusted prices per square meter, rents per square meter as well as the price-to-rent ratio. The main variable of interest –Tax Increase– is an intensity measure capturing the level of changes in the RETT. Variable Tax Increase$_{t-(1-6)}$ accounts for the 6 quarters following a change in the RETT. In Columns (1-3), we illustrate the effects of RETT increases in Urban (Kreisfreie Cities) housing markets. Columns (4-6) show the effects for Rural housing markets. We also include quarterly unemployment change from year to year in order to account for the general macroeconomic trend at the regional level. Regional fixed effects at the county level and time fixed effects are included. Standard errors are clustered on the Federal State times quarter level. *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: IV-Regressions (2^{nd}-stage) on the effect of house price growth on mortgage lending growth

	OLS			IV		
	Total	Urban	Rural	Total	Urban	Rural
$\Delta \hat{Price}_{t-1}$	0.043*	0.126*	0.019	1.365**	1.534	1.193*
	(0.025)	(0.068)	(0.068)	(0.626)	(5.038)	(0.661)
Bank Controls						
Log Total assets$_{t-1}$	−12.07***	−10.96***	−12.40***	−11.97***	−10.92***	−12.27***
	(1.501)	(1.640)	(1.629)	(1.211)	(3.186)	(1.319)
Equity Ratio$_{t-1}$	−0.617***	−0.603***	−0.580***	−0.618***	−0.536**	−0.606***
	(0.096)	(0.177)	(0.129)	(0.106)	(0.259)	(0.141)
Securities Ratio$_{t-1}$	0.013	0.044**	0.006	0.007	0.033	0.002
	(0.009)	(0.019)	(0.010)	(0.012)	(0.048)	(0.012)
Liquidity Ratio$_{t-1}$	−0.005***	−0.003	−0.006***	−0.005***	−0.005	−0.006***
	(0.002)	(0.004)	(0.002)	(0.002)	(0.010)	(0.002)
Deposits Ratio$_{t-1}$	−0.089***	−0.058**	−0.097***	−0.091***	−0.067	−0.096***
	(0.017)	(0.039)	(0.019)	(0.018)	(0.044)	(0.021)
Observations	48,865	9,176	39,689	48,865	9,176	39,689
R-Squared	0.096	0.103	0.092			
Kleibergen Paap F-stat				12.59	1.216	11.63
Cragg- Donald F-stat				81.54	4.753	78.33
TimeFE	Yes	Yes	Yes	Yes	Yes	Yes
RegionFE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster S.E. (County*Quarter)	Yes	Yes	Yes	Yes	Yes	Yes

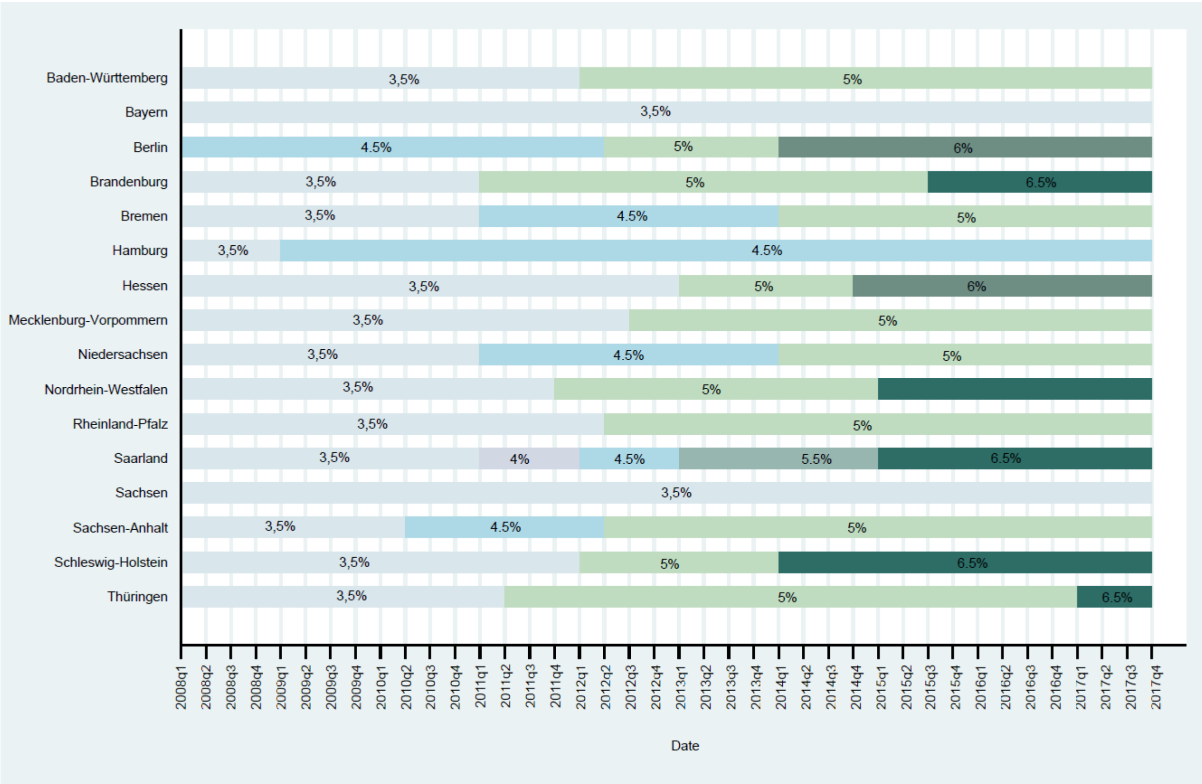
This Table shows the regression results of the effects of changes in house price growth on household lending growth. The dependent variable is mortgage lending growth. The main variable of interest –$\Delta \hat{Price}_{t-1}$– is the quality adjusted house price quarterly change in Columns (1-3), whereas for Columns (4-6) is the predicted quality adjusted house price quarterly change from the first stage of the instrumental regression. We dont use the predicted HPI information for the federal states of Bavaria and Saxony, as there were no changes in the RETT in these regions. We furthermore include Bank and Time (Quarter) fixed effects. Standard errors are clustered on the County times quarter level. Due to the difference in sample dimensions between the first and second stage of the IV regression, we bootstrap our results using 1000 iterations. The Kleibergen-Paap and Cragg-Donald F-stat of the first stage are also reported. *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: IV-Regressions (2^{nd}-stage) on the effect of house price growth on mortgage lending growth - Conditional on bank location and capitalization

	Mortage Lending Growth				
	(1)	(2)	(3)	(4)	(5)
$\Delta \hat{Price}_{t-1}$	0.648**	1.371***	0.787***	-0.055	-0.885
	(0.270)	(0.384)	(0.309)	(1.449)	(1.246)
Rural $*\Delta \hat{Price}_{t-1}$	0.662***		0.568*		2.134**
	(0.237)		(0.291)		(1.073)
Low_{t-1} $*\Delta \hat{Price}_{t-1}$		-0.299	-0.362		
		(0.303)	(0.302)		
Rural $*$ $\text{Low}_{t-1}*\Delta \hat{Price}_{t-1}$			0.333		
			(0.598)		
Equity $\text{Ratio}_{t-1}*\Delta \hat{Price}_{t-1}$				0.219	0.286
				(0.227)	(0.224)
Rural $*$ Equity $\text{Ratio}_{t-1}*\Delta \hat{Price}_{t-1}$					-0.266
					(0.192)
Low_{t-1}		0.271	0.191		
		(0.333)	(0.536)		
Rural $*$ Low_{t-1}			-0.106		
			(0.737)		
Log Total assets_{t-1}	-11.99***	-12.06***	-12.02***	-12.20***	-12.03***
	(1.501)	(1.519)	(1.531)	(1.534)	(1.509)
Equity Ratio_{t-1}	-0.616***	-0.640***	-0.622***	-0.831***	-0.703***
	(0.097)	(0.104)	(0.107)	(0.248)	(0.161)
Securities Ratio_{t-1}	0.006	0.007	0.006	0.004	0.006
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Liquidity Ratio_{t-1}	-0.006***	-0.006***	-0.005***	-0.006***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Deposits Ratio_{t-1}	-0.084***	-0.089***	-0.083***	-0.082***	-0.083***
	(0.017)	(0.017)	(0.017)	(0.019)	(0.017)
Observations	48,865	48,865	48,865	48,865	48,865
F-Stat	10.91	9.458	7.516	10.83	8.602
Time FE	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Clustered S.E. (County*Quarter)	Yes	Yes	Yes	Yes	Yes

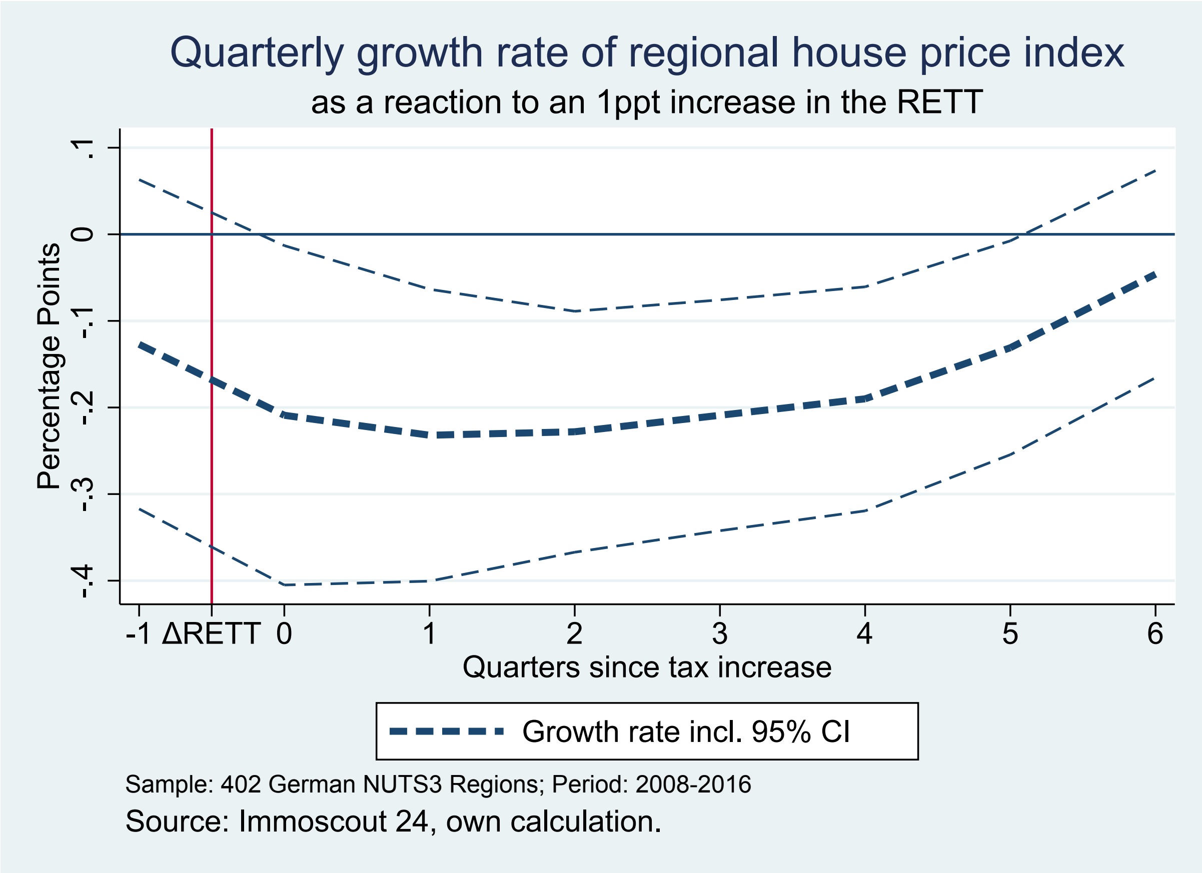
This Table shows the regression results of the effects of changes in instrumented quality adjusted house price growth on household lending growth, conditional on bank location and capitalization. The dependent variable is the quarterly growth rate of mortgage lending from the previous quarter. Each covariate capturing house price changes is included by means of Bartik instruments, estimated in separate first stage regressions. The main variable of interest –$\Delta \hat{Price}_{t-1}$– is the predicted quality adjusted house price quarterly change from the first stage of the instrumental regression. Variable –Rural– is a dummy variable equal to 1 when the bank is located in a rural region. Variable –Low– is a time-varying dummy variable indicating a bank below the bottom (P25) capitalization quartile, whereas Equity Ratio is a continuous measure of bank capitalization. Bank characteristics as well as fixed effects at the bank and time level are included as control variables. Standard errors are clustered at the county times quarter level. Banks located in in Bavaria and Saxony are excluded, since no tax increase was activated in these federal states throughout the whole sample period. *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Real Estate Transfer Tax in German States from 2008 to 2017



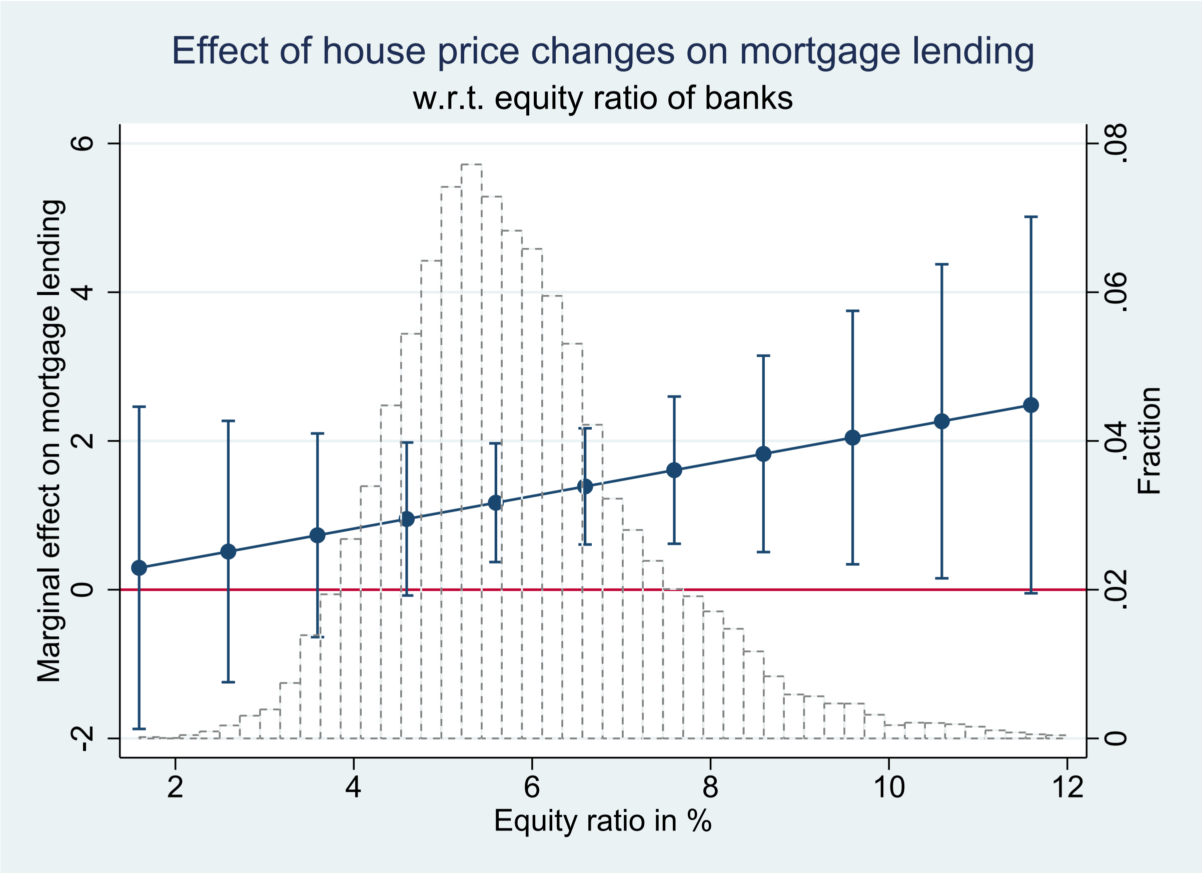
Notes: This figure represents the levels of the real estate transfer tax (RETT) in the German federal states from the first quarter of 2018 until the end of 2017. The figure does not report the first state change (Berlin, 2006:q3) since our house price data begin in 2008. Source: Official announcements of German state governments.

Figure 2: Tax increase and the regional house price index



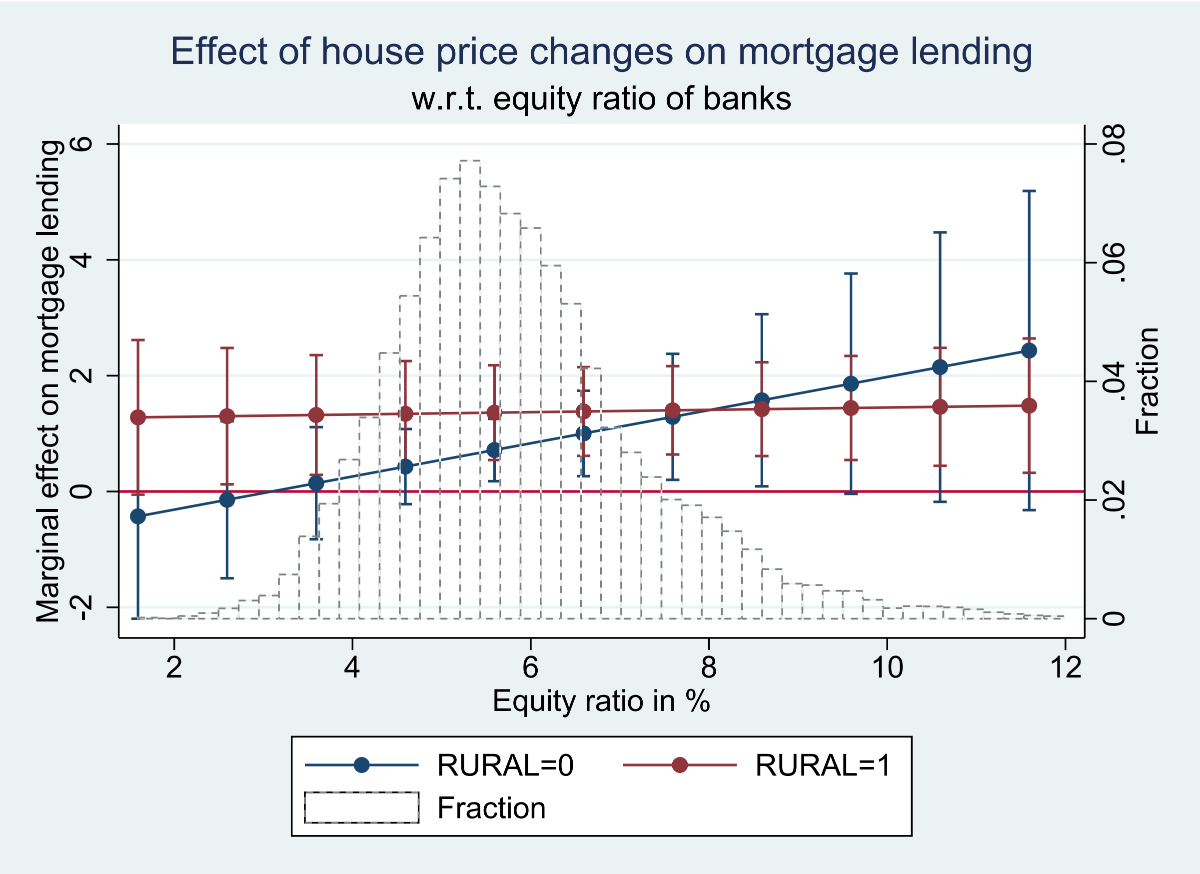
Notes: This figure illustrates the effect of a 1ppt RETT increase on the regional house price index for the sample of all 402-NUTS-3 regions in Germany. The graph is based on an event study presented in Table 2 in Column (1). The coefficent estimates are represented by the solid line, the correpsonding 95%-confidence interval by the dashed lines. Source: Immoscout24.de, own calculations.

Figure 3: Marginal effects of house price increases conditional on bank capitalization



Notes: This figure illustrates the effect of house price growth induced by a change in the RETT on mortgage lending conditional on the capitalization of regionally operating banks. The graph is based on the 2^{nd}-stage of the IV-regression presented in Table 5 in Column (4). The estimated effect is presented by the blue line with dots. The intercept (for banks with an equity ration of 0) is determined by the coefficient estimates for $\Delta \hat{Price}_{t-1}$, namely -0.055, whereas the slope is determined by the interaction term of $\Delta \hat{Price}_{t-1}$ and $Equity\ Ratio_{t-1}$ taking a value of 0.219. The joint standard errors are obtained by means of the Delta-Method. For each dot on the blue line, the vertical line represents the corresponding 95%-confidence interval. The distribution of the equity ratio of the banks' in the sample is illustrated by the dashed bars in the background of the figure. Source: Immoscout24.de, Monthly Balance Sheet Statistics, own calculations.

Figure 4: Marginal effects of house price increases conditional on bank capitalization and geographic location.



Notes: This figure illustrates the effect of house price growth induced by a change in the RETT on mortgage lending conditional on the capitalization of regionally operating banks and their geographic location. The latter refers to a distinction between banks located in urban regions (defined as county-free cities), and rural regions (counties with at least 2 municipalities). The graph is based on the 2^{nd}-stage of the IV-regression presented in Table 5 in Column (5). The estimated effect for banks operating in urban regions is presented by the blue line with dots, while the red line represents the effect for banks located in rural regions. For banks located in urban regions, the intercept (for banks with an equity ration of 0) is determined by the coefficient estimate for $\Delta \hat{Price}_{t-1}$, namely -0.885, whereas the corresponding slope is determined by the interaction term of $\Delta \hat{Price}_{t-1}$ and $Equity\ Ratio_{t-1}$ taking a value of 0.286. For banks operating in rural regions, the intercept is determined by the sum of the coefficient estimates for $\Delta \hat{Price}_{t-1}$ and $Rural * \Delta \hat{Price}_{t-1}$, equal to 1.249. The corresponding slope is determined by sum of two interaction terms, $Equity\ Ratio_{t-1}$+$\Delta \hat{Price}_{t-1}$ and $Rural$*$Equity\ Ratio_{t-1}$+$\Delta \hat{Price}_{t-1}$ taking a value of 0.020. The joint standard errors are obtained by means of the Delta-Method. For each dot on both lines, the vertical line represents the corresponding 95%-confidence interval. The effect of zero is marked by a red line. In the case that the vertical line capturing the 95% confidence interval does not intersect with the zero line, the effect may be consdiered as signfiicant coditional on a bank's equity ratio. The distribution of the equity ratio of the banks' in the sample is illustrated by the dashed bars in the background of the figure. Source: Immoscout24.de, Monthly Balance Sheet Statistics, own calculations.

Appendix

Hedonic House Price Index vs Statistical Averages

Instead of using a quality adjusted house price index, we specify here observed statistical averages at the NUTS-3 level based on the raw listing data instead of the hedonic price indices developed in subsection 3.1. Table A.2 show the regression outcomes of Equation 1 using the observed averages for prices, rents and the price-to-rent ratio. These estimates indicate no statistically significant effect of the tax regime on house prices. The specification of a RETT change indicator variable instead of the intensity of tax changes does not affect the results qualitatively and is available upon request.

-Table A.2 here-

We conclude that a rigorous (fiscal) policy evaluation thus necessitates the development of more sophisticated house price indices that gauge sample size changes, quality adjustments, time, and stratification effects.

Gauging RETT changes with an indicator

The variable $TaxIncrease$ is an intensive measure of the level of the tax change. This metric gauges more information than a dummy variable, since we have increases ranging from 0.5% to 1.5%. It also eases the interpretation of economic significance because we can associate a tax increase of 1% to a quantifiable change in house prices. At the same time, the more important effect on regional real estate markets maybe the signal sent by state government that the fiscal policy stance changed. In that case, the mere existence of a change conditional on other (un)observable state-specific traits may be decisive for the effectiveness of RETT changes.

Therefore, Table A.3 illustrates the sensitivity of our headline results towards choosing an indicator versus a continuous tax change variable, the specification of regional fixed effects, and the inclusion of observable county-level macro conditions gauged by changes in the unemployment rate.

-Table A.3 here-

The empirical results are shown here for the full sample and the price-to-rent ratio specified as the dependent variable. They are qualitatively very similar to those reported in Table 3. This also holds for the subsamples and the other outcome variables, which are available upon request.

Fiscal placebo shocks

To scrutinize the findings of Table 2, we run placebo regressions with random treatments at random time stamps and treatment groups. Table A.4 shows the regression outcomes of 1000 bootstrap "random treatment" estimations.

-Table A.4 around here-

We choose to run 1000 arbitrary simulations to approximate the coefficient estimates of 16^{26} (16 Federal states, 27 times the tax was increased) possible treatment combinations. The bootstrap simulations illustrate that coefficients converge after approximately 40-60 iterations. In each simulation, random federal states implement a tax increase of 0.5%–1.5% at random periods. The actually treated regions serve as not-treated control groups. This outcome corroborates our headline results in Table 2. The insignificant estimates strongly suggest that the dampening effect of RETT hikes on hedonic HPI is not a statistical artifact.

Determinants of RETT changes

In a next step, we test the exogeneity of changes in the RETT from the development of previous house prices. As the level of the RETT is determined at the federal state level, we conduct a panel regression at the level of federal states explaining the level of the RETT. The fixed-effect regression can be expressed as follows:

$$TaxRate_{f,y} = \alpha_f + \alpha_y + \beta_1 \ln HousePrice_{f,y-1} + \beta_2 \ln Debt\ p.c._{f,y-1} + \beta_3\ last\ election + e_{y,t} \quad (7)$$

with $TaxRate_{f,y}$ capturing the level of the RETT in percentage points in federal state f in year y. The key variable of interest, $\ln HousePrice_{f,y-1}$, measures the lagged logartihm of the population-weighted average value of the hedonic house price indices of the NUTS-3 regions located in the corresponding federal state. The relegation of the RETT to the federal state level was part of a larger effort to provide states with means to consolidate their public budgets. Hence, we include the federal states' debt per capita ratio as a control variable, which is provided by the German Federal Ministry of Finance at an annual frequency. The panel regression is implemented at an annual level. The model further contains dummy varialbes with respect to the number of years since the last election in the corresponding federal state with the election year serving as the benchmark. We also specify fixed effects at the level of federal states α_f, year dummies α_y and standard-errors clustered at the level of federal states.

-Table A.5 around here-

The results of the panel regression are reported in Table A.5. The key variable of interest, $\ln HousePrice_{f,y-1}$, as well as the fixed effects are included in each specification. The estimates clearly show that the level of the RETT is not affected by the previous development of the housing market. The debt per captia indicator is included in column (2) and (4) with estimates suggesting that an increase in the federal states' debt may induce a rise in the RETT. The estimates with respect to the time since last election provide a small indication that governments do not increase the RETT in years of election.

Comparing contiguous counties

The staggered scheme of increases in the RETT prevents a classical differences-in-differences setup for a sample of all German regions for three reasons. First, the tax was increased in 14 out of 16 federal states leading to a relatively small control group. Second, across federal states the RETT was increased at different points in time. Third, several federal states raised the RETT multiple times. Therefore, the main analysis of RETT hike effects on regional housing markets presented in section 4 is based on an event analysis. Yet, the comparison between a subset of very similar regions may sharpen our attempt to identify causal effects of RETT changes. We therefore sacrifice some external validity and focus in this appendix on regional real estate markets located in the federal states of Baden-Württemberg and Bavaria.

In Bavaria, the RETT remained at 3.5% whereas it was increased from 3.5% to 5% in Baden-Württemberg in November 2011. It was the only increase of the RETT in one of the two federal states, which are both relatively large. Baden-Württemberg has slightly more than 11 million inhabitants living in 44 NUTS-3 regions, whereas Bavaria consists of 96 NUTS-3 regions with nearly 13 million inhabitants.[17] Both states are located in the south of Germany and share a common border of 829 km (Gebhardt, 2008, see p.45) with 10 NUTS-3 regions in Baden-Württemberg and 13 regions in Bavaria. Beyond common geographic characteristics, the economic figures are very similar between both federal states. In 2018, the unemployment rates in both federal states were the lowest in Germany with 2.2% in Bavaria and 2.5% in Baden-Württemberg, whereas the national unemployment rate was at 3.8%.[18] Except for the three German city-states, GDP per capita reaches the hightest figures in Bavaria (EUR 46,100) and Baden-Württemberg (EUR 45,200). Hence, this setting lends itself to a classical diff-in-diff set up with sufficiently many observations, in which real estate located in Baden-Württemberg is treated and Bavarian ones are the control group.

Table A.6 here-

To test whether the dependent variable (house prices) is exposed to a common trend in both groups before the RETT hike in Baden-Württemberg in November 2011, consider Graphs A.1 and A.2 as well as Table A.6. The latter shows that house price growth in all Bavarian regions was significantly higher prior the tax increase, which is also illustrated

[17]See 2018 figures provided by eurostat regional database, *demo_r_d2jan*.
[18]See 2018 figures provided by eurostat regional database, *lfst_r_lfur2gac*.

in Graph A.2. Hence, a classical diff-in-diff analysis might lead to biased estimates. In contrast, for the subsample of NUTS-3 regions located at the border between both federal states the common trend of house price growth seems to hold. Until 2011 house price growth between the treated regions in Baden-Württemberg did not differ significantly from the growth rates in the control regions in Bavaria. In the first two years after the tax increase, house price growth in Bavaria was significantly above the average growth rate in Baden-Württemberg. This observation would be in-line with the economic expectation that a tax increase leads to lower house prices in the treated regions.

Graphs A.1 and A.2 here-

Differences between the sub-sample of contiguous regions and the subsample of all regions located in the two federal states may reflect heterogeneity in the regional housing markets within states. This heterogeneity is especially pronounced in Bavaria. According to the hedonic price index described in section 3.1, the seven most expensive NUTS-3 regions are located in the area of Munich with each exceeding a square-meter-price of EUR 5,000 in 2017. In these regions house prices nearly doubled in comparison to the prices in 2010. At the same time, northern regions in Bavaria located at the former inner-German border exhibit among the lowest real estate prices in Germany. In 2017, average house prices per m^2 remained below the value of EUR 1,000 and had hardly experienced any increase with respect to the house price values in 2010. Given this important intra-state price dispersion, we focus on contiguous regions, which also reduces the vulnerability to regional shocks. We implement the diff-in-diff analysis for all and contiguous counties in the two states as follows:

$$\ln P_{r,t} = \alpha_r + \alpha_t + \beta Treatment_{r,t} + \sum_{i=1}^{2} \gamma_i \Delta Unemployment_{r,t-i} + e_{r,t}. \tag{8}$$

The log of regional house prices $P_{r,t}$ is the dependent variable. $Treatment_{r,t}$ denotes the main variable of interest, namely the magnitude of the tax increase in percentage points of 1.5 so as to interpret β as an elasticity. We also specify two lags of regional unemployment rate changes as well as time and regional fixed effects. For each subsample, the regression is implemented three times: (i) an unweighted scheme, (ii) using the regions' population figures of the year 2010 as weights, (iii) weights obtained from a weighting procedure coping with the potential violation of the common trend assumption, which is more relevant for the subsample of all 140 regions located in both federal states. To ensure sufficiently similar house price growth before the policy shock between both groups of dwellings, we assign weights to the control group by means of entropy balancing (see Hainmueller, 2012). These weights may take only non-negative values for the control group, leading to similar values of the variable of interest; in our case the average growth rate of house prices. The weights obtained for the pre-treatment year 2010 are used for the analysis with the period of investigation from 2008 until 2017. The average growth rate of house prices in Bavaria is 0.54 for the sub-sample of contiguous regions and 0.569 for the subsample using all 140 regions (see Table A.6).

Table A.7 here-

Estimation results are shown in Table A.7. Without weights, the estimates for contiguous regions suggest that a one percentage point RETT increase reduces regional house prices by about 1.2%. For all 140 regions, the coefficient estimate is twice as large with a value of 2.5%, which may be biased though given the violation of the common trend assumption. This bias is supported by the more pronounced elasticity when accounting for the population size. This weighting scheme assigns higher weights to larger cities, such as Munich, which experienced very strong real estate price appreciation. When controlling for the violation of the common trend by means of entropy balancing, the elasticity shrinks to a value of about 2. This value is still above the estimates based on the sub-sample of contiguous regions. Accounting for the population size, a one percentage point increase in the RETT induces a decline of house prices by 0.5%, while the specification based on balancing weights suggests a drop of 1.6%.

Overall, a one percentage point increase in the real estate transfer tax induces a decline of regional house prices of slightly more than 1%. Hence, this conservative identification strategy yields strikingly similar results compared to the event analysis in Section 4.

Table A.1: Variable definition

Variable name	Source	Unit	Frequency	Level	Description
Primary dependent variables: House Prices					
Price per square meter	RWI Essen(Immobilienscout24.de)	Levels	Quarterly(2008-2017)	County(Kreis)	Size adjusted house and apartment purchase prices
Rent per square meter	RWI Essen(Immobilienscout24.de)	Levels	Quarterly(2008-2017)	County(Kreis)	Size adjusted house and apartment rental price
Alternative dependent variables: Household Mortgage Lending lending					
Mortgage Lending	BISTA	Levels	Monthly(1999-2017)	Bank	Balance sheet information on collateralized household lending
Bank Controls lagged by one quarter					
Bank Size	BISTA	Levels	Monthly(1999-2017)	Bank	Total assets minus total loans
Deposits Ratio	BISTA	Ratio	Monthly(1999-2017)	Bank	Total deposits over total assets
Equity Ratio	BISTA	Ratio	Monthly(1999-2017)	Bank	Total capital over total assets
Liquidity Ratio	BISTA	Ratio	Monthly(1999-2017)	Bank	Cash on Hand over total assets
Securities Ratio	BISTA	Ratio	Monthly(1999-2017)	Bank	Total securities over total assets
Regional Macro-Aggregates					
Unemployment	Buntesagentur fuer Arbeit	Levels	Quarterly(2009-2017)	County(Kreis)	Regional Unemployment Rate

This Table shows definitions and sources of the variables. Acronym for BISTA stands for Monatliche Bilanzstatistik; provided by the Deutsche Bundesbank.

Table A.2: RETT increase effects on observed statistical averages for prices, rents and the price-to-rent ratio growth

	Total Sample			Urban Regions			Rural Regions		
	Price to Rent(%) (1)	Price(%) (2)	Rent(%) (3)	Price to Rent(%) (4)	Price(%) (5)	Rent(%) (6)	Price to Rent(%) (7)	Price(%) (8)	Rent(%) (9)
Tax Increase$_t$	0.000	−0.003	−0.003*	−0.006	−0.007	−0.001	0.004	−0.000	−0.004*
	(0.004)	(0.004)	(0.002)	(0.006)	(0.005)	(0.003)	(0.005)	(0.004)	(0.002)
Tax Increase$_{t-1}$	0.001	−0.001	−0.003	0.007	0.005	−0.003	−0.001	−0.003	−0.002
	(0.006)	(0.005)	(0.002)	(0.008)	(0.006)	(0.003)	(0.008)	(0.008)	(0.002)
Tax Increase$_{t-2}$	−0.000	−0.000	−0.000	−0.004	−0.003	0.001	0.002	0.002	−0.001
	(0.005)	(0.005)	(0.002)	(0.007)	(0.006)	(0.003)	(0.007)	(0.007)	(0.002)
Tax Increase$_{t-3}$	−0.008	−0.007	0.001	−0.008	−0.004	0.004*	−0.008	−0.008	−0.000
	(0.006)	(0.006)	(0.002)	(0.009)	(0.010)	(0.002)	(0.007)	(0.007)	(0.002)
Tax Increase$_{t-4}$	−0.002	−0.004	−0.003	−0.007	−0.010	−0.003	0.000	−0.002	−0.002
	(0.006)	(0.006)	(0.002)	(0.009)	(0.007)	(0.004)	(0.008)	(0.008)	(0.003)
Tax Increase$_{t-5}$	−0.002	−0.001	0.002	−0.001	0.004	0.006**	−0.002	−0.002	0.000
	(0.008)	(0.008)	(0.002)	(0.011)	(0.011)	(0.002)	(0.010)	(0.010)	(0.002)
Tax Increase$_{t-6}$	−0.003	−0.005	−0.002	−0.005	−0.007	−0.002	−0.002	−0.004	−0.002
	(0.006)	(0.006)	(0.002)	(0.010)	(0.011)	(0.003)	(0.007)	(0.007)	(0.002)
Regional Controls									
Unemployment Change$_{t-1}$	0.005	0.004	−0.001	0.006	0.005	−0.001	0.004	0.003	−0.000
	(0.004)	(0.004)	(0.001)	(0.006)	(0.006)	(0.002)	(0.006)	(0.005)	(0.002)
Unemployment Change$_{t-2}$	−0.002	−0.002	−0.001	−0.004	−0.004	−0.000	−0.001	−0.002	−0.000
	(0.004)	(0.004)	(0.001)	(0.006)	(0.006)	(0.002)	(0.006)	(0.005)	(0.002)
Observations	12801	12801	12801	3555	3555	3555	9246	9246	9246
R-Squared	0.024	0.032	0.019	0.030	0.031	0.055	0.030	0.040	0.016
TimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
RegionFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster S.E. (State*Quarter)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This Table shows the regressions of the Tax increase, as used in the event analysis of Table 2. The dependent variables are the observed statistical averages of the price per square meter, rental price per square meter and the price-to-rent ratio. The main variable of interest –Tax Increase– is an intensity measure capturing the level of changes in the RETT. We replicate the event analysis in Table 2 with one quarter prior until 6 quarters after the increase of the tax both for the total sample of regions, and we also split between urban and rural regions. We also include quarterly unemployment changes from year to year in order to account for the general macroeconomic trend at the regional level. Regional fixed effects at the county level and time fixed effects are included. Standard errors are clustered on the Federal State times quarter level. *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: RETT increase effects on the price-to-rent ratio growth (Dummy vs Intensity measure)

	Dummy Specification			Intensity Specification		
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Increase$_{t-1}$	-0.201	-0.163	-0.165	-0.151	-0.132	-0.132
	(0.127)	(0.199)	(0.192)	(0.149)	(0.185)	(0.185)
Tax Increase$_{t-1}$	-0.206^{*}	-0.165	-0.162	-0.159^{*}	-0.137^{*}	-0.134
	(0.070)	(0.123)	(0.133)	(0.073)	(0.093)	(0.104)
Tax Increase$_{t-2}$	-0.162	-0.122	-0.120	-0.125	-0.104	-0.101
	(0.221)	(0.339)	(0.346)	(0.239)	(0.304)	(0.319)
Tax Increase$_{t-3}$	-0.262^{**}	-0.222^{**}	-0.223^{**}	-0.210^{**}	-0.188^{**}	-0.187^{**}
	(0.013)	(0.037)	(0.034)	(0.010)	(0.020)	(0.020)
Tax Increase$_{t-4}$	-0.262^{***}	-0.233^{**}	-0.234^{**}	-0.205^{***}	-0.186^{**}	-0.187^{**}
	(0.005)	(0.021)	(0.021)	(0.004)	(0.015)	(0.015)
Tax Increase$_{t-5}$	-0.196^{**}	-0.167^{*}	-0.171^{*}	-0.149^{**}	-0.130^{*}	-0.132^{*}
	(0.012)	(0.068)	(0.064)	(0.012)	(0.060)	(0.059)
Tax Increase$_{t-6}$	-0.095	-0.067	-0.067	-0.063	-0.044	-0.044
	(0.170)	(0.412)	(0.406)	(0.214)	(0.450)	(0.447)
Regional Controls						
Unemployment Change$_{t-1}$			-0.084			-0.081
			(0.115)			(0.127)
Unemployment Change$_{t-2}$			0.098^{**}			0.093^{*}
			(0.049)			(0.060)
Observations	12801	12801	12801	12801	12801	12801
R-Squared	0.063	0.117	0.117	0.063	0.117	0.117
TimeFE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
RegionFE	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Cluster S.E. (State*Quarter)	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

This Table shows the regression results of increases in the RETT on the price-to-rent ratio growth, conditional on the morphology of the main covariate. The left hand side variable is the regional price to rent ratio growth from the previous quarter. Columns (1-3) and (4-6) illustrate the dummy and intensity Tax Increase specifications respectively. The intensive measure in Columns (4-6) provides economic interpretation of the results and also differentiates between the different levels of the increases in the tax. We furthermore drop subsequent hikes in the tax rate that take place within 8 quarters after an increase. In Columns (3) and (6), We also include quarterly unemployment changes from year to year in order to account for the general macroeconomic trend at the regional level. Regional fixed effects at the county level and time fixed effects are included. Standard errors are clustered on the Federal State times quarter level. *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: RETT increase effects on the hedonic price indices for prices, rents and the price-to-rent ratio - Random treatment

	Event Analysis			Pre-Post Dummy Analysis		
	Price(%)	Rent(%)	Price-to-Rent(%)	Price(%)	Rent(%)	Price-to-Rent(%)
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Increase$_{t+1}$	0.000	0.001	0.000	-0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tax Increase$_{t}$	0.001	0.001	0.000	0.001	0.001	0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Tax Increase$_{t-(1-6)}$				0.004	0.002	-0.002
				(0.001)	(0.000)	(0.000)
Tax Increase$_{t-1}$	0.003	0.002	-0.001			
	(0.001)	(0.001)	(0.000)			
Tax Increase$_{t-3}$	0.003	0.002	-0.001			
	(0.001)	(0.001)	(0.000)			
Tax Increase$_{t-4}$	0.003	0.001	-0.001			
	(0.001)	(0.001)	(0.000)			
Tax Increase$_{t-4}$	0.003	0.001	-0.002			
	(0.001)	(0.001)	(0.000)			
Tax Increase$_{t-5}$	0.004	0.001	-0.001			
	(0.001)	(0.001)	(0.000)			
Tax Increase$_{t-6}$	0.003	0.001	-0.001			
	(0.000)	(0.001)	(0.000)			
Regional Controls						
Unemployment Change$_{t-1}$	-0.092***	-0.085***	0.006	-0.091***	-0.077***	0.013
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Unemployment Change$_{t-2}$	0.056***	-0.000	-0.057***	0.089***	0.027*	-0.061***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

This Table illustrates the regression assessing the impact of placebo increases in the RETT on the growth of the HPI for Prices, Rents and Price-to-Rent ratio,where we bootstrap the regression outcomes 1000 thousand times, as a sufficient test for validity of our main estimates. The dependent variables are the quarterly growth rates of the quality adjusted prices per square meter, rents per square meter as well as the price-to-rent ratio. The variable of interest –Tax Increase– is an intensity measure capturing the level of changes in the RETT. We illustrate two different specifications using a cumulative dummy regression in Columns (1-3) and an event analysis in Columns (4-6). In the pre-post dummy analysis, variable Tax Increase$_{t-(1-6)}$ accounts for the 6 quarters following a change in the RETT. We also include quarterly unemployment change from year to year in order to account for the general macroeconomic trend at the regional level. Regional fixed effects at the county level and time fixed effects are included. Standard errors are clustered on the Federal State times quarter level. *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Fixed-effect regression on the impact of house prices on RETT

RETT in percentage points	(1)	(2)	(3)	(4)
$\ln HousePrice_{f,t-1}$	-2.895	-2.367	-2.888	-2.344
	(1.922)	(1.757)	(1.966)	(1.805)
$\ln$ *Debt p.c.*$_{f,t-1}$		1.239***		1.276***
		(0.183)		(0.180)
Time since last election. Year of election serving as baseline.				
one year since last election			0.142*	0.148*
			(0.073)	(0.073)
two years since last election			0.126	0.137
			(0.109)	(0.101)
three years since last election			0.194	0.245*
			(0.135)	(0.121)
four years since last election			0.124	0.142*
			(0.085)	(0.074)
Observations	144	144	144	144
Federal States	16	16	16	16
R-Squared	0.752	0.792	0.757	0.798
RegionFE(State)	Yes	Yes	Yes	Yes
TimeFE(Year)	Yes	Yes	Yes	Yes
Clustered S.E.- Federal State	State	State	State	State

Notes: This Table shows the regression results assessing the impact of regional house prices on the level of the RETT at the level of federal states. The dependent variable is the level of the RETT in percentage points. The key dependent variable, logarithm of the lagged average house price in federal state f, as well as regional fixed effects at the federal state level and year fixed effects are included in each specification. The house price in federal state f is determined by the population weighted annual average house price indices in the NUTS-3 regions located in the federal state of consideration. In Columns (2) and (4), we include the federal states debt per capita, whereas Columns (3) and (4) contain dummy varialbes capturing the time since last election. Standard errors are clustered at the federal state level. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Development of regional house prices in Baden-Würtemberg and Bavaria

	Average quarterly house price growth in %					
	Contiguous regions (23)			All regions (140)		
	1	2	3	4	5	6
	BW	BY	diff.	BW	BY	diff.
2008	0.507	0.205	0.302	-0.511	-0.457	0.053
	(0.176)	(0.290)	(0.365)	(0.140)	(0.105)	(0.084)
2009	-0.118	-0.176	0.058	0.044	0.233	0.189**
	(0.118)	(0.134)	(0.185)	(0.059)	(0.055)	(0.091)
2010	0.540	0.356	0.185	0.569	0.901	0.332***
	(0.153)	(0.146)	(0.213)	(0.059)	(0.051)	(0.085)
2011	0.870	0.820	0.050	0.755	1.433	0.677***
	(0.133)	(0.121)	(0.180)	(0.057)	(0.059)	(0.095)
RETT was increased in Baden-Württemberg in November 2011						
2012	1.106	1.750	0.645***	1.222	1.542	0.320***
	(0.134)	(0.152)	(0.210)	(0.068)	(0.064)	(0.105)
2013	0.933	1.411	0.479**	1.250	1.197	0.052
	(0.114)	(0.144)	(0.193)	(0.059)	(0.066)	(0.106)
2014	1.123	0.720	0.403*	1.239	1.201	0.037
	(0.128)	(0.172)	(0.226)	(0.063)	(0.070)	(0.111)
2015	2.297	1.678	0.619**	2.068	2.086	0.018
	(0.127)	(0.230)	(0.286)	(0.069)	(0.072)	(0.117)
2016	1.536	1.708	0.172	2.231	2.065	0.167
	(0.188)	(0.290)	(0.370)	(0.085)	(0.095)	(0.152)
2017	1.408	1.899	0.491	1.678	2.164	0.486*
	(0.366)	(0.470)	(0.626)	(0.165)	(0.184)	(0.294)
Regions	10	13		44	96	

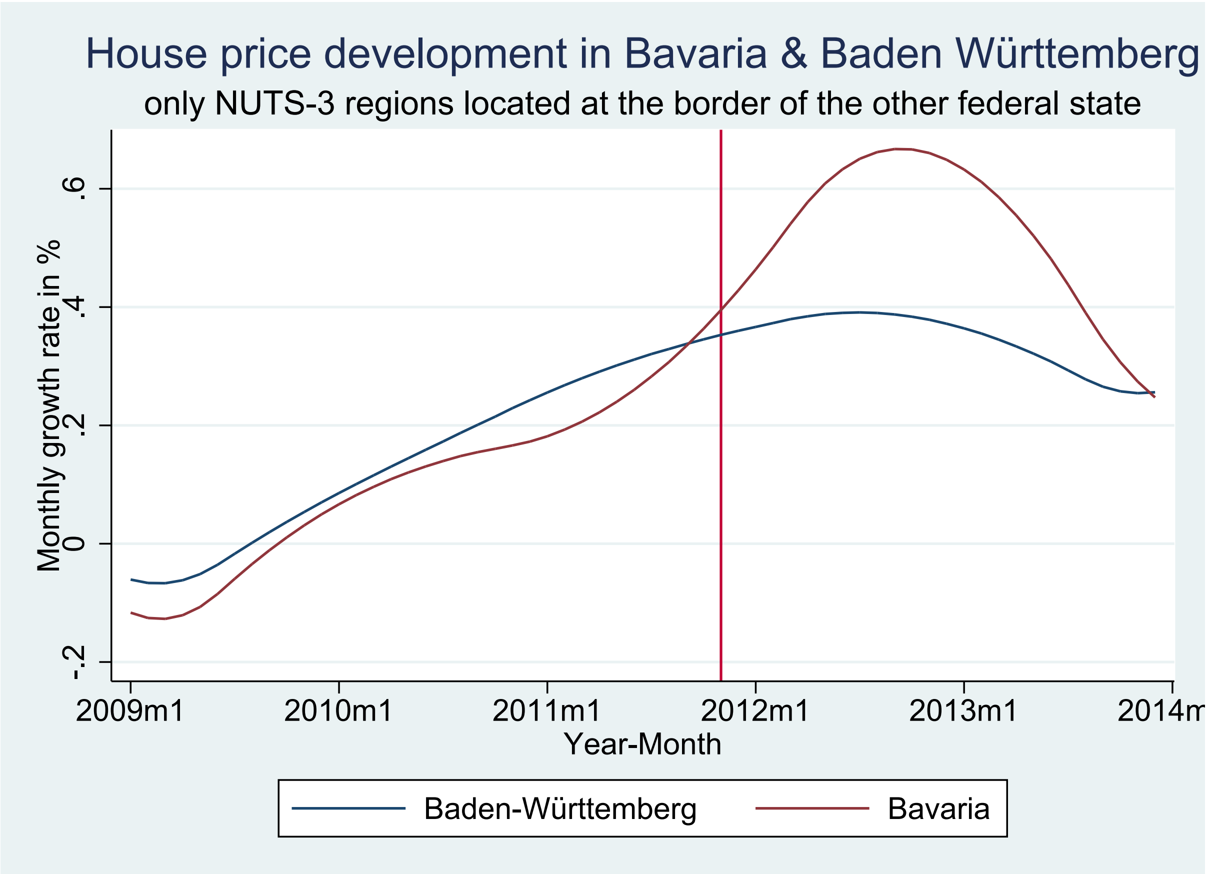
This Table illustrates the differences in the house price index growth between Baden-Württemberg(BW) and Bavaria(BY) before and after a tax increase in BW in 2011. Columns (3) and (6) illustrate the t-test difference between the two groups and significance. The tests are implemented for two subsamples. First, only contiguous regions located at the border between Baden-Württemberg and Bavaria (Columns (1-3)). Second, all NUTS-3 regions located in Baden-Württemberg and Bavaria (Columns (4-6)). *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Differences-in-Differences regression on the effects of a RETT increase regional house price index levels between Baden-Würtemberg and Bavaria

	Log levels of regional house price index					
	Contiguous regions			All regions		
¸	(1)	(2)	(3)	(4)	(5)	(6)
Post-Tax Increase in BW in %	-1.168***	-0.459*	-1.662***	-2.532***	-4.126***	-2.006***
	(0.170)	(0.231)	(0.162)	(0.210)	(0.257)	(0.122)
Unemployment Change$_{t-1}$	-1.353	-0.532	-1.171	-0.696	-1.081	-0.766
	(1.554)	(1.519)	(1.459)	(0.667)	(0.848)	(0.559)
Unemployment Change$_{t-2}$	-0.404	-0.453	-0.554	0.323	-0.112	0.413
	(1.388)	(1.393)	(1.319)	(0.623)	(0.811)	(0.543)
Observations	759	759	759	4,620	4,620	4,620
R-squared	0.956	0.956	0.959	0.973	0.979	0.975
NUTS-3 Regions	23	23	23	140	140	140
Weights	No	Pop	Ebal	No	Pop	Ebal
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered S.E. (State*Quarter)	Yes	Yes	Yes	Yes	Yes	Yes

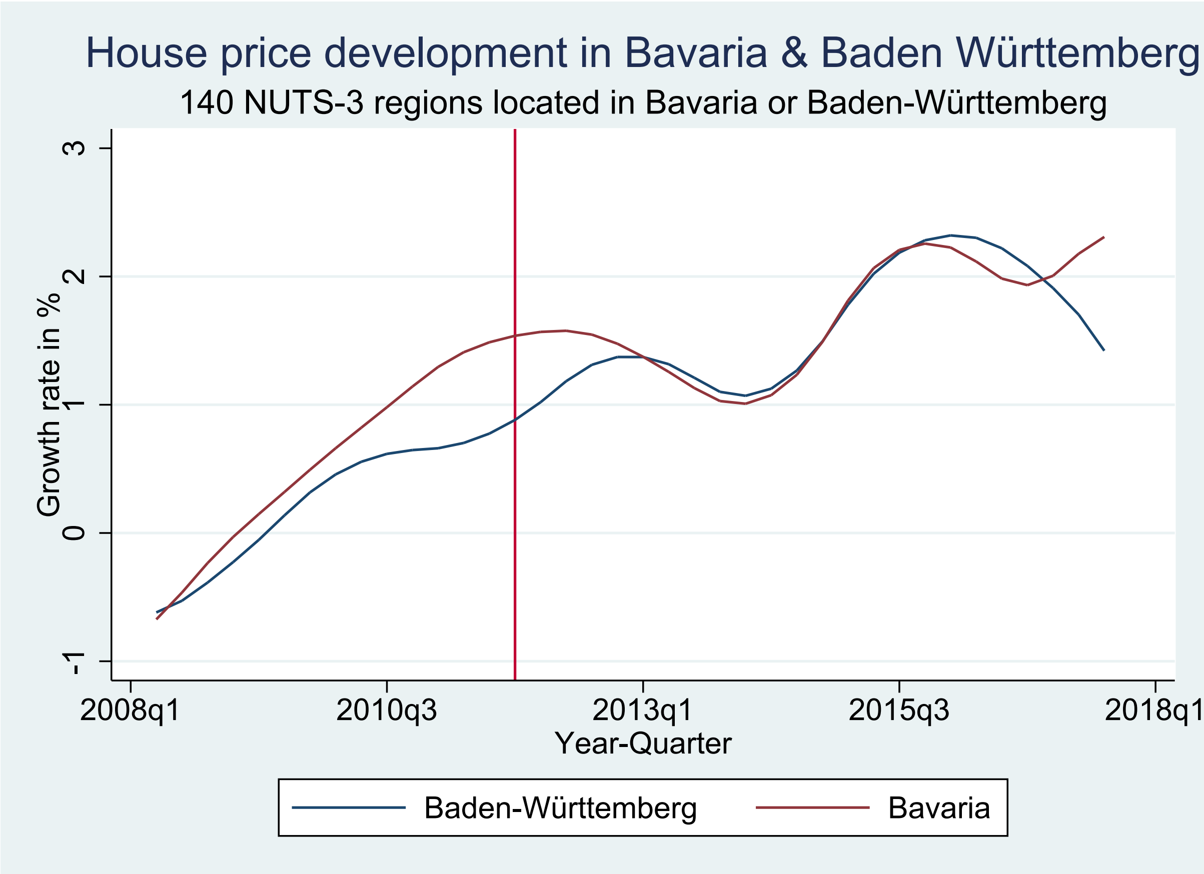
This Table shows the regression results from differences-in-differences regressions on house prices in the federal states of Baden-Württemberg and Bavaria. The dependent variable is in levels of the quality adjusted house price index. The treatment variable is defined as the tax increase in Baden-Württemberg from 3.5% to 5% in November 2011. Hence, the variable takes the value of 1.5 in the post-treatment period. The analysis is implemented for two subsamples. First, only contiguous regions located at the border between both Baden-Württemberg and Bavaria. Secondly, all NUTS-3 regions located in both Federal States. Second, Each subsample contains three regressions using different weighting schemes: unweighted, population weights(Pop) based on number of inhabitants in 2010, and weighting factors obtained from entropy balancing(Ebal) accounting for the growth rate of house prices in 2010. We also include quarterly unemployment changes from year to year in order to account for the general macroeconomic trend at the regional level. Regional fixed effects at the county level and time fixed effects are included. Standard errors are clustered on the Federal State times quarter level. *Standard errors* in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.1: Quarterly HPI growth rate in contiguous regions in Baden Württemberg and Bavaria



Source: Immobilienscout24.de, own calculations

Figure A.2: Quarterly HPI growth rate of all regions in Baden Württemberg and Bavaria



Source: Immobilienscout24.de, own calculations

References

Adelino, M., A. Schoar, and F. Severino (2015). House prices, collateral, and self-employment. *Journal of Financial Economics 117*(2), 288–306.

Bauer, T. K., S. Feuerschütte, M. Kiefer, P. an de Meulen, M. Micheli, T. Schmidt, and L.-H. Wilke (2013). Ein hedonischer immobilienpreisindex auf basis von internetdaten: 2007–2011. *AStA Wirtschafts-und Sozialstatistisches Archiv 7*(1-2), 5–30.

Beier, R., M. Krueger, and M. Schaefer (2017). Monthly balance sheet statistics - data report 2017-02. Technical report, Deutsche Bundesbank, Research Data and Service Centre (RDSC).

Boelmann, B. and S. Schaffner (2018). Fdz data description: Real-estate data for germany (RWI-GEO-RED). Advertisements on the internet platform immobilienscout24. Technical report, RWI Projektberichte.

Brunnermeier, M. and I. Schnabel (2015). Bubbles and central banks: Historical perspectives. Technical report, CEPR Discussion Papers.

Card, D. and A. Krueger (1994). Minimum wages and employment: A case study of the fastfood industry in New Jersey and Pennsylvania. *American Economic Review 84*, 772–793.

Chakraborty, I., I. Goldstein, and A. MacKinlay (2018). Housing price booms and crowding-out effects in bank lending. *The Review of Financial Studies 31*(7), 2806–2853.

Chakraborty, I., I. Goldstein, and A. MacKinlay (2020). Monetary stimulus and bank lending. *Journal of Financial Economics 136*, 189–218.

Cloyne, J., K. Huber, E. Ilzetzki, and H. Kleven (2019). The Effect of House Prices on Household Borrowing: A New Approach. *American Economic Review 109*(6), 2104–2136.

Cvijanović, D. (2014). Real estate prices and firm capital structure. *The Review of Financial Studies 27*(9), 2690–2735.

Dachis, B., G. Duranton, and M. A. Turner (2011). The effects of land transfer taxes on real estate markets: Evidence from a natural experiment in toronto. *Journal of economic Geography 12*(2), 327–354.

Deutsche Bundesbank (2018). *Financial Stability Review 2018*. Frankfurt a.M., Germany.

Dinkel, M. and B. Kurzrock (2012). Asking prices and sale prices of owner-occupied houses in rural regions of germany. *Journal of Interdisciplinary Property Research 13*(1), 5–25.

Farhi, E. and J. Tirole (2012). Bubbly liquidity. *The Review of Economic Studies 79*(2), 678–706.

Favara, G. and J. Imbs (2015). Credit supply and the price of housing. *American Economic Review 105*(3), 958–92.

Fritzsche, C. and L. Vandrei (2016). The german real estate transfer tax: Evidence for single-family home transactions. Technical report, ifo Working paper.

Gan, J. (2007). The real effects of asset market bubbles: Loan-and firm-level evidence of a lending channel. *The Review of Financial Studies 20*(6), 1941–1973.

Gebhardt, H. (2008). *Geographie Baden-Württembergs: Raum, Entwicklung, Regionen*, Volume 36. W. Kohlhammer Verlag.

Gerlach, S. and W. Peng (2005). Bank lending and property prices in hong kong. *Journal of Banking & Finance 29*(2), 461–481.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift (2018). Bartik instruments: What, when, why, and how. Technical report, National Bureau of Economic Research.

Gouriéroux, C. and A. Laferrère (2009). Managing hedonic housing price indexes: The french experience. *Journal of Housing Economics 18*(3), 206–213.

Grodecka, A. (2020). On the Effectiveness of Loan-to-Value Regulation in a Multiconstraint Framework. *Journal of Money, Credit and Banking 52*(5), 1231–1270.

Hainmueller, J. (2012). Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. *Political Analysis 20*(1), 25–46.

Heinemann, F., E. Janeba, C. Schröder, and F. Streif (2016). Fiscal rules and compliance expectations – Evidence for the German debt brake. *Journal of Public Economics 142*, 11–23.

Hill, R. J., M. Scholz, et al. (2014). Incorporating geospatial data in house price indexes: a hedonic imputation approach with splines. *Graz Economics Papers 5.*

Himmelberg, C., C. Mayer, and T. Sinai (2005). Assessing high house prices: Bubbles, fundamentals and misperceptions. *Journal of Economic Perspectives 19*(4), 67–92.

Holly, S., M. H. Pesaran, and T. Yamagata (2010). A spatio-temporal model of house prices in the USA. *Journal of Econometrics 158*(1), 160–173.

Hott, C. (2011). Lending behavior and real estate prices. *Journal of Banking & Finance 35*(9), 2429–2442.

Huang, R. (2008). The real effect of bank branching deregulation: Comparing contiguous counties across US state borders. *Journal of Financial Economics 87*, 678–705.

Justiniano, A., G. E. Primiceri, and A. Tambalotti (2019, June). Credit supply and the housing boom. *Journal of Political Economy 127*(3), 1317 – 1350.

Keys, B. J., T. Mukherjee, A. Seru, and V. Vig (2010). Did securitization lead to lax screening? Evidence from subprime loans. *The Quarterly Journal of Economics 125*(1), 307–362.

Koetter, M. (2019). Lending effects of the ECB's asset purchases. *Journal of Monetary Economics*, forthcoming.

Koetter, M. and A. Popov (2020). Political Cycles in Bank Lending to the Government. *Review of Financial Studies*, forthcoming.

Kopczuk, W. and D. Munroe (2015). Mansion tax: the effect of transfer taxes on the residential real estate market. *American economic Journal: economic policy 7*(2), 214–57.

Mense, A., C. Michelsen, and K. Kholodilin (2018). Empirics on the causal effects of rent control in germany.

Mian, A. and A. Sufi (2009). The consequences of mortgage credit expansion: Evidence from the US mortgage default crisis. *The Quarterly Journal of Economics 124*(4), 1449–1496.

Mian, A. and A. Sufi (2011). House prices, home equity-based borrowing, and the US household leverage crisis. *American Economic Review 101*(5), 2132–56.

Mian, A. and A. Sufi (2018). Finance and Business Cycles: The Credit-Driven Household Demand Channel. *Journal of Economic Perspectives 32*(3), 1–30.

Peek, J. and E. S. Rosengren (2000). Collateral damage: Effects of the japanese bank crisis on real activity in the united states. *American Economic Review 90*(1), 30–45.

Petkova, K. and A. J. Weichenrieder (2017). Price and quantity effects of the german real estate transfer tax.

Reinhart, C. M. and K. S. Rogoff (2008). Is the 2007 US sub-prime financial crisis so different? An international historical comparison. *American Economic Review 98*(2), 339–344.

Rodnyansky, A. and O. M. Darmouni (2017). The effects of quantitative easing on bank lending behavior. *Review of Financial Studies 30*(11), 3858–3887.

Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung (2013). Gegen eine rückwärtsgewandte Wirtschaftspolitik - Jahresgutachten 2013/14. Wiesbaden.

Saiz, A. (2010). The geographic determinants of housing supply. *The Quarterly Journal of Economics 125*(3), 1253–1296.

Stock, J. and M. Yogo (2005). *Asymptotic distributions of instrumental variables statistics with many instruments*, Volume 6. Chapter.

Monitoring Real Estate Markets using Lag-Free House Price Indices *

Antonios Mavropoulos†

Abstract

House price cycles are usually larger than business cycles. Nevertheless, transitory shocks on the housing market can lead to adverse short-term effects and negative externalities. Therefore, it is important to monitor house prices in a frequent and granular fashion. In this paper, I construct lag-free house price indices across 14 EMU countries using web-scraped data and show that the data produce reliable lag-free indices that supplement the 1 to 2 quarters lag of Eurostat house price indices across Europe. Moreover, the high frequency, detailed house price indices enable the early detection of over-heated markets from the country level (NUTS1) to a regional (NUTS3) geographical scale. The paper offers a quarterly and geographically granular monitoring tool of housing markets across Europe which can be useful for researchers and policy-makers alike.

Keywords: House Price Indices, Lag-free monitoring, Web-scraped data
JEL Codes: R30; R31; R32

*I would like to thank Michael Koetter and Huyen Nguyen for their guidance and the participants of the IWH Doctoral and Brown bag seminars for their constructive comments.

†Halle Institute for Economic Research (IWH), E-mail: antonios.mavropoulos@iwh-halle.de.

1 Introduction

Real estate, both the rental and sales market has always been a critical part of the economy and largely influences households' wealth and welfare (Campbell and Cocco (2007); Piketty and Zucman (2014); Berger et al. (2017)). House price fluctuations not only affect household decisions but are also interdependent with the financial markets (Kaplan et al. (2015); DeFusco (2018); Cloyne et al. (2019)). They can be a source of financial instability as described in the seminal papers by Attanasio et al. (2009); Mian and Sufi (2011) and Mian et al. (2013) as well as act as a financial accelerator mechanism of extensive credit supply that funnels through house prices (Gerlach and Peng (2005); Mora (2008) and Hott (2011)).

From a macro perspective, house prices can be a propagator of shocks (Holly et al. (2010)), as they can affect the transmission of monetary policy (Aoki et al. (2004); Del Negro and Otrok (2005); Vansteenkiste and Hiebert (2011)) and determine the transfusion of shocks in relevance to the local real estate market (McCarthy and Peach (2004); Saiz (2010) and Gyourko et al. (2013)). By any means, house prices affect our economic and social structure. Therefore, it is of high importance to be able to measure house prices and their fluctuations as precisely as possible. Furthermore, policy makers, economists and supervising authorities need a tool that enables them to react in a timely fashion in order to prevent unintended house price movements and adverse effects.

Common house price monitoring tools are elaborate statistical measures of the housing markets but usually lack in terms up-to-date and geographically granular information. This is due to the costly data collection of house prices and a time intensive harmonizing process of the differently sourced information. Can we therefore monitor house prices in a more timely fashion? The scope of this paper is to provide an alternative data source which allows lag-free monitoring of housing markets across EMU countries. The paper proposes an addition to the already existing country-specific indices published by each country's respective statistical office, which also allows for research of housing markets on a granular level, from country (NUTS1) to regional (NUTS3) level, as well as intra-market analysis of the different sub-segments of the housing markets.

In this paper, I construct house price indices from a plethora of web-scraped data across 14 EU countries and compare them to the respective Eurostat indices. The quarterly Eurostat indices range up to Q4:2019. I aim to show that the quarterly, lag-free series fill the usual 3-4 month lag of the published indices, and are able to follow similar trends. Another advantage of the data is that harmonized indices across all countries are produced using a rather parsimonious hedonic specification, which nevertheless includes most of the main housing price determinants. The national statistical offices across European countries follow similar procedures (as I discuss in extent in the Appendix) in line with guidelines from Eurostat, that should account for the variation in the nature of data collected across each country.

The accuracy of Eurostat's source data is monitored by assessing the methodological soundness of price and weight sources and the adherence of each member country's statistical institute to Eurostat's methodological recommendations. The variety of data sources though makes the harmonization process liable to the intrinsic data discrepancies[1]. In contrary, I collect the data from the larger online real-estate portals which ensure analogous data sources of online adverts.

Another potential source of bias can be related to the price concept in use (transaction price, appraisal, offer price, etc.). Some countries such as Iceland and Norway generally use transaction prices. Whereas this is in line with the objective of a house price index; that is to measure the evolution of actual transaction prices as realized by buyers and sellers, even transaction prices (as recorded in administrative registers, for instance) may, in some cases, be subject to an under-reporting bias. To account for house price source homogeneity, I collect information only on asking prices. Whereas transaction prices and asking prices might differ, Dinkel and Kurzrock (2012) show for rural areas in Rhineland-Palatine, besides a constant price markup between asking and transactions prices there are no systematic differences. Furthermore, Han and Strange (2016) show that asking prices are many times equal to the transaction price of a house and Shimizu et al. (2016) investigate asking and transaction prices in different stages of the buying/selling process to conclude that, when quality of housing is controlled for, asking and transaction prices are comparable. The advantage of the scraped data over transaction data is also the frequent and swift reporting. Information on all listings is reported and updated at a monthly frequency whereas with transactions, the data are subject to time lags. That enables the monitoring of real estate price fluctuations on the spot. Overall, I propose that by ensuring homogenized data sources and variable comparability, I allow for an easier and less costly harmonization process.

I illustrate that, in most cases, the indices are highly comparable with the Eurostat indices, allowing for timely policy interventions and research on the housing markets across Europe. I furthermore decompose the indices based on Eurostat regional typologies to illustrate the different trends within each Country which provides insights on the regional housing markets.

The paper contributes to the empirical literature on house prices as well as to policy-making by creating reliable, lag-free and cross-country harmonized house price indices using novel web-scraped data. Furthermore, it offers a tool to analyze geographically granular regions and investigate housing market sub-segments. Since hedonic models are "pricey" in the sense that they require a plethora of observations, I collect millions of listings that can cover to a large extent both urban and rural regions and overcome selection bias, which is a common problem in house price index construction, especially

[1] Administrative data, bank (mortgage) data, construction companies, real estate agents, etc.

the ones who use repeat sales models. Furthermore, the data collection process allows for the creation of a comparable and up-to-date price to rent ratio, a useful indicator of future price movements and of real estate market imbalances and over-heatings (see e.g.Shiller (2006) and Himmelberg et al. (2005)).

2 A taxonomy of real estate price indices

Over the past three decades, the hedonic-based regression approach has been extensively utilized in the housing market literature to investigate the relationship between house prices and housing characteristics. The primary reasons for such extensive application are analyzing household demand based on the intrinsic aspects of housing as well as constructing housing price indices which allow for a quality controlled monitoring of real estate markets (see, for example, Can (1992); Sheppard (1999))[2]. The relatively recent application of hedonic indices is well justified. Even though hedonic methodologies can adjust for both sample and quality mix changes, they require a sufficiently detailed number of available characteristics[3] as well as a relative large sample of observations. On the downside, rather free choices of the sets of characteristics that go into the empirical models can lead to varying estimates and a harder reproducibility of results. In response to that, In this paper, I argue that a EU harmonized construction of the house price indices where the same set of information is used, is essential for monitoring and comparing European housing markets.

The main source of house price indices information in EU is the Eurostat Housing statistics. It is a collection of indices across EU member countries, with data which derive from the respective statistical offices of each country. These statistical offices are responsible for the collection of the data, as well as the construction of the indices. Harmonization and quality checks are then performed by Eurostat[4]. However, these indices are constructed using various data sources. In some cases, the indices are constructed by government departments and not statistical offices (Ireland, France, Spain and the UK (Extended discussion can be found here: de Haan and Diewert (2011))). The derived indices are then collectively published on annual and quarterly frequency at the NUTS1 geographical level by Eurostat.

2.1 Hedonic vs Repeat-Sales Models

The two main hedonic house price index methodologies are time-dummy and characteristics based imputation methods. The first, used for example by Follain Jr and Malpezzi (1981), de Haan (2004), Haughwout et al. (2008), Diewert et al. (2009) and Bauer et al.

[2]For extensive analysis on hedonic indices see Triplett (2004); Hill and Melser (2008); Hill (2011).

[3]However, having information on location, type of property, its age and its floor space may explain most of the variation in the price.

[4]https://ec.europa.eu/eurostat/documents/7590317/0/Technical-Manual-OOH-HPI-2017/

(2013) is a relatively simple approach where the evolution of prices is captured by normalized time dummy coefficients. Its flexibility though comes in the expense of degrees of freedom. Another downside is that it assumes that covariate effects (i.e. house characteristics) are homogeneous across regions. The characteristic based imputation method allows for covariate heterogeneity across regions at a smaller cost. In this method, prices are imputed from a hedonic stratified model (see Gouriéroux and Laferrère (2009)). The imputed prices are calculated over a reference period, which usually is the first couple of years of data. The development of this reference sample in the following years is then captured by demeaning the actual attributes and price of future listings. By doing so, the future sample is synthetically similar to the one in the reference period. Stratification allows for the capture of homogeneous house price effects.

Hedonic models are however subject to criticism arising from potential problems relating to fundamental model assumptions and estimation such as the identification of supply and demand, market disequilibrium, the selection of independent variables, the choice of functional form of hedonic equation and market segmentation. These problems have been of great concern in the literature (see Malpezzi (2003), Fan et al. (2006)). The shortfalls of the hedonic methodologies are overcome by the level of detail they incorporate, and the fact that they can exploit large data and qualitative changes between different samples. That is the main reason that hedonic methodologies have been used quite extensively both in micro and macro-economic applications.

The most common house price monitoring tools in the US are the Case and Shiller house price indices of repeat sales models[5] and the OFHEO house price index, which is constructed using monthly, single family mortgage transactions from Fannie Mae and Freddie Mac. The data in the above indices are transactions of dwellings and thereby do not cover the housing market to a large extent (with the exception of the largest metropolitan cities). Furthermore, there is a lag between the transaction date and the input date of the data (as described for example in Mueller and Yannelis (2019)).

The upside of repeat sales methodologies is in their simplicity, as they do not require any other characteristic than the address of the residential unit. Furthermore, they are easy to track and reproduce. The downsides of these indices are that they are computed using a fixed sample of residential units from their observed transaction prices over time. The physiology of these data does not allow for a big number of observations. Furthermore, it does not separate house price changes from depreciation and renovations. Also, lemons are likely to be more frequently sold. By eliminating houses which exchange ownership within a short period leads to an even smaller sample size. Lastly, these data do not cover the residential market to the extent that the web-scraped housing price data do, as it is even less likely to observe numerous transactions on geographically smaller– i.e.

[5] NUTS1 or 20-city, 10-city and 20 individual metropolitan areas indices computed at a quarterly frequency.

rural areas. These issues are offset using the high volume detailed data I collect. In many cases, I am able to stratify the hedonic models at the postal code level, a geographical scale in which prices are assumed to be homogeneous.

Due to the nature of the web-scraped data, I am able to use hedonic models for the computation of the indices. The large number of observations, as well as the quality defining characteristics which are included, provide an alternative to the repeat sales methodologies that have been commonly used, for example in the OFHEO[6] and Case and Shiller house price indices[7].

3 Data Description

In this section, I provide a description of the nature and technical aspects of the web-scraped data.

Due to the lack of a real estate portal that covers the European housing market ,I collect web-scraped data from 14 EMU country specific websites. Table 1 reports the countries as well as the respective data sources. The websites are chosen under two criteria. First, they need to offer a representative sample of the country's real estate market and second, they need to allow data collection using scraping tools.

– Table 1 here–

The datasets entail information of real estate offerings on prices, rents as well as various quantitative and qualitative characteristics that determine the value of a property. There are incremental additions to the data each month, which allows both the construction of monthly indices as well. The dataset collection starts on July 2017.

I collect data on four segments of residential real estate markets. Namely, houses for sale or rent as well as apartments for sale or rent. Information on commercial housing is not collected, neither on land sales. The main categories in terms of observation count are usually houses for sale and apartments for rent. Apartments for sale and houses for rent are smaller segments of the real estate markets but their inclusion provides necessary insights on the composition of the regional real estate markets. In this paper, I use only information on sales prices, while controlling whether the dwelling is a house or an apartment.

Each dataset contains a plethora of variables. I distinct these variables into primary and secondary ones. For example, prices, square meters, type of housing and location are classified as "primary" variables. These variables allow the construction of a harmonized EMU Area dataset. Due to the heterogeneity of the sources, a different set of information

[6]The former US Office of Federal Housing Enterprise Oversight (OFHEO) and the Federal Housing Finance Board (FHFB) later established the FHFA in 2008.

[7]Applications of repeat sales models can be found in Case and Shiller (2003); Harding et al. (2007); Clapp and Giaccotto (1998).

can be collected. Country specific house price indices can be created with the additional use of other secondary variables. These variables can for example be the number of rooms, number of floors and energy class. For this paper, I restrict into harmonized EMU indices as I want to allow not only real-time monitoring of house prices, but also econometrically sound comparison of house prices across different EMU countries.

In general, these listings are posted from the owners, who have to fill a website specific questionnaire illustrating several characteristics of the property. Each listing carries its own specific unique identification, therefore price changes across time can be observed. This allows for the creation of repeat sales indices under the trade-off of a lower sample size. Description variables are also reported, allowing for mining textual information that is not necessarily reported during the collection process. The description variables usually contain other qualitative traits as proximity to public transport, schools, hospitals and parks. Furthermore, there are variables portraying qualitative traits such as the presence of a balcony, a garage and fireplace among others. These are not used in the hedonic index construction, as a large proportion of the data usually lacks such information. Figure 1 illustrates the data collection across all countries.

– Figure 1 here–

Each month, incremental data are added to the already existing data pool. EMU area countries that are not illustrated here[8] are due to lack of comprehensiveness of the source data and/or lack of alternatives. Although the collection process for the majority of the websites started in February 2018, the data on Germany go back to July 2017. The missing data in the case of Ireland, Italy, Sweden and Switzerland can be overcome using imputation methods.

To understand the volume of information included in the data, I illustrate the amount of scraped observations by country, month and real estate market segment. Table 2 displays the summary statistics, observation count and house typology structure for each month of the data.

– Table 2 here –

Furthermore, each country has its own set of variables. Table 3 illustrates the variables included in each country's dataset. Columns 2-5 display housing market segmentation. The rest of the columns are ordered by significance. The most significant information when creating hedonic house price indices are size adjusted prices, number of rooms, residence type(i.e. apartment of house) and location. These information, with the exception of the British data[9], are available for all the datasets. For the construction of

[8]Belgium, Cyprus, Bulgaria, Croatia,, Czech Republic, Denmark, Hungary, Latvia, Malta, Romania, Slovenia and Slovakia.

[9]The size adjustment of the British data can only be accounted by bedrooms.

a harmonized EMU cross-country index, a rather parsimonious model needs to be used, where I abstract from the use of secondary variables, such as the presence of a balcony or a fireplace[10].

– Table 3 here –

I combine the above information to construct a visual representation of the quality of the datasets across countries. Below, in Figure 2 I illustrate data quality quadrants on sales and rentals respectively. The top right quadrant signifies the area where the best datasets in terms of number of available variables and number of collected obervations per month reside. I choose to report 15 quantiles instead of the exact observation number for visibility. The German, Italian, French and Spanish data contain many more observations than the average therefore datasets with fewer observations are bundled in the left side of the quadrant. Exact information of the observations per month can be seen in Table 2.

– Figure 2 here –

Due to the recurring nature of the data collection process, all the above graphs and tables give a representative image of the data at Q3:2019.

4 Harmonized Real Estate Price Indices

In this section, I illustrate the empirical methodology for the construction of the cross-country EMU harmonized index.

4.1 Empirical Approach

As outlined by Hill et al. (2014), hedonic models are ideal for the construction of quality-adjusted house price indices. Dwelling i should be adjusted by k dwelling-specific characteristics (e.g. size, year of construction etc.), $X_{k,i}$, as well as by its geographical location, which can captured by the strata, S_i (see e.g. Saiz, 2010). Analogue to Gouriéroux and Laferrère (2009), in the first stage I estimate quarterly hedonic price indices for each of the 14 EMU countrries in the data.

$$\ln P_{i,y,q} = \alpha_0 + \sum_{k=1}^{K} \alpha_k X_{k,i} + \sum_{s=1}^{S} \beta_s S_{s,i} + \sum_{y=1}^{2} \gamma_y Y_y + \sum_{q=1}^{4} \delta_q Q_q + \epsilon_{i,y,q}, \tag{1}$$

where $P_{i,y,q}$ denotes the asked price or rent per m^2 of dwelling i in year y and quarter q. The model contains an intercept, α_0 and a vector of housing characteristics with k elements $-X_{k,i}-$. Furthermore a vector equal to the number of strata s in the NUTS-3 county $-S_{s,i}-$ is included, as well as annual fixed effects $-Y_y-$ per year y and seasonal fixed

[10] A detailed description of each country's index is in the Appendix.

effects $-Q_q-$ in quarter q within the reference period. In the second stage, I estimate the price of a reference dwelling $-P_0-$, at mean values of the covariates denoted in equation (1) during the reference period:

$$\ln \hat{P}_0 = \hat{\alpha}_0 + \sum_{k=1}^{K} \hat{\alpha}_k \bar{X}_{k,0} + \sum_{s=1}^{S} \hat{\beta}_s \bar{S}_{s,0} + \sum_{y=1}^{Y} \hat{\gamma}_y \bar{Y}_{y,0} + \sum_{q=1}^{4} \hat{\delta}_q \bar{Q}_{q,0}. \quad (2)$$

In the third stage, I estimate the price of the reference dwelling in period τ by adjusting the observed price of dwelling i in period τ. Specifically, the differences in the characteristics between dwelling i and the reference dwelling are accounted given the average traits in the reference period. Whereas I omit year fixed effects, which are only specified within the reference period, quarterly indicators account for seasonality:

$$\ln \hat{P}_{i,\tau} = \ln P_{i,\tau} - \sum_{k=1}^{K} \hat{\alpha}_k (X_{k,i,\tau} - \bar{X}_{k,0}) - \sum_{s=1}^{S} \hat{\beta}_s (S_i - \bar{S}_{s,0}) - \sum_{q=1}^{4} \delta_q (Q_q - \bar{Q}_{q,0}). \quad (3)$$

The hedonic price index for period τ is derived from the average of adjusted estimated prices. The outcome index is a Laspeyres type index as seen below:

$$\hat{P}_\tau = \frac{1}{N} \sum_{i=1}^{N} \exp(\hat{P}_{i,\tau}) \quad (4)$$

4.2 Cross-country EMU Harmonized Index

The main goal of the data is to produce harmonized and comparable lag-free indices across EMU countries. To achieve that, I have to rely on a parsimonious regression model where the set of variables included across countries remains the same. Therefore, house prices go through the same qualitative and quantitative adjustments.

On the left hand side, house prices are standardized in euros[11]. I then adjust these prices by square meters,[12].

On the right hand side, I am constrained to use information available across all datasets. Table 3 illustrates a representation of what can be used for the creation of the EMU harmonized index. Fortunately, the hedonic index literature suggests a handful of explanatory variables that are of essential importance. These are, size, plot area, location and type of housing. Out of those, I do not use plot area, which is missing information from most of the datasets. The parsimonious version of the model then uses the type of housing (be it a house or an apartment) and stratifies the listings at the city

[11] In the case of a different currency like in England, Scotland, Norway, Sweden and Switzerland, I use monthly exchange rates to convert prices in euros.

[12] The English and Scottish data do not include square meters, therefore I can either exclude or approximate square meter information by adjusting price per room.

level[13].

Another requirement for the index construction is that at least one year, or 12 months of observations are required in order to construct a reference period that accounts for seasonality. Usually the first twelve months of each country should serve as the reference period. Since the data collection does not start simultaneously for all countries, for the sake of homogeneity, I use 2019 as a reference period, since it is the year with the least missing cross sections among the countries[14]. Due to the large number of different datasets, I need to resort to a rather brute method in order to treat all datasets equally. I therefore drop the bottom and top 1% of each country, in order to reduce the exposure to outliers. City strata and regional fixed effects are also included, as well as the interaction of these fixed effects with the type of housing. Standard errors are clustered at the city times month level to allow for serial correlation.

5 Index Comparison

In this section I provide visual evidence of index comparability among the member countries in the sample data. Since information both on sales and rentals is collected, I have the opportunity to construct indices for different segments of the housing market. This allows us to create price to rent ratio indicators, as well as investigate the dynamics between the two segments of the housing market. Since Eurostat and the respective EU country statistical offices publish house price indices based on the transactions of sold houses, for this paper, I only use indices deriving from the sales data[15]. In order to make the indices comparable to the quarterly ones from Eurostat, monthly information is aggregated to quarters in order to construct quarterly equivalent indices. Figure 3 illustrates comparison of the EMU Country indices The derived indices are then compared to three different Eurostat indices, namely an index using the total number of houses, and indices on new and old dwellings.

For visualization, I normalize the indices for each country to 100, at the beginning of each respective EMU country index. For example, the Austrian indices are normalized to 100 at 2018:Q1, when the data collection process for Austria started.

Furthermore, the Appendix discusses country specific index methodologies and provides essential details on the handling and regional typologies of the data.

– Figure 3 here–

[13] Postal code information is not observed for all datasets. Therefore city information is either available or can be derived from postal code information.

[14] A new and better rendition of the Italian data and Scotland for example start in 2019.

[15] The Eurostat indices are constructed using cash or mortgage transaction and cover only the sales market, therefore rents are excluded.

5.1 Urban and Rural Trends

Potential over-representation of specific regions can always be present when constructing house price indices. Furthermore, different regions can follow different trends in response to regional shocks such as investment on the housing markets which usually takes place in urban regions, or land scarcity (Saiz (2010)). Although these two issues combined can partially be overcome by proper weighting schemes, in this section I illustrate the trends between rural, urban and mid-urban regions and visually inspect the convolution of indices with respect to geographical topography.

The regional segregation is done according to Eurostat regional typology classification standards. More specifically, NUTS3 regions are classified into the above typologies. The calculation of these indices is carried through exclusive hedonic regressions instead of Pooled OLS regressions with regional typology dummies as an additional explanatory variable. The reason for this is two-fold. First, by adding another variable I violate the harmonized indices which are produced in the previous section and second, by pooling the data together I allow the different sub-samples to affect the outcomes of the other ones, which is liable to regional representation. Although this is important to allow when constructing national NUTS1 indices, it can be considered a downside for regional indices.

Figure 4 illustrates the comparison. There are two less countries represented here, as Luxembourg and Norway cannot be decomposed into the aforementioned regional typologies in line with Eurostat standards.

– Figure 4 here–

Finally, in addition to the visual comparison between the indices, both on the national and regional level, I illustrate correlations between the portrayed indices of Eurostat and the ones this paper constructs, Table 4 in line with Fernald (2014). This table quantifies the relevance of the indices, compared to the European standard. Although there are some cases where the correlation coefficients do not converge, for most of the countries the indices evolve similarly and have a high and positive correlation coefficient. Small differences can be attributed to the aforementioned fact that hedonic indices are liable to the sample of data, methodology choice and harmonization processes, but eventually the indices successfully capture the past trends of Eurostat's house price indices.

– Table 4 here–

6 Conclusion

In this paper, I present a novel way of monitoring house prices in a lag-free and less costly, in terms of data collection fashion. I construct quarterly house price indices using web-scraped data for 14 EU countries. In order to test the fit of the indices, I compare the outcome indices with the Eurostat quarterly house price indices. The high correlation of the indices with the published Eurostat indices, allows researchers as well as policy makers to investigate and monitor the latest trends in housing markets from NUTS1 to NUTS3 regions. On top of the 1 to 2 quarters of timely information that the indices carry, I allow for the creation of harmonized indices at an EMU scale, using a parsimonious hedonic model.

I furthermore show that regional typology differences play an important factor in understanding house price trends. This calls and allows for application of granular heterogeneous macro-prudential measures across regions within each country. Whereas some indices perform lesser than others in terms of compatibility with the Eurostat Indices, I illustrate that the differences are mostly driven by regional differences and sample compositions. The paper also illustrates that the monitoring of house prices can be done using online adverts of asking prices, which allows to timely interventions and up-to-date research on the housing markets across Europe.

Tables

Table 1: Real estate portals

Austria	`https://www.wohnnet.at`	**Luxembourg**	`https://www.immotop.lu`
England	`https://www.rightmove.co.uk`	**Netherlands**	`https://www.jaap.nl`
Estonia	`https://www.kv.ee`	**Norway**	`https://www.finn.no`
Finland	`https://www.etuovi.com`	**Poland**	`https://www.domy.pl`
Germany	`https://www.immobilienscout24.de`	**Portugal**	`https://www.immovirtual.com`
Ireland	`https://www.daft.ie`	**Scotland**	`https://www.s1homes.com`
Italy	`https://www.casa.it`	**Sweden**	`https://www.hemnet.se`
Lithuania	`https://www.ober-haus.lt`		

This Table illustrates the respective websites that the data are collected from for each country in the dataset.

Table 2: Summary statistics and observation count

Country	Mean	Median	Standard Dev.	Min	Max	N(per month)	N(houses)	N(apartments)
AU	3,942.35	3,816.97	1,938.91	3,917.24	4,583.77	19350	5288	14074
DE	2,285.96	1,903.23	8,835.83	2,226.07	10,708.22	126772	84115	46538
EE	1,307.14	1,252.00	857.19	1,239.28	2,723.00	13042	3952	9548
FI	2,590.87	2,245.45	1,590.95	1,239.67	2,830.26	42793	676	42157
IE	3,047.09	2,535.71	1,519.73	2,500.68	4,560.19	12529	9984	2552
IT	1,609.82	1,363.64	1,065.85	1,584.23	1,631.63	108134	88638	19503
LT	1,733.83	1,228.57	1,575.49	1,591.44	2,519.21	407	259	202
LU	3,013.78	2,285.71	2,163.61	2,879.67	6,816.91	21944	10630	11323
NL	3,242.11	2,537.74	27,010.59	3,242.11	16,439.86	52815	46902	5417
NO	2,263.26	2,472.43	8,055.35	3,244.67	16,252.06	12112	6551	4853
PL	1,570.18	1,425.62	765.08	1,257.40	1,969.36	7204	2341	5057
PT	1,701.97	1,225.00	1,355.50	504.12	2,771.18	73025	36776	44126
SC	61,064.09	57,500.00	27,187.49	59,744.11	61,890.02	8469	3249	5222
SE	4,521.88	3,010.50	5,834.86	2,619.70	5,747.67	15586	8525	9507
UK	1.08e+06	700000.00	1.31e+06	990345.38	1.21e+06	19037	16200	2841

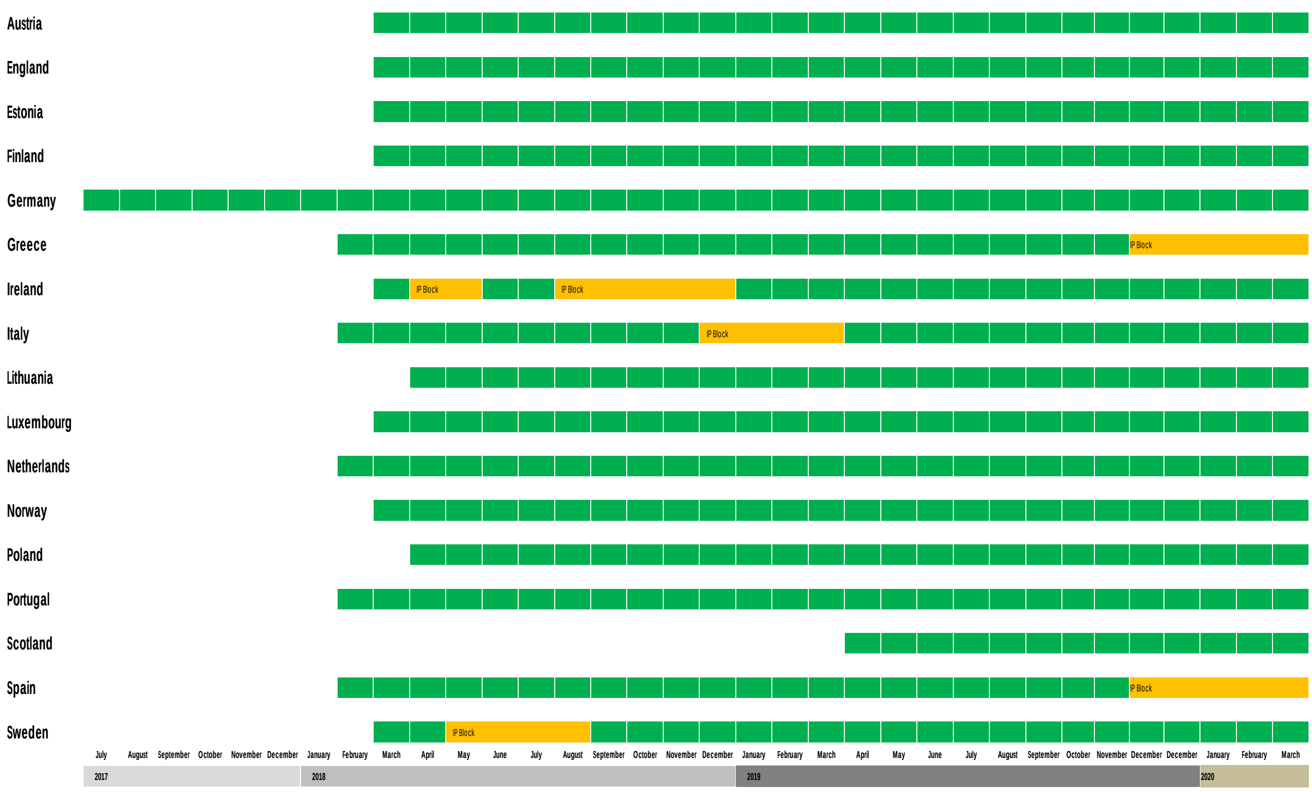
This Table illustrates the summary statistics for our EMU dataset. We illustrate the mean, median, min and max values for prices per square meter. Furthermore, we portray the average number of observations scraped until March, 2019 and distinguish between houses and apartments. Prices for Scotland and England, which together comprise the United Kingdom, are adjusted per room and not per square meter.

Table 3: Country specific variable availability

Country	Buy House	Buy Apartment	Rent House	Rent Apartment	URL	ID	Date	Title	Price	Sqm.	Rooms	Bathrooms	Bedrooms	NUTS3	City	Area	Postal	Address	Residence Type	Description	Floor (of total)	Construction Year
Austria	X	X	X	X	X	X	X	X	X	X	X	-	-	X	X	-	X	-	X	-	-	-
England	X	X	X	X	X	X	X	X	X	-	-	-	X	X	X	-	-	X	X	-	-	-
Estonia	X	X	X	X	X	X	X	X	X	X	X	-	-	X	X	-	-	-	X	-	-	-
Finland	X	X	X	X	X	X	X	X	X	X	X	-	-	X	X	-	-	-	X	-	-	-
Germany	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Ireland	X	X	X	X	X	X	X	X	X	-	-	-	X	X	X	X	-	X	X	-	-	-
Italy	X	X	X	X	X	X	X	X	X	X	X	X	-	X	X	-	-		X	-	-	X
Lithuania	X	X	X	X	X	X	X	X	X	X	X	-	-	X	X	-	-	-	X	-	-	X
Luxembourg	X	X	X	X	X	X	X	X	X	X	X	X	-	X	X	-	-	-	X	-	-	X
Netherlands	X	X	X	X	X	X	X	X	X	X	-	-	-	X	X	-	X	X	X	-	-	-
Norway	X	X	X	X	X	X	X	X	X	X	-	-	-	X	X	-	X	X	X	-	X	X
Poland	X	X	X	X	X	X	X	X	X	X	-	-	-	X	X	-	-	-	X	-	-	X
Portugal	X	X	X	X	X	X	X	X	X	X	-	-	-	X	X	X	-	-	X	-	-	-
Scotland	X	X	X	X	X	X	X	X	X	-	X	-	-	X	X	-	-	-	X	-	-	-
Sweden	X	X	-	-	X	X	X	X	X	X	X	-	-	X	X	X	-	X	X	-	-	-

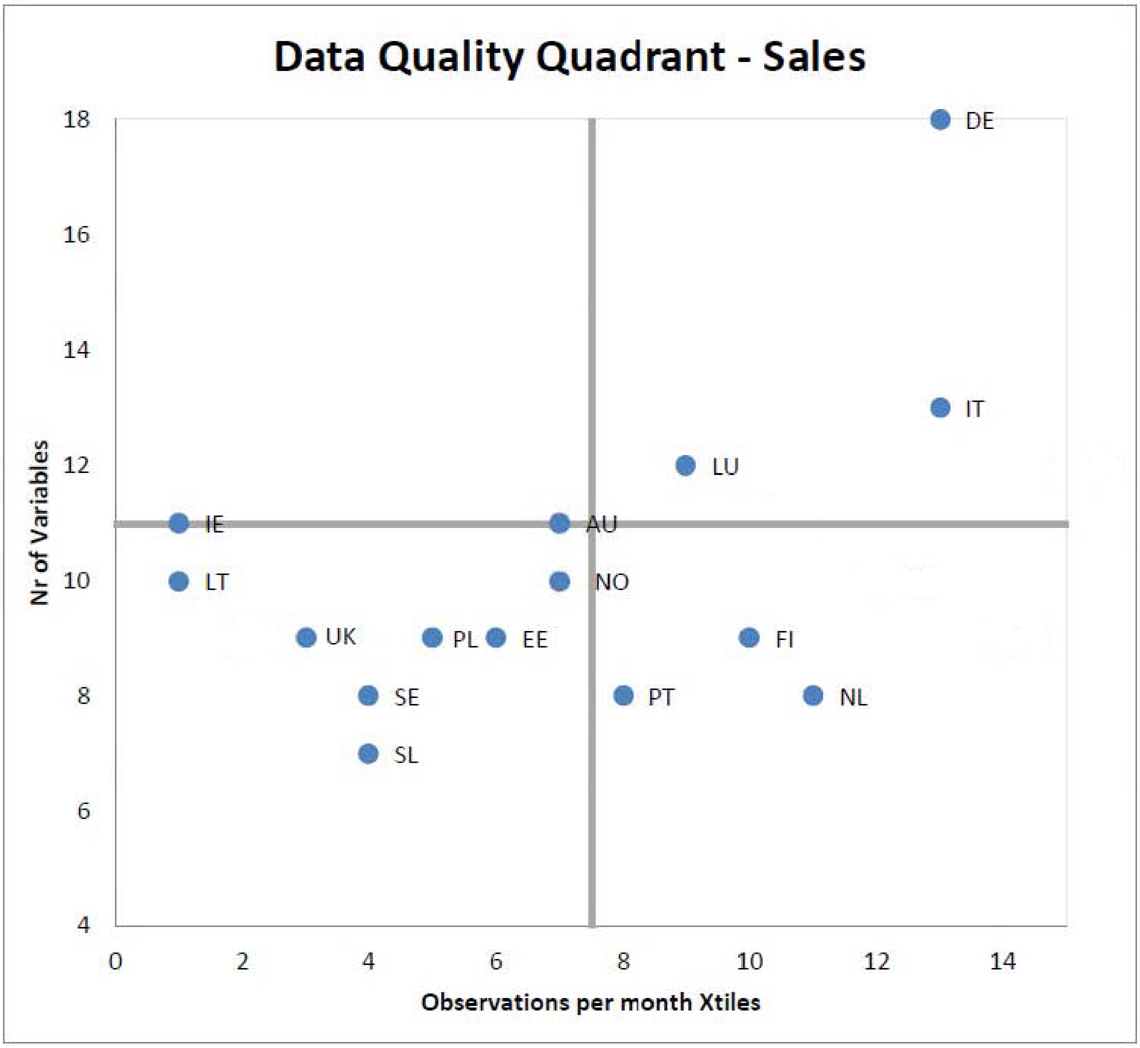
This Table shows the respective variable availability for each dataset in our dataset, as well as the respective housing categories covered in the data.

Figure 1: Scraping timeline



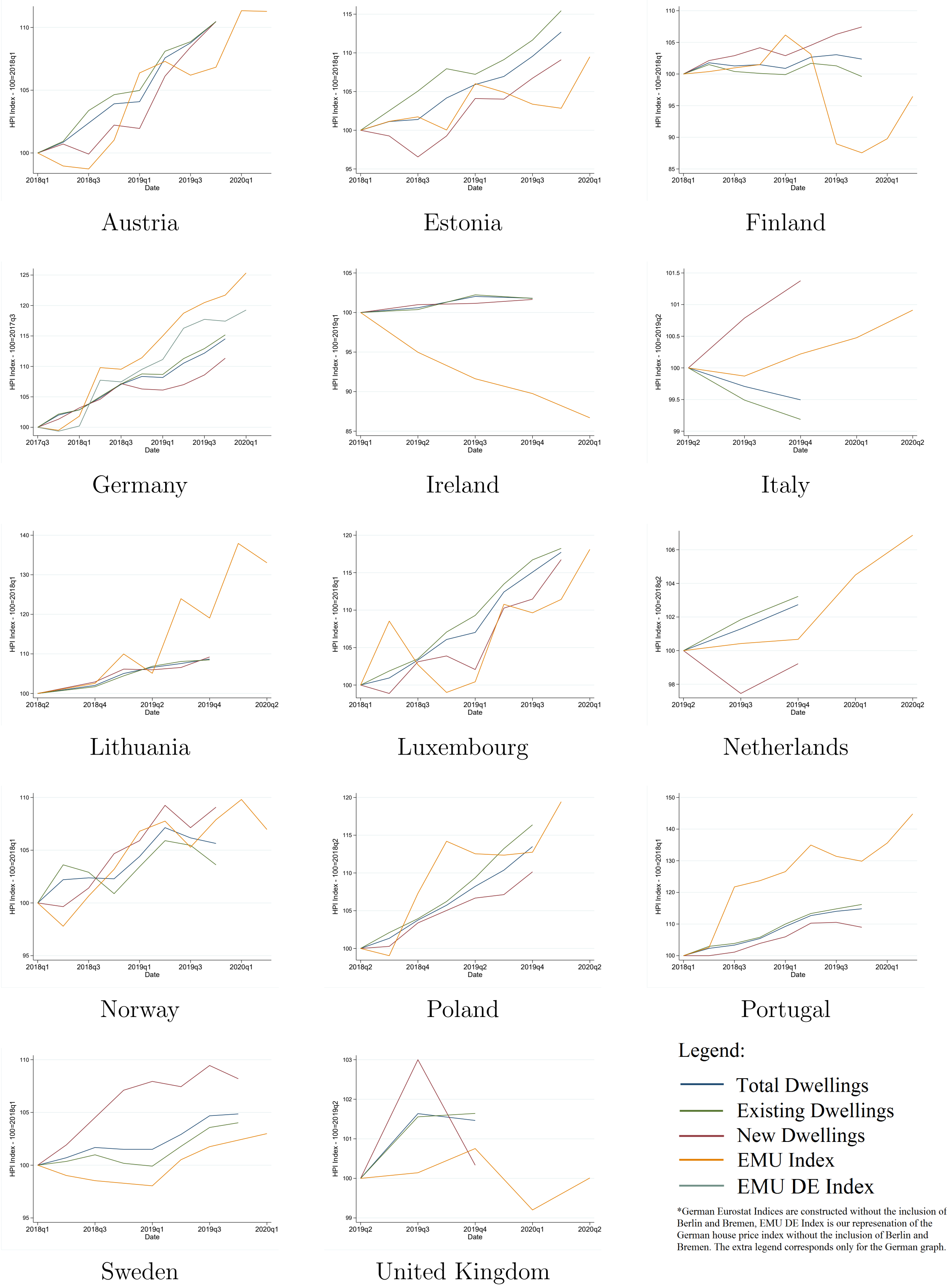
This figure illustrates the current state (Q1:2020) of the data collection process and availability. Spain and Greece are illustrated but are not included due to collection issues.

Figure 2: Quality quadrants



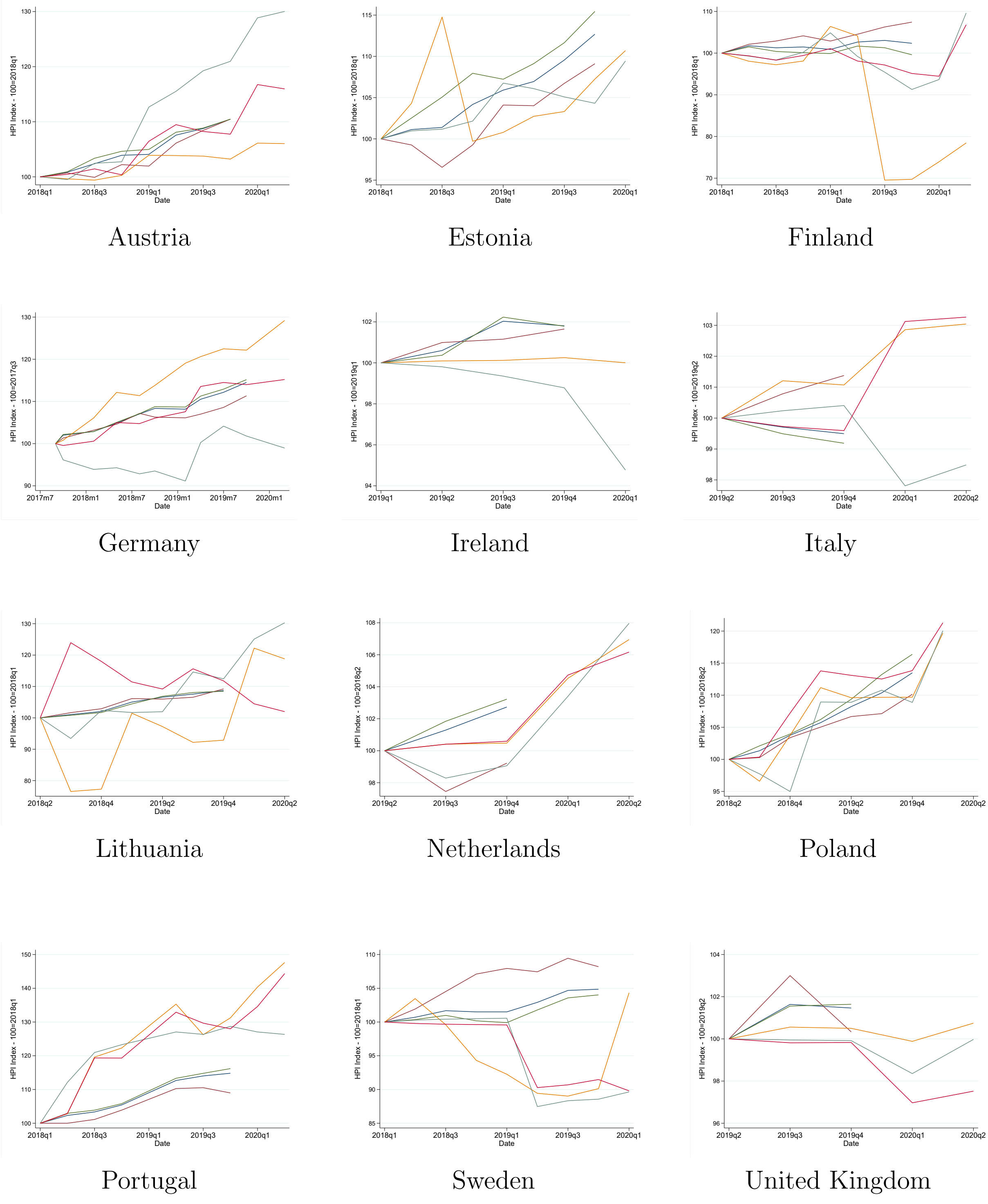
This figure illustrates quality quadrants on each country specific dataset within our data. On the X-axis we document the extensive margin of the data (i.e. the amount of observations covered in each monthly scrape). For visual reasons we choose to show xtiles instead of the actual observations, because the German dataset is far more extensive and thus bundles the datasets with the lesser observations per month together. On the Y-axis, we illustrate the number of variables included in each dataset. The top-right quadrant indicates data of the highest quality, but discretion is needed, as the German dataset pushes the barrier and forces other datasets to fall into lesser quadrants.

Figure 3: Index comparison



This figure illustrates the index comparison between the purblished indices of Eurostat and our own. Specifically, three types of Eurostat indices (Total sample, new dwellings and existing dwellings) are compared to the EMU index we construct.

Figure 4: Rural and urban index comparison



Legend:

- Total Dwellings
- Existing Dwellings
- New Dwellings
- EMU Urban
- EMU Rural
- EMU Mid-urban

This figure illustrates the index comparison between the purblished indices of Eurostat and our own. Specifically, three types of Eurostat indices (Total sample, new dwellings and existing dwellings) are compared to the EMU index for urban, rural and mid-urban regions we construct.

Table 4: Correlation matrices

Country	Eurostat Index	(1) NUTS1 Index	(2) HPI - Rural	(3) HPI - Urban	(4) HPI - Mid-Urban
Austria	HPI - total	0.850**	0.951***	0.797*	0.870**
	HPI - new	0.806*	0.922**	0.749*	0.817*
	HPI - existing	0.852**	0.945***	0.802*	0.874**
Estonia	HPI - total	0.542	-0.002	-	0.736*
	HPI - new	0.582	-0.198	-	0.729*
	HPI - existing	0.468	0.129	-	0.679
Finland	HPI - total	-0.559	-0.592	-0.602	-0.710*
	HPI - new	-0.699	-0.699	-0.757*	-0.816*
	HPI - existing	0.074	0.017	0.079	-0.033
Germany	HPI - total	0.970***	0.406	0.952***	0.953***
	HPI - new	0.926***	0.286	0.915***	0.883***
	HPI - existing	0.972***	0.416	0.954***	0.958***
Ireland	HPI - total	-0.940	-0.854	0.787	-
	HPI - new	-0.978*	-0.885	0.951*	-
	HPI - existing	-0.887	-0.817	0.717	-
Italy	HPI - total	-0.548	-1.000**	-0.860	0.995
	HPI - new	0.556	1.000*	0.855	-0.994
	HPI - existing	0.504	-0.999*	-0.885	0.999*
Lithuania	HPI - total	0.832*	0.803*	0.350	0.016
	HPI - new	0.785	0.746	0.304	0.076
	HPI - existing	0.838*	0.819*	0.354	-0.003
Luxembourg	HPI - total	0.670	-	-	-
	HPI - new	0.694	-	-	-
	HPI - existing	0.647	-	-	-
Netherlands	HPI - total	0.984	-0.520	0.912	0.970
	HPI - new	-0.432	0.962	-0.639	-0.494
	HPI - existing	0.998*	-0.614	0.953	0.991
Norway	HPI - total	0.841**	-	-	-
	HPI - new	0.969***	-	-	-
	HPI - existing	0.523	-	-	-
Poland	HPI - total	0.824*	0.781*	0.808*	0.864*
	HPI - new	0.777*	0.867*	0.915**	
	HPI - existing	0.781*	0.777*	0.767*	0.825*
Portugal	HPI - total	0.872**	0.843*	0.876**	0.900**
	HPI - new	0.881**	0.797*	0.877**	0.908**
	HPI - existing	0.866**	0.848*	0.872*	0.894**
Sweden	HPI - total	0.784*	-0.872**	-0.803*	-0.883**
	HPI - new	0.320	-0.603	-0.900**	-0.645
	HPI - existing	0.902**	-0.880**	-0.644	-0.875**
United Kingdom	HPI - total	0.570	-0.887	1.000***	-1.000*
	HPI - new	-0.241	-0.239	0.660	-0.639
	HPI - existing	0.681	-0.944	0.990	-0.993

This table illustrates the correlation matrices between the published Eurostat indices and the EMU ones we construct. In columns (1-4) the EMU National(NUTS1), rural, urban and mid-urban index is respectively illustrated. Under the column "Eurostat Index", house price indices for the total sample, new dwellings and existing dwellings is respectively portrayed. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix

Table A.1: Country abbreviations

Austria	AU	**Luxembourg**	LU
England	EN	**Netherlands**	NL
Estonia	EE	**Norway**	NO
Finland	FI	**Poland**	PL
Germany	DE	**Portugal**	PT
Ireland	IE	**Scotland**	SC
Italy	IT	**Sweden**	SE
Lithuania	LT		

This Table illustrates the abbreviations used for each respective Country.

Table A.2: EMU index dataset variables

Variable Name	Description
country_id	Name of the country of the dwelling
city	Name of the city of the dwelling
nuts_3	Eurostat standardized NUTS3 information
date	Monhtly date
dateq	Quarterly date
sqm_price	Price(or rent) per square meter
lnprice	Natural logarithm of price(or rent) per square meter
residence_type	Type of housing (1=House, 2=Apartment)

This table describes the core variables used for the EMU index construction.

EU Country Index Discussion

Austria

Austrian house price indices are collected and calculated from the federal statistical office of Austria (Statistik Austria). The Austrian house price index is a quarterly indicator that measures the changes in the dwellings transaction prices that households acquire on the market regardless of its final use [16]. Furthermore, both houses and flats are included. The HPI is a chain-linked Laspeyres-type price index. Similarly to the methodology used in this paper, the indices are constructed using a reference period (2015) and track the evolution of prices from that period. The data derive from National Accounts and account for the overall amount of transactions. The indices are stratified (Vienna, cities, and urban areas) as well as dweling type (flats and houses). As commonly implemented in the hedonic literature, the logarithm of prices is used as the dependent variable. Social housing, government subsidies as well as housing cooperatives are controlled for, if included in the transaction price. Unfortunately, I cannot observe social housing transcations in our data.

For the analysis I use 225 thousand observations over the span of Q1:2018 to Q1:2020 in which, around 49% of the sample are housing units in urban regions, 21% in mid-urban classified regions and the rest 30% is rural.

Estonia

Estonian house price indices are collected and calculated from the federal statistical office of Estonia (Statistics Estonia). The statistical unit of measure is dwellings of private households and land purchased together with the dwelling. A stratification approach based on the geographical area, locality, dwelling age and type, and the number of rooms is used.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015) and track the evolution of prices from that period.

Estonian typology although does not classify any region as urban and therefore, I use around 300 thousand observations over the span of Q1:2018 to Q1:2020 in which, around 61% of the sample are housing units in mid-urban regions, the rest 39% is rural.

Finland

Finnish house price indices are collected and calculated from the federal statistical office of Finland (Statistics Finland). The data decompose into information on existing

[16] Prices include land value.

dwellings[17], new dwellings [18] and existing detached houses [19].

The HPI is a log-Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015).

For the analysis I use 690 thousand observation over the span of Q1:2018 to Q1:2020 in which, around 35% of the sample are housing units in urban regions, 25% in mid-urban classified regions and the rest 40% is rural.

Germany

German house price indices are collected and calculated from the federal statistical office of Germany (DESTATIS).The German house price index is a quarterly indicator of transaction prices that households acquire on the market regardless of its final use [20]. Furthermore, both houses and flats are included. The HPI is a chain-linked Laspeyres-type price index. The indices are constructed using a reference period (2015). The HPI is weighted by the total expenditure in the residential property market which is derived from data taken from GEWOS (Institute for City, Regional and Housing Research) as well as from DEGI(German Association for Real Estate Investment Funds). The index covers 14 out of the 16 federal states (excluding Berlin and Bremen.). For simplicity, I account for consumer price index changes but do not weigh the indices by population. The variables included in the construction of the German index from DESTATIS are: Existing and new turnkey-ready dwellings: type of dwelling (single-, two-family house, freehold flat), type of house (free-standing, terraced, semidetached), type of construction (conventionally built, prefabricated), date of purchase, total purchase price, age of dwelling, size of plot of land, size of living area, proportionate price of plot of land, standard land value ('Bodenrichtwert'), furnishing/luxury elements (kitchen, sauna/swimming-pool, attic storey), car parking facilities, characteristics of location (state, district, municipality; general rating of location: simple/medium/good), number of rooms/floors.

For the analysis around 4 million observations are used over the span of Q3:2017 to Q1:2020 in which, around 78% of the sample are housing units in rural regions, whereas the rest 22% is in urban.

[17]The data of the statistics on dwelling prices are based on the price information gathered by the Finnish Tax Administration for asset transfer tax calculation purposes.

[18]The data of the statistics on new dwelling prices are based on the information Statistics Finland receives via a private price monitoring service about transactions in new dwellings made by the largest real estate agents and building contractors.

[19]The data on transaction prices are obtained from the real estate register of the National Land Survey of Finland.

[20]Prices include land value.

Ireland

Irish house price indices are collected and calculated from the federal statistical office of Ireland (Central Statistics Office Ireland). HPI data for the purchases of new and existing dwellings are compiled on the basis of full transaction prices. They are collected through the use of administrative data sources.

The HPI covers all transactions of dwellings made by households regardless of its final use. The weights for the two sub-indices are equal to the total value of dwelling transactions for new and existing dwellings, respectively. Both prices and weights include land value.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015).

Around 154 thousand observations are used over the span of Q1:2019 to Q2:2020 in which, around 26% of the sample are housing units in urban regions, and the rest 74% in rural.

Italy

Italian house price indices are collected and calculated from the federal statistical office of Italy (Italian National Institute of Statistics). The data decompose into information on existing dwellings[21]. The HPI compilation is based on final market prices that are paid by households (VAT included); non-market prices are ruled out from the scope of the HPI (for example, the calculated final price for a dwelling that was developed by a step-by-step self-building is excluded).

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015).

For the analysis I use 1,1 milion observations over the span of Q2:2019 to Q1:2020 in which, around 20% of the sample are housing units in urban regions, 50% in mid-urban classified regions and the rest 30% is rural.

Lithuania

Lithuanian house price indices are collected and calculated from the federal statistical office of Lithuania (Lithuanian Department of Statistics (Statistics Lithuania)). The statistical unit of measure is dwellings of private households and land purchased together with the dwelling. A stratification approach based on the geographical area, locality, dwelling age and type, and the number of rooms is used.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015).

[21] The data are based on administrative information; in particular, prices of dwellings are gathered from notarial deeds of sales data provided by the Tax Office.

For the analysis I use around 9 thousand observations over the span of Q2:2018 to Q2:2020 in which, around 60% of the sample are housing units in urban regions, 22% in mid-urban classified regions and the rest 18% is rural.

Luxembourg

Luxembourg's house price indices are collected and calculated from the federal statistical office of Luxembourg (STATEC). They are based on the official prices as indicated in notary acts which have been registered at the 'Administration de l'Enregistrement et des Domaines' (AED). The HPI is a chain-linked Laspeyres-type price index published using a common index reference period (2015=100).

The indices for the value of housing transactions (purchased by households) have the year 2015 as common reference period.

Luxembourg comprises of only one NUTS3 region due to its size, therefore there can be no regional decomposition of indices.

Netherlands

Dutch house price indices are collected and calculated from the federal statistical office of Netherlands (Statistics Netherlands - Economics and Business statistics). HPI data for the purchases of new and existing dwellings are compiled on the basis of full transaction prices. They are collected through the use of administrative data sources.

The HPI covers all transactions of dwellings made by households regardless of its final use. The weights for the two sub-indices are equal to the total value of dwelling transactions for new and existing dwellings, respectively. Both prices and weights include land value.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. Similarly to the methodology used in this paper, the indices are constructed using a reference period(2015) and the evolution of prices from that period.

Around 600 thousand observations are used over the span of Q2:2019 to Q2:2020 in which, around 66% of the sample are housing units in urban regions, 32% in intermediate mid-urban regions and the rest 2% in rural.

Norway

Norwegian house price indices are collected and calculated from the federal statistical office of Netherlands (Statistics Norway). HPI data for the purchases of new and existing dwellings are compiled on the basis of full transaction prices. They are collected through the use of administrative data sources.

The HPI covers all transactions of dwellings made by households regardless of its final use. The weights for the two sub-indices are equal to the total value of dwelling transactions for new and existing dwellings, respectively. Both prices and weights include land value.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015).

Eurostat does not provide typology heterogeneity for Norway in order to safely observe house price heterogeneities based on regional diversity.

Poland

Polish house price indices are collected and calculated from the federal statistical office of Poland (Central Statistical Office of Poland). HPI data for the purchases of new and existing dwellings are compiled on the basis of full transaction prices. They are collected through the use of administrative data sources.

The HPI covers all transactions of dwellings made by households regardless of its final use. The weights for the two sub-indices are equal to the total value of dwelling transactions for new and existing dwellings, respectively. Both prices and weights include land value.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015).

I use around 141 thousand observations over the span of Q2:2018 to Q2:2020 in which, around 28% of the sample are housing units in urban regions, 70% in intermediate mid-urban regions and the rest 2% in rural.

Portugal

Portuguese house price indices are collected and calculated from the federal statistical office of Portugal (Instituto Nacional de Estatística, Statistics Portugal). HPI data for the purchases of new and existing dwellings are compiled on the basis of full transaction prices. They are collected through the use of administrative data sources.

The HPI covers all transactions of dwellings made by households regardless of its final use. The weights for the two sub-indices are equal to the total value of dwelling transactions for new and existing dwellings, respectively. Both prices and weights include land value.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015).

I use around 815 thousand observations over the span of Q1:2018 to Q2:2020 in which, around 3% of the sample are housing units in urban regions, 84% in intermediate mid-urban regions and the rest 14% in rural.

Sweden

Swedish house price indices are collected and calculated from the federal statistical office of Sweden (Statistics Sweden). HPI data for the purchases of new and existing dwellings are compiled on the basis of full transaction prices. They are collected through the use of administrative data sources.

The HPI covers all transactions of dwellings made by households regardless of its final use. The weights for the two sub-indices are equal to the total value of dwelling transactions for new and existing dwellings, respectively. Both prices and weights include land value.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period (2015).

I use around 156 thousand observations over the span of Q1:2019 to Q2:2020 in which, around 25% of the sample are housing units in urban regions, 53% in intermediate mid-urban regions and the rest 22% in rural.

United Kingdom

UK's house price indices are collected and calculated from the federal statistical office of the United Kingdom (Office for National Statistics). HPI data for the purchases of new and existing dwellings are compiled on the basis of full transaction prices. They are collected through the use of administrative data sources.

The HPI covers all transactions of dwellings made by households regardless of its final use. The weights for the two sub-indices are equal to the total value of dwelling transactions for new and existing dwellings, respectively. Both prices and weights include land values.

The HPI is a chained Laspeyres-type price index adjusted for quality changes using hedonic models. The indices are constructed using a reference period(2015). The composition of the UK's indices comprises out of housing information deriving from England and Scotland (see Table 1).

I use around 350 thousand observations over the span of Q2:2019 to Q2:2020 in which, around 54% of the sample are housing units in urban regions, 40% in intermediate mid-urban regions and the rest 6% in rural.

References

Aoki, K., J. Proudman, and G. Vlieghe (2004). House prices, consumption, and monetary policy: a financial accelerator approach. *Journal of financial intermediation 13*(4), 414–435.

Attanasio, O. P., L. Blow, R. Hamilton, and A. Leicester (2009). Booms and busts: Consumption, house prices and expectations. *Economica 76*(301), 20–50.

Bauer, T. K., S. Feuerschütte, M. Kiefer, P. an de Meulen, M. Micheli, T. Schmidt, and L.-H. Wilke (2013). Ein hedonischer immobilienpreisindex auf basis von internetdaten: 2007–2011. *AStA Wirtschafts-und Sozialstatistisches Archiv 7*(1-2), 5–30.

Berger, D., V. Guerrieri, G. Lorenzoni, and J. Vavra (2017). House prices and consumer spending. *The Review of Economic Studies 85*(3), 1502–1542.

Campbell, J. Y. and J. F. Cocco (2007). How do house prices affect consumption? evidence from micro data. *Journal of monetary Economics 54*(3), 591–621.

Can, A. (1992). Specification and estimation of hedonic housing price models. *Regional science and urban economics 22*(3), 453–474.

Case, K. E. and R. J. Shiller (2003). Is there a bubble in the housing market? *Brookings papers on economic activity 2003*(2), 299–362.

Clapp, J. M. and C. Giaccotto (1998). Price indices based on the hedonic repeat-sales method: application to the housing market. *The Journal of Real Estate Finance and Economics 16*(1), 5–26.

Cloyne, J., K. Huber, E. Ilzetzki, and H. Kleven (2019). The effect of house prices on household borrowing: a new approach. *American Economic Review 109*(6), 2104–36.

de Haan, J. (2004). Direct and indirect time dummy approaches to hedonic price measurement. *Journal of Economic and Social Measurement 29*(4), 427–443.

de Haan, J. and W. Diewert (2011). Handbook on residential property price indexes. *Luxembourg: Eurostat*.

DeFusco, A. A. (2018). Homeowner borrowing and housing collateral: New evidence from expiring price controls. *The Journal of Finance 73*(2), 523–573.

Del Negro, M. and C. Otrok (2005). Monetary policy and the house price boom across us states.

Diewert, W. E., A. O. Nakamura, and L. I. Nakamura (2009). The housing bubble and a new approach to accounting for housing in a cpi. *Journal of Housing Economics 18*(3), 156–171.

Dinkel, M. and B. Kurzrock (2012). Asking prices and sale prices of owner-occupied houses in rural regions of germany. *Journal of Interdisciplinary Property Research 13*(1), 5–25.

Fan, G.-Z., S. E. Ong, and H. C. Koh (2006). Determinants of house price: A decision tree approach. *Urban Studies 43*(12), 2301–2315.

Fernald, J. (2014). A quarterly, utilization-adjusted series on total factor productivity. Federal Reserve Bank of San Francisco.

Follain Jr, J. R. and S. Malpezzi (1981). Another look at racial differences in housing prices. *Urban Studies 18*(2), 195–203.

Gerlach, S. and W. Peng (2005). Bank lending and property prices in hong kong. *Journal of Banking & Finance 29*(2), 461–481.

Gouriéroux, C. and A. Laferrère (2009). Managing hedonic housing price indexes: The french experience. *Journal of Housing Economics 18*(3), 206–213.

Gyourko, J., C. Mayer, and T. Sinai (2013). Superstar cities. *American Economic Journal: Economic Policy 5*(4), 167–99.

Han, L. and W. C. Strange (2016). What is the role of the asking price for a house? *Journal of Urban Economics 93*, 115–130.

Harding, J. P., S. S. Rosenthal, and C. Sirmans (2007). Depreciation of housing capital, maintenance, and house price inflation: Estimates from a repeat sales model. *Journal of urban Economics 61*(2), 193–217.

Haughwout, A., J. Orr, and D. Bedoll (2008). The price of land in the new york metropolitan area. *Current Issues in Economics and Finance 14*(3).

Hill, R. (2011). Hedonic price indexes for housing.

Hill, R. J. and D. Melser (2008). Hedonic imputation and the price index problem: an application to housing. *Economic Inquiry 46*(4), 593–609.

Hill, R. J., M. Scholz, et al. (2014). Incorporating geospatial data in house price indexes: a hedonic imputation approach with splines. *Graz Economics Papers 5.*

Himmelberg, C., C. Mayer, and T. Sinai (2005). Assessing high house prices: Bubbles, fundamentals and misperceptions. *Journal of Economic Perspectives 19*(4), 67–92.

Holly, S., M. H. Pesaran, and T. Yamagata (2010). A spatio-temporal model of house prices in the usa. *Journal of Econometrics 158*(1), 160–173.

Hott, C. (2011). Lending behavior and real estate prices. *Journal of Banking & Finance 35*(9), 2429–2442.

Kaplan, G., K. Mitman, and G. Violante (2015). Consumption and house prices in the great recession: Model meets evidence. *Manuscript, New York University*.

Malpezzi, S. (2003). Hedonic pricing models: a selective and applied review. *Housing economics and public policy*, 67–89.

McCarthy, J. and R. W. Peach (2004). Are home prices the next bubble? *Economic Policy Review 10*(3).

Mian, A., K. Rao, and A. Sufi (2013). Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics 128*(4), 1687–1726.

Mian, A. and A. Sufi (2011). House prices, home equity-based borrowing, and the us household leverage crisis. *American Economic Review 101*(5), 2132–56.

Mora, N. (2008). The effect of bank credit on asset prices: evidence from the japanese real estate boom during the 1980s. *Journal of Money, Credit and Banking 40*(1), 57–87.

Mueller, H. M. and C. Yannelis (2019). The rise in student loan defaults. *Journal of Financial Economics 131*(1), 1–19.

Piketty, T. and G. Zucman (2014). Capital is back: Wealth-income ratios in rich countries 1700–2010. *The Quarterly Journal of Economics 129*(3), 1255–1310.

Saiz, A. (2010). The geographic determinants of housing supply. *The Quarterly Journal of Economics 125*(3), 1253–1296.

Sheppard, S. (1999). Hedonic analysis of housing markets. *Handbook of regional and urban economics 3*, 1595–1635.

Shiller, R. J. (2006). Long-term perspectives on the current boom in home prices. *The Economists' Voice 3*(4).

Shimizu, C., K. G. Nishimura, and T. Watanabe (2016). House prices at different stages of the buying/selling process. *Regional Science and Urban Economics 59*, 37–53.

Triplett, J. (2004). Handbook on hedonic indexes and quality adjustments in price indexes.

Vansteenkiste, I. and P. Hiebert (2011). Do house price developments spillover across euro area countries? evidence from a global var. *Journal of Housing Economics 20*(4), 299–314.

To rent or not to rent: A household finance perspective on Berlin's short-term rental regulation*

Antonios Mavropoulos †

Abstract

With the increasing concerns that accompany the rising trends of house sharing economies, regulators impose new laws to counteract housing supply scarcity. In this paper, I investigate whether the ban on short-term entire house listings activated in Berlin in May 2016 had any adverse effects from a household finance perspective. More specifically, I derive short-term rental income and counter-factually compare it with long-term rental income to find that the ban, by decreasing the supply of short-term housing, accelerated short-term rental income but did not have any direct effect on long-term rental income. Commercial home-owners therefore would find renting on the short-term market to be financially advantageous.

Keywords: Housing markets, Airbnb, Short-term rental markets, Sharing economy regulation
JEL Codes: R30; R31; D31

*I would like to thanks Michael Koetter and Huyen Nguyen for their guidance and the participants of the IWH Doctoral and Brown bag seminars for their constructive comments.
†Halle Institute for Economic Research (IWH), E-mail: antonios.mavropoulos@iwh-halle.de.

1 Introduction

In the recent years, most Berliners have gotten acquainted with increasing rental prices. Although the rising trend of housing prices in the last decade can be fundamentally justified (Kajuth (2017); Kholodilin and Michelsen (2017))[1], the latter years introduced another factor of potential price acceleration in the form of the sharing economy for housing. With housing becoming more expensive or affordable due to changes in the elemental factors of demand and supply, sharing economy websites like Airbnb, Wimdu or 9flats are a contributing factor for providing supply deficiencies in the housing market, as they remove dwellings from the long-term rental market for the purpose of renting to interim tenants but not to the city's residents. This effect is especially profound in large cities such as Berlin, i.e. cultural metropolises and touristic destinations where high demand drives accounts for a higher short-term rental income for commercial home-owners[2].

The economic and social effects of booming STR economies are still to a large extent uncharted territory. The convenience and supplementary income that they may provide to short-term tenants and commercial home-owners respectively, can be counteracted by welfare dis-utilities and housing market gentrification. Policy intervention, which has so far come in many forms, as I discuss in the following section of the paper, aims for welfare equilibrium. Due to the recent nature of the housing sharing economy, policy making does not have a standard prior and consequently, it is of great importance to investigate its outcomes.

To counteract an ever increasing supply of housing of short-term rental (STR for the remainder of the thesis) housing units, Berlin's city senate introduced in May 2016 the "Zweckentfremdungsverbot" (law against misappropriation of housing space). This regulation threatened anyone who offers an entire flat[3] for rent with the intent of generating profit. Any commercial exploitation had to be sanctioned with a special permit from the city. However, after a sharp decline in the offerings, flat owners continued to supply short-term rental properties, even amidst an active ban and potential fines.

Following the debate on short-term renting, its costs and accompanying benefits (Guttentag (2015); Coldwell (2017); Ioannides et al. (2018)), this paper investigates the effects of Berlin's ban of Airbnb listings on the housing market from a household finance perspective. In contrast to the existing empirical literature on the effects of Airbnb on rental prices, I look into how the regulation can accelerate landlords' incentives to rent their housing units on the short-term rental market. The question I pose is whether regulation

[1] https://www.immobilienscout24.de/content/dam/is24corporate/documents/unternehmen/Publikationen/2014/Immobilienreport_2014_IS24.pdf

[2] A landlord who does not reside in their residence, but offers it on the rental market.

[3] The regulation was targeted against those who rent an entire flat (more than 50% of the property), not a spare room in their apartment.

on the short-term rental market amplified STR financing incentives of commercial home-owners, therefore inducing adverse effects on the rental market. Did the consequent decreased supply of STR offerings on the market, increase short-term rental income? From a household finance perspective, a commercial home-owner observing accelerating STR incomes would find that renting her housing unit through the sharing economy to be financially beneficial in comparison to long-term renting. The first order effect should be an acceleration in STR income. Higher premiums on STR rentals should then impose pressure in long-term rental prices as well, which would eventually be the exact opposite of what the regulation intended.

Figure 1 visually illustrates evidence of such behaviour. The STR/LTR[4] is the short-term rental income from renting a housing unit on the short-term rental market divided by the counterfactual long-term rental income generated by renting in the long-term rental market. I observe that, in anticipation of the ban, short-term rental income to long-term rental income ratio sharply decreased from 2.75 to 1.25. This is possibly due to cancellations in line with legal conformity. In the post regulation period though, quite a few commercial home-owners[5] disobeyed the regulation and this contributed to an accelerating supply of STR housing. This could be explained by two potential mechanisms. Either the regulation was able to curb rental price hikes, or landlords were therefore able to absorb the forfeited income from others, explaining the increase in their STR income. The Data section of the paper describes how I construct this ratio in detail.

– Figure 1 here –

Short-term renting might be a substitute for renting in the long-term, but there are a few crucial differences between the two. First and foremost, the STR market is geographically selective. Housing units available for short-term renting are usually concentrated around the city centers, close to their financial, historical and touristic areas as Figure 3 in the Appendix illustrates. These areas are usually occupied by offices and buy-to-let properties, therefore home-ownership rates should be lower than in a city's outskirts. Nevertheless, home-ownership rates differ within high-concentration STR postal code areas as I show in the following section. I therefore investigate heterogeneous effects of the regulation with regard to home-ownership rates. I test the following hypotheses: First, neighborhoods with higher home-ownership rates should provide a buffer against such effects, as the rental market is a smaller portion of the total housing stock. Second, neighborhoods with saturated, high amount of STR dwellings should not be drastically affected as well, as market clearance is accounted from the superfluous supply of STR housing units. The results can be suggestive of different types as well as levels of regulations to be imposed across several areas within the city, as short-term rental markets are

[4] Long-term rental income.

[5] Homeowners who rent their apartment on the short-term market.

selective to gentrified and booming areas.

From a theoretical perspective, the recent literature aims to provide perspectives on the collaborative economy, including STR platforms such as Airbnb (Dredge and Gyimóthy (2017); Gyódi (2017); Hatzopoulos (2018)). Jefferson-Jones (2015) and Lee (2016) provide a background on how sharing economies for housing can influence house prices, whereas other papers propose regulations measures (Miller (2014); Gurran and Phibbs (2017); Nieuwland and van Melik (2018)).

On the empirical side, only a handful of papers deal with identifying the effects of STRs on house price accelerations (Horn and Merante (2017), Koster et al. (2018) and Garcia-López et al. (2019)). Barron et al. (2018) investigate the effects of nearby presence of Airbnb listings on rental prices in the US using Google trends as an instrument, to find a 1% increase in Airbnb listings leads to a 0.018% increase in rents and a 0.026% increase in house prices. Segú (2018) investigates the effect of the beginning of the economic activity of Airbnb on rental prices, using the dwelling's distance to the beach as an instrument, to find that a 4% increase in rents. Franco et al. (2019) implement a differences-in-differences analysis between the two major cities in Portugal, namely Lisbon and Porto, to find an overall increase in property values of 34% and 10.9% for rents due to the short-term lease regulatory reform.

This paper contributes to the empirical literature of the sharing economy market for housing by combining data on STR and LTR markets and looking on adverse effects of STR regulation from a household finance perspective. I aim to derive and combine short-term rental income and compare it with the counterfactual long-term rental income in order to investigate into financing decisions made from the landlords after such regulation. The perspective of the paper diverges from the aforementioned empirical literature, as I aim to show whether Berlin's regulatory framework on the STR market induced increasing incomes for non-conforming landlords. Furthermore, I aim to provide evidence of the interdependence between short-term and long-term rental markets as well as investigate potential spillovers on other sub-segments of the STR market.

Arguably, these effects can migrate from rentals to sales, fluctuating not only rents but also housing sales prices. Investors, observing the ever-increasing demand for short-term rentals might find that investing in new construction and acquisition of real estate can yield positive returns. Vice versa, and in line with Kim et al. (2017), where they find that quota regulation depreciated the value of non-resident owned properties and decreased their demand, I also investigate whether the regulation had any effect in house price acceleration.

The empirical evidence suggest that the regulation on the short-term rental market in Berlin accelerated STR income cumulatively by 50%, whereas I find no direct effects on long-term rental income. Furthermore, I illustrate that saturated markets were a mitigating factor of STR income acceleration, as postal code areas with high home-ownership rates were able to provide a buffer against such movements in STR income. I furthermore find that STR income growth induces an acceleration of LTR prices after one quarter but the effect is rather minuscule. Nevertheless, using the regulation as an instrument for the supply of STR housing, I find that an increase on STR supply by 1%, increases long-term rental prices by around 6 euros. Investigating migration patterns from one sub-division of the STR market to the other, namely, from entire apartments to rooms which satisfy the less than 50% of the housing unit rule, an increase in the supply of rooms to rent on the STR market is observed and a marginally insignificant decline on their income. Regarding spilovers on the house prices, I find that the ever-accelerating housing market of Germany's capital was quite inelastic towards the decreasing supply of STR housing due to the regulation.

2 Regulatory Background

Regulation on STR markets is not a rare spectacle for the larger and most touristic cities around the world. As large metropolitan areas and urban centers suffer from the lack of available housing units, several laws have been implemented in order to control supply. In order to understand the motives and targets of these regulations, I provide a birds-eye view on the different policies and the regulatory background for the ban of short-term listings on the German capital, which served as the most austere regulation so far.

The most common regulation on the house sharing economy is a days per year quota. In Paris, since December 2017, regulators gave an allowance to citizens who want to sublet their home on an online platform if they register with the cities authorities in order to ensure that their property is not rented for more than 120 days per year. In Barcelona, since May 2018 online platforms are forced to provide regulators access to their online data, in order to make prosecution easier for dwellings that are rented for more than 31 days a year. Since January the 1st, 2019 in Amsterdam, landlords can offer houses on the STR market for up to 30 days a year. In London, since March 2015 an STR income tax has been introduced. On the other side of the Atlantic, in New York and San Fransisco, apartments could not be rented for more than 30[6] and 90[7] days respectively.

Due to the strict rental laws applied in Berlin (long-term rental income is staggered and can only exceed the local comparative rent by a maximum of to 10%(In German, *Mietpreisbremse*)[8]), as well due to the high demand for housing experienced in the German capital, short-term renting became a good alternative for landlords.

According to Airbnb publicly available metadata, Berlin currently has around 22

[6] In June 2018, the city passes law to disclose host information. In January 2019, the is upheld.

[7] However, San Francisco decided to crack down further in 2015, forcing all Airbnb hosts to register with the city via an onerous process.

[8] https://www.stadtentwicklung.berlin.de/wohnen/mieterfibel/de/m_miete1.shtml

thousand active rental offerings [9], while 47% of these rentals are entire houses or flats[10]. Whereas the percentage of offerings might not seem that high from a first glimpse, the total number of long-term rental flats are dispersed all across Berlin, whereas short-term listings are more likely to be located Figure 4 and Figure 3 in the Appendix show the dispersion and density of dwellings for each market respectively.

Starting in May 2016, Germany's capital banned landlords from renting apartments through short-term rental online platforms such as Airbnb. The law targeted online listings where more than 50% of the house or apartment was offer for short-term rent. There are two main subsegments of the STR housing market. Commercial home-owners can choose to offer an entire dwelling or sublet a room. The regulation targeted only entire dweelings for short-term rent. Although the supply of STR housing is endogenous to house and rental price acceleration due to reverse-causality, the new law that came into place specifically mentioned that its target is to reduce STR supply, suggesting an exogenous shock to rental price fluctuations. Nevertheless, I perform a series of tests in the empirical results section in order to potentially account for biased results due to endogeneity.

The law was announced two years before its activation, giving a window for preparation both for city's officials and commercial home-owners alike. The penalty for breaking the law was a substantial €100,000 fine, levied on the landlords. Figure 2 below illustrates the relative effectiveness of the regulation in reducing the supply of short-term rental units.

– Figure 2 here –

With the ban activated in May 2016, I observe a big decline in the number of offered listings on the STR market. Nevertheless, the regulation did not manage to eradicate the supply of entire apartments listed online. Demand for cheaper short-term housing in Berlin remained inelastic and quite a few landlords did not abide with the new law. The inability of Berlin's authorities to track and fine the non-conforming landlords imposed enforcing barriers, and not long after, commercial home-owners re-entered the sharing economy rental market, essentially barring the supply of the long-term rental market once again.

Although the focus of this paper is only on the ban on the STR market of entire housing units, it is worth mentioning that two years afterwards, due to the regulation's enforcing inability, Berlin's authorities lifted it, imposing a new set of rules. The city's assembly decided, that, under certain conditions, landlords will be allowed once more to rent out their own home without restrictions, and to rent out second homes for up to 90

[9] 1.1% of the 1.9 million flats in Berlin. (https://www.statistik-berlin-brandenburg.de/BasisZeitreiheGrafik/Zeit-Gebaeude_Wohnen.asp?Ptyp=400&Sageb=31000&creg=BBB&anzwer=7).

[10] Source: http://insideairbnb.com/berlin/

days a year. The new guidelines nonetheless impose some pretty firm prerequisites on vacation rentals and far more stringent penalties. All landlords seeking to rent out their home will only be allowed to do so if they get a general permit from their borough, even if they intend only to rent their property out for occasional short stays. While landlords applying for a permit at their primary residency will likely be approved, second home owners may face a more rigorous process. Landlords who leave an apartment vacant, meanwhile, will need a special permit from the borough to do so after three months of vacancy without having a permanent tenant registered, cutting the current vacancy grace period in half. Most strikingly, the maximum penalty for breaking the rules has been multiplied by five, to a potential fine of €500,000.

3 Data

In order to research the effects of the regulatory ban of short-term rental housing units on household financial decisions and eventually rental prices, I combine data on online adverts from the largest platform on short-term listings, namely Airbnb, with data on long-term rental prices. Furthermore, I want to assess the disperse effects that the regulation on STR might have across different areas of the Berlin, so regional variables at the district level are collected.

Firstly, I use granular (postal-code) information on short-term listings from InsideAirbnb[11] spanning from October 2015 up to May 2017[12]. The data contain information such as the price, listing identifier, number of bed and bathrooms, type of housing, postal code, monthly date and cleaning fee. Square meters are also provided but on their majority are missing (98.88%). Auxiliary to this dataset, a calendar dataset illustrates the listing identification, daily date, price and whether the listing was rented out. The price of each listing is aggregated on a monthly level, thus allowing for calculation of the monthly income from each exact online advert. On the next step, I merge the two aforementioned datasets and include information such as income per listing per month (STR income) and days rented, which can serve as a proxy for demand. Prices are reported in US dollars, so in order to make them comparable, historical daily exchange rates from US dollars to Euro are used. The dataset contains 883,090 observations on short-term Berlin adverts on Airbnb, but since 57.21% of the observations do not contain postal code information, the final Airbnb sample consists of around 380,000 observations.

The second strand of data in use is information from Immobilienscout24.de, Germany's largest online platform of housing units for either rent or sales. The dataset is reduced to postal-code level information on Berlin's housing market for rentals which

[11] http://insideairbnb.com/get-the-data.html

[12] The Airbnb data span span from: October 2015 - February 2016; April 2016 - May 2017 and April 2018 - July 2019. I use only the first and second rendition of the data until May 2017, since I do not want the effects to interfere with potential anticipation effects from the lift of the ban in May 2018.

are available from January 2007 up to May 2017[13]. Firstly, I use data provided by the RWI in Essen which span from January 2007 to October 2017. We append the collected data on a monthly basis from the website using web-scraping techniques in order to extend the dataset's time-frame. The data entail granular information on all residential dwellings (apartments and houses), which were offered for sale or for rent on the website. The advantages of the data lie within the high volume of observations (approximately 33 million observations for the whole of Germany), the geographical information (in 98.5% of the data we observe each listing's postal code) and the plethora of qualitative traits of dwellings, which allow the construction of elaborate quality adjusted hedonic regional house price indices. Eventually, since the analysis focuses on the rental market for apartments in Berlin, I reduce the data to around 1,7 million observations.

One shortfall of the data is that asking are collected but not transaction prices. However, Dinkel and Kurzrock (2012) show for rural areas in Rhineland-Palatine that besides a slight price markup there are no systematic differences between asking and transaction prices. Furthermore, I observe asking prices for both the STR market as well as the LTR market, so continuity ensures comparability between the two datasets.

In order to control for cross-sectional discrepancies among districts, I furthermore include yearly district information on new building permits, new construction and total over-night stays as well as number of guests, which should capture regional effects and touristic attractiveness. I also use a cross-sectional, time-invariant variable of population among postal codes, as population weighted regressions can be implemented to account for the size of each district. These data derive from the statistical office of Berlin and Brandenburg[14].

The first step of the data entails matching STR online adverts with long-term rental dwellings based on their location and characteristics. Short-term listings come with fixed cleaning cost. There is no need to deduct this fixed cost since it bears the renter and not the landlord. In other words, it will not affect her STR income. In what can be referred as a short-term spread, the difference between income from short-term renting and renting is a measure of the intensity of the market towards the short-term option. The larger the spread is, short-term renting should become a more attractive option for a commercial home-owner. I match dwellings from both markets on information on their postal code, date, number of bedrooms and bathrooms. I include only STR adverts of entire apartments, as advertised rooms for rent have intrinsic differences with entire apartments. With the number of characteristics being limited, one STR listing can be matched with multiple LTR adverts. The deriving dataset matches 44.67% between the two datasets, accounting to 1,4 million observations, between January 2016 and November

[13]The data span until August 2019, but for the analysis I reduce the information so that it matches the Airbnb dataset.

[14]https://www.statistik-berlin-brandenburg.de/

2018, which is an imposed limitation of the Airbnb data.

Since there is a break in the data between May 2017 and April 2018, I do not use data later than May 2017. The final dataset includes 559,998[15] pairwise information between STR and LTR listings from January 2016 to May 2017, or else 5 months of information before the ban and 12 months afterwards.

Table 1 provides the summary statistics of the data, before and after the STR regulation was activated. The means, standard deviations, number of observations and a t-test between the two samples(before and after the regulation) is illustrated to test if there are statistically significant differences in the variables of interest before and after the ban on short-term housing.

– Table 1 here –

Furthermore, Table A.3 in the Appendix describes the variables in use, their sources and unit of measurement.

4 Empirical Identification and Results

4.1 Effects on the STR to LTR ratio

In the first stage of the analysis, I investigate the effects of the regulation of Berlin's STR markets on the generated incomes from both the short-term and long-term market. My hypothesis suggests that the ban, which aimed to reduce the supply of STR housing units increased income for landlords who disobeyed the law. Therefore, I construct a measure of STR attractiveness from the landlord perspective, namely the STR to LTR income ratio.

The accelerated income as Figure 5 in the Appendix illustrates, does not derive from increased prices, but from increasing demand. That is due to the "booking lag", i.e. the fact that bookings on the STR market are likely to be made several months in advance, so prices could not be adjusted in response to higher demand. Individual bookings which were canceled due to the ban, probably migrated to other adverts who remained online, thus effectively, increasing the days that the remaining listings were rented and consequently, the landlord's STR income. Figure 6 illustrates the average rental days per short-term apartment before and after the ban. Even though that especially the anticipation of the ban curbed demand, the increase in short-term rental income can be mainly explained by the fact that the apartments who stayed in the market were occupied at an higher rate, and were therefore able to yield higher incomes. Such effect could have negative connotations for the development of rental prices in the surrounding region as well. As people observe accelerating STR incomes, landlords might be willing to either

[15] I drop the bottom and top 1% of the STR to LTR ratio across all dates and postal codes.

jump in the STR market until it gets saturated, or charge a price mark-up for new rentals, in order to account the premium of the STR market.

In order to test the first hypothesis, I calculate growth variables for each postal code region in Berlin. As Figure 1 descriptively suggests, the STR to LTR ratio levels dropped in anticipation of the regulation, but an upward trend followed right afterwards. In order to extract meaningful empirical evidence, I aggregate the sample from individual listings to postal codes per month and then decompose the ratio to STR and LTR income. The reasoning is that I aim to understand which is the driving force of potential changes in the ratio. My hypothesis suggests that the change should originate from changes in STR income first, and that in the medium to long-term rental prices can also grow, in response to the success of STRs in the market.

My second hypothesis states that, the effects of the regulation in STR saturated markets, should be diminished. That is, the STR to LTR ratio in areas like these could still grow, but at a lower pace, as there was already an overflow of STR listings in the pre-regulation period. Since the overflowing supply reduces, there is still slack to meet demand and incomes should accelerate at a lower pace. Furthermore, a decline in rental prices might be observed, since STR migration to the long-term rental market should be easier than in less dense and non-gentrified areas. The empirical methodology in the first approach is of the following:

$$SLgrowth_{p,t} = \alpha_y + \alpha_q + \beta Bnb\_ban_t + \gamma Supply_{p,t} + \delta Controls_{r,y} + \epsilon_{p,t} \tag{1}$$

where $SLgrowth$ is the short-term to long-term income ratio, where p and t denotes postal code and monthly date respectively. *Bnb_ban* is a time dummy which is equal to one from May 2016 and onwards. *Supply* is the logarithmic transformation of the number of STR listings in each postal code and date. This variable serves as a control for the supply of STRs and also investigates whether increased supply mitigates the adverse effects of the regulation. I furthermore include year α_y and quarterly α_q dummies to account for time trends and seasonality. Date fixed effects are not included, as they are collinear with the regulation *Bnb_ban* time dummy variable. I finally cluster standard errors by district[16], in order to account for cross-sectional autocorrelation. Berlin is split into administrative areas[17]. These account to 24 unique areas in the sample. According to Cameron et al. (2008), clustering at the area level would induce upward bias in the statistical significance of the estimates. I therefore cluster on an area sub-level (Districts). There are in total 59 districts in the sample. Areas and districts are illustrated in Table A.4 in the Appendix. Postal codes in the same district are likely to experience similar trends with regard to their rental markets, both in the long as well as the short-term. Table 2 illlustrates the

[16] Districts are derived from the first four digits of each postal code.
[17] *Ortsteile or Bezirke* in German

results.

– Table 2 here –

The results in Table 2 support the first hypothesis, that the ban accelerated the growth of STR income. In Columns 1,3 and 4, I examine the effects on the STR to LTR ratio growth, STR income growth and LTR income growth respectively. The results indicate that throughout the period of the ban, the ratio increased by 52,3% (Results in Column 1). As a follow-up, I decompose the ratio to observe that this result derives mainly from the effects of the regulation on the STR market. The ban cumulatively increased STR income by 60,6% whereas there were marginally no effects on the LTR income growth. Nevertheless, the coefficient for LTR income growth is positive, subtly indicating that the ban did not manage to reduce rental hikes. In Columns 2,4 and 6 I include an STR supply control, to investigate the follow-up hypothesis of mitigating effects for regions where STR supply is high. The results suggest that a 1% increase in the levels of STR supply, reduces the ratio growth rate by 0,2%. This means that in areas above the 80% percentile of the STR supply distribution, STR grew around 16% less than in other regions with scarcer supply. The effect is more profound on the STR market as can be observed in Column 4, whereas in Column 6, the coefficient suggests that for 1% increase in the STR supply levels, rental income growth declined by 0,007%. In districts above the 80% percentile LTR rental income decreased by 0,5% showing minor evidence that in areas where a lot of STR listings existed, there was a larger migration from the STR to the LTR market after the ban and thus, a slight decrease in rental income from the landlord's perspective. This effect can also be observed on Figure 7, as the regulation manages, in the short-term, to inflate short-term rental income, but has a rather minimal effect on long-term rental prices within one year after its activation. The analysis is supplemented by including the STR listings number weighted by population, in order to scrutinize the findings on STR supply and consequently, that postal code areas with a bigger number of STR listings experience slower STR growth. Table A.2 in the Appendix illustrates that a 1% increase in the supply of STR housing per capita in the post period contributes to the deceleration of SL growth and STR growth by 0,7% and 0,9% respectively.

On the follow-up hypothesis, I test whether higher level of home-ownership rates are able to negate the acceleration of of the STR to LTR ratio. Due to the lack of data on home-ownership rates at the postal code and monthly level in Germany, I use supply indicators as a home-ownership proxy. This variable is constructed by dividing the number of housing units for sale, houses and apartments over the total stock of housing both for sale and rent. The ownership proxy is constructed as follows:

$$Ownership\_rate_p = \frac{\#\, of\ listings\ for\ sale_p}{\#\, total\ housing\ stock_p} \tag{2}$$

I consider that the supply of housing among regions is directed towards satisfying demand. In densely populated central areas of big cities, home-ownership rates are low, because tenants are usually commuting workers and university students. Therefore, in central areas it is more likely that advertised online adverts are directed towards the rental market. Home-ownership rates should therefore be lower in comparison to other areas where apartments or houses for sale are more frequently advertised, in order to satisfy demand. I want to investigate whether areas with higher home-ownership rates provide a buffer against increasing STR income. In order to empirically test this, I include the home-ownership proxy in an empirical estimation similar to the previous one and its interaction with the ban, while controlling for the supply of STRs, yearly trends and seasonality. The empirical methodology to assess the effect of home-ownership rates on mitigating STR to LTR income growth is of the following:

$$\begin{aligned} SLgrowth_{p,t} = {} & \alpha_y + \alpha_q + \beta Bnb\_ban_t + \gamma Ownership\_rate_p + \delta Bnb\_ban_t \times Ownership\_rate_p \\ & + \eta Supply(inln)_{p,t} + \iota Controls_{r,y} + \epsilon_{p,t} \end{aligned} \tag{3}$$

where variables are constructed in line with Equation 1. *ownership_rate* is a time and postal code varying home-ownership proxy, illustrated in Equation 2. Standard errors are clustered by district. Table 3 illlustrates the results.

– Table 3 here –

The results in Table 3 show that postal code areas with high home-ownership rates, were able to mitigate the effects of the regulation. Similarly with the results on Table 2, the effect is positive and significant for STR income growth. Areas where home-ownership rates are high, are usually in he city's outskirts and therefore demand for STRs is quite lower. Arguably, since demand is lower, STR landlords do not have enough leverage to push prices upwards,nor do they expect to see an increase in demand even after the ban was activated. The interaction term of the regulation and home-ownership rates across postal codes indicates whether commercial home-owners in areas with high levels of home-ownership experienced a lower increase in their STR growth. In the pre-ban period, I observe no cross-sectional differences. In the post period though, areas with high ownership rates indeed experienced a smaller increase in the SL ratio and STR growth, with no effect on LTR growth. For example, a postal code region where 80% of the home-ownership proxy is satisfied, i.e. 80% are home-owners, was able to mitigate the short to long-term rental income growth by around 8%.

The empirical analysis suggests that the regulation on the short-term rental market in Berlin induced an increase in STR income growth, all the meanwhile, not being able to reduce rental prices (in the analysis it is denoted as long-term rental income). The ban on STRs increased the income for landlords who decided to remain in the market and exploit an environment of steady seasonal demand but decreased supply. Furthermore, I illustrate that saturated postal code areas were a mitigating factor of STR income acceleration, as well as areas with high home-ownership rates which where able to provide a buffer against such movements in STR income. Therefore commercial home-owners in these areas were more likely to offer their dwelling on the long term market than in the short term.

4.2 Effects on the Long-term Rental Market

Whereas I observe that the regulation did not manage to curb the growth rates of long-term rental prices within one year after its activation, I additionally investigate whether the increase in STR income had any second order effects on the long-term rental market.

As observed in Figure 8 LTR prices were on an increasing trend and continued to grow before and after the ban with no observable differences in trends. Under the findings in Table 2, landlords could ask for a price mark-up on their long-term rental prices.

The empirical methodology to assess the effect of STR growth rates on LTR growth is the following:

$$LTRgrowth_{p,t} = \alpha_y + \alpha_q + \beta Bnb\_ban_t + \gamma STRgrowth_{p,t-3} + \delta Ownership\_rate_{p,t} + \epsilon_{p,t} \quad (4)$$

Firstly, since I do not require data on the STR market in order to investigate whether the ban had any effect on LTR growth, I am able to extend the sample period to one year before the ban and two years afterwards. Secondly, in order to look at the effects of STR growth on the LTR market as a second order effect, I include quarterly lags of STR growth. These lags are arbitrarily defined, under the assumption that LTR prices timely react to STR growth, but not long afterwards. Table A.1 illustrates different sets of lags, in order to scrutinize the choice of a quarterly lag. Table 4 below illustrates the findings.

–Table 4 here–

In the first two columns, I duplicate the findings in Table 2 as I extend the period of analysis to one year before and two years after the activation of the ban. The findings suggest that the ban did not manage to curb the observed acceleration of rental income on the long-term market. Furthermore, I find supportive evidence of interdependence between growth in the STR and LTR markets, but the effect is rather marginal. I find that a quarterly lagged 1% increase in the STR income growth rate increases LTR prices by 0,002%. The effect is not profound, but as I observe in Table A.1, LTR and STR growth rates are positively correlated during the first quarter. Eventually the findings are slightly supportive of the second order effect hypothesis.

4.3 Spillovers on the Short-term Rental Market for Rooms

Berlin's regulation on the STR market intended to reduce the supply of housing on the short-term market and migrate these housing units to the long-term market. Unfortunately, I do not observe specific listings migrating from one market to the other, but I can investigate whether the decreased supply of STR units was accompanied by an increase in the supply of STR listings for rooms, which could be a substitute segment of the short-term housing market for landlords.

Figure 9 illustrates a hike in the supply of rooms to rent right after the ban, supporting the hypothesis that STR commercial home-owners who obliged to the new regulation preferred to offer less than 50% of their apartment on the STR market instead of migrating to the LTR market. If this assumption holds, then the regulation, did not manage to effectively increase the supply of LTR housing units. Also observed in the graph, rental rooms income had a slight increase, but not comparable to the one observed on STRs of entire apartments.

In line with the empirical identification of the regulatory effects on STR income for entire apartments Equation 1, I look into the progression of income growth for short-term rented apartments. Table 5 illustrates the results.

– Table 5 here –

I find no statistically significant effects of the regulation on the STR income for rooms. I although find a borderline significant negative relationship between the ban and room income. This might be due to the increased supply of STR room units. These effects are supportive of the hypothesis, but due to lack of significance, the findings cannot be conclusive. From a financing decision perspective though, the findings imply that there are no substitution effects between the two sub-categories of STR housing.

4.4 Spillovers on the Long-term Sales Market

Along Kim et al. (2017), where the authors find a value depreciation of non-tenant occupied dwellings following a regulation on the sharing economy for housing, I investigate whether the ban on STR housing units spilled-over the sales market for apartments. The regulation might have induced divesting in the sharing economy for housing, potentially decreasing the growth rate of house prices. In a similar identification fashion as in Table 4, I include long-term sales price growth as the dependent variable.

– Figure 8 here –

The results indicate that the sales market was inelastic to changes in the regulatory framework of STRs, as well as their supply and STR income growth. The visual evidence in Figure 8 show an ever increasing upward trend of sales prices for apartments, with no distinct differences between the period before or after the regulation was introduced. This explains the fact that I do not find any significant effects as well as is suggestive that sales prices, were experiencing high levels of growth that a regulation on STRs was not able to abate.

5 Robustness Checks

Although the regulation specifically mentions that its target was to reduce the supply of STR listings, suggesting an exogenous shock to rental price fluctuations, I perform a series of tests in order to potentially account for biased results due to endogeneity. The endogeneity may arise from two main factors. First, a second order intent of the regulators was to apply downward pressure to rental price hikes, by increasing the LTR housing stock, implying endogeneity to rental price growth. The second channel of endogeneity would be reverse causality between STR supply and LTR rental prices. Selective STR markets are usually condensed in gentrified, booming areas. Vice-versa, rental prices in areas where there are a lot of sharing economy housing units accelerate due to scarce supply.

5.1 Instrumental Regression

In order to tackle endogeneity, I first perform an instrumental variable regression, where I use the regulation as an instrument for STR housing supply on LTR rental prices. The first and second stage of the regression is as follows:

$$LTR_{p,t} = a + \beta S\hat{upply}_{p,t} + \gamma X_{r,y} + \epsilon_{p,t}$$

$$Supply_{p,t} = \alpha_y + \alpha_q + \delta Bnb\_ban_t + \upsilon_{p,t}$$

Table 7 illustrates the results.

– Table 7 here –

The results in Table 7 indicate the positive relation between STR housing supply and LTR rental levels and growth. In Columns 1 and 2, I find that if the supply of STR housing increases by 1%, then rental prices by around 5.6 euros. In Columns 3 and 4, I investigate the effects of increases in STR housing supply on rental price acceleration. I observe that a 1% increase in STR supply increases long-term rental price growth by 1,3%. The Kleibergen-Paap F-test for all tests has a value of more than 40, which indicates statistical instrument validity. Furthermore, the fact that I do not find any causal evidence in Table 2 between the regulation and long-term rental prices (which is interchangeably used as income as well) supports the exclusion restriction, that the regulation has an effect on rental prices only through the supply of STR housing units. The results are supportive of the findings in Duso et al. (2020), where the authors find that proximity of housing units to STR dwellings increases its rental price.

5.2 Generalized Method of Moments Regression

Another commonly used method of tackling endogeneity is a GMM Arellano-Bond estimator. Lagged values of the dependent variables are therefore used as instruments, to control for endogeneity. Usually, researchers use up to two lags of the dependent variable as these "internal instruments" (Schultz et al. (2010) and Wintoki et al. (2012) [18].).

Table 8 illustrates the GMM estimator results, where I include 2 lags of the dependent variable. Whereas the results on rental price levels indicate a much smaller relation deriving from increases in the supply of STR housing, a 1% increase in the supply of STR supply increases long-term rental price growth by 1,5%, 0,2 basis points more than the results indicated in Column 4 of Table 7.

– Table 8 here –

These results aim to supplement the findings in Table 4, where I find that an increase in STR income has a small and marginally significant effect on LT rental income growth. Since I use the terms long-term rental income and prices interchangeably, Table 7 suggests that an increase in STR supply applies pressure to rental prices. The regulation shortly managed to decrease STR supply which indicates that rental prices would decelerate. The cumulative findings though suggest that rental prices where quite inelastic in response to this decrease in STR supply, which mostly derives from the minuscule inflow of new LTR housing units right after the regulation.

6 Conclusion

This paper aims to shed light onto potential adverse effects of the regulation on the STR market on household financing incentives. Using the case of Berlin for identification, where a partial ban was implemented and then lifted after two years due to its inability to control the supply of STRs on the market, I investigate whether the ban increased STR income for those who stayed in the market even after the regulation was activated, thus creating higher incentives for landlords to rent their dwelling on the short-term rental market. The regulation aimed to reduce the supply of STRs to zero, by funneling new

[18] A comprehensive analysis of how a GMM estimator can deal with endogeneity bias can be found in Ullah et al. (2018)

housing units into the long-term rental market and by the means of increasing supply, mitigating an accelerating trend of rental prices.

Due to the fact that demand was staggered and that the enforcing authorities failed to impose fines to non-conforming landlords, those who remained experienced an increase in their STR income up to 50%. This increase deemed the STR market more attractive from a financial point of view, and consequently failed to decrease long-term rental income. Furthermore, I show that areas with high levels of STR supply as well as high home-ownership rates are able to dampen STR income acceleration. The first can be explained by excessive STR supply in the first case, or else a lemon scenario, where STR units who were not generating income due to overflowing supply, could leave the market without creating any significant difference in the supply demand equilibrium. Secondly, in a diverse scenario, areas with high home-ownership rates are likely to provide a buffer to STR acceleration, since demand in these areas should be lower, as I explain in the main body of the paper.

These results provide evidence that regulators who want to contain the negative externalities of the STR market, have to consider the heterogeneous effects across spatial units. The STR market is endogenous to rental hikes and is more likely to be a larger part of the total number of housing units in cultural, historical and financial centers. Imposing a city-wide regulation might not only have little effect, but might also hurt landlords who use the sharing economy to generate income in areas where there STR market is small enough to meet demand, and therefore does not have enough capacity to push rental prices upwards.

I furthermore find that STR growth has a positive effect on pushing LTR prices after one quarter but the effect is rather minuscule. On the other side, there seems to be no significant relation between the regulation on STRs, or STR supply on apartment sales price growth. Investigating migration patterns from one sub-division of the STR market to the other, namely, from entire apartments to rooms which satisfy the less than 50% of the housing unit rule, I observe an increase in the supply of rooms to rent on the STR market and a marginally insignificant negative growth rates on their income.

As suggested in the introduction, a second order effect of the regulation could be that rental prices, in response to accelerating STR income, impose a mark-up on new contracts. The evidence on the short-term are not suggestive of such behavior, but possibly these effects incubate for a longer period of time. These findings provide a ex-post overview of the realized outcomes of a regulation on the STR market. With the empirical research on the sharing economy for housing being relatively new, further analysis of the effects of STR regulation is important both from a financial and social perspective.

Table 1: Summary statistics

	Full Sample						Pre Ban Period			Post Ban Period			T-test(Pre-Post)	
	Mean	Std.Dev.	25th	Median	75th	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Delta	p-value
Listing summary statistics														
STR income	1590.99	1210.67	547.00	1554.00	2252.00	559998	1768.95	1146.19	144290	1529.22	1226.24	415708	239.73***	(0.00)
LTR income	797.57	379.66	540.00	712.61	964.00	559998	755.97	381.56	144290	812.00	377.93	415708	-56.03***	(0.00)
STR/LTR ratio	2.25	1.77	0.75	1.98	3.24	559998	2.66	1.82	144290	2.10	1.73	415708	0.55***	(0.00)
Bathrooms	1.02	0.13	1.00	1.00	1.00	559905	1.02	0.14	144231	1.02	0.13	415674	0.00	(0.18)
Bedrooms	1.15	0.39	1.00	1.00	1.00	558335	1.16	0.39	143650	1.15	0.39	414685	0.01***	(0.00)
Cleaning fee	32.12	17.03	20.00	30.00	40.00	417387	32.10	16.21	109448	32.13	17.31	307939	-0.04	(0.56)
Postal code summary statistics														
STR/LTR growth	0.22	1.88	-0.2	-0.0	0.2	2170	0.04	2.20	307	0.24	1.82	1863	-0.20	(0.08)
STR income growth	0.21	1.82	-0.1	0.0	0.2	2170	0.03	1.91	307	0.24	1.80	1863	-0.21	(0.06)
LTR income growth	0.03	0.22	-0.1	0.0	0.1	2170	0.01	0.17	307	0.03	0.23	1863	-0.02	(0.27)
Sales/Total	0.44	0.20	0.3	0.4	0.6	2615	0.48	0.21	642	0.43	0.20	1973	0.04***	(0.00)
Rentals/Total	0.56	0.20	0.4	0.6	0.7	2615	0.52	0.21	642	0.57	0.20	1973	-0.04***	(0.00)
STR Supply	40.47	58.30	5.0	16.0	45.0	2472	41.01	57.98	615	40.29	58.42	1857	0.72	(0.79)

This table shows descriptive statistics for the main characteristics of online adverts both on the STR and LTR market. The second panel of the table illustrates aggregated postal code level summary statistics, namely, the growth variables of STR and LTR income, as well as the growth of their ratio. There are 186 unique postal codes in the dataset. We show full sample statistics and distinguish between pre and post ban period. In the last two columns we perform a t-test of the mean differences between the two periods and report the first difference and its statistical significance. STR supply refers to the amount of online adverts on average by date across all postal codes. Cleaning fee is in euros.

Table 2: The effects of the regulation on short and long-term income growth

	(1)	(2)	(3)	(4)	(5)	(6)
	SLgrowth		STR income growth		LTR income growth	
Bnb_ban	0.599***	0.473**	0.597***	0.449**	0.004	0.002
	(0.216)	(0.218)	(0.200)	(0.201)	(0.018)	(0.017)
Supply(ln)		−0.546**		−0.645**		−0.009*
		(0.232)		(0.272)		(0.005)
District Controls						
New Permits(ln)	−0.171	−0.244	−0.280	−0.367	−0.001	−0.002
	(0.225)	(0.236)	(0.326)	(0.339)	(0.027)	(0.026)
Overnight(ln)	−0.046	0.022	−0.057	0.023	0.006	0.007
	(0.124)	(0.142)	(0.124)	(0.142)	(0.013)	(0.012)
New Construction(ln)	−0.003	−0.173	−0.051	−0.252	−0.003	−0.006
	(0.212)	(0.260)	(0.205)	(0.265)	(0.017)	(0.017)
Constant	1.285	3.472	2.340	4.921	−0.032	0.005
	(1.572)	(2.130)	(2.373)	(3.118)	(0.196)	(0.198)
Observations	2251	2251	2251	2251	2251	2251
R-Squared	0.044	0.064	0.039	0.058	0.015	0.015
Adjusted R-Squared	0.017	0.036	0.010	0.030	−0.014	−0.014
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Regional F.E.	*District*	*District*	*District*	*District*	*District*	*District*
Cluster S.E.	*District*	*District*	*District*	*District*	*District*	*District*

This Table shows the baseline results of the effects of the STR regulation on STR and LTR income growth. The main variable of interest –Bnb_ban– is a dummy indicator for the period after the ban on short-term listings was activated. Variable –Supply– is a logarithmic transformation of the total supply of STR housing per postal code and month. The results derive from an unbalanced dataset of 186 postal codes by 16 monthly observations. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional autocorrelation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: The effects of the regulation on short and long-term income growth, conditional on home-ownership

	(1)	(2)	(3)	(4)	(5)	(6)
	SLgrowth		STR income growth		LTR income growth	
Bnb_ban	0.606**	0.481*	0.620**	0.472*	0.014	0.012
	(0.265)	(0.266)	(0.240)	(0.238)	(0.019)	(0.019)
Bnb_ban=0 × Ownership_rate	−0.155	−0.105	−0.158	−0.099	0.013	0.014*
	(0.173)	(0.145)	(0.151)	(0.122)	(0.008)	(0.008)
Bnb_ban=1 × Ownership_rate	−0.161*	−0.109*	−0.177**	−0.116	0.004	0.005
	(0.084)	(0.062)	(0.084)	(0.073)	(0.004)	(0.004)
Supply(ln)		−0.533**		−0.631**		−0.010*
		(0.230)		(0.272)		(0.005)
District Controls						
New Permits(ln)	−0.171	−0.242	−0.279	−0.364	−0.000	−0.001
	(0.222)	(0.236)	(0.324)	(0.340)	(0.027)	(0.026)
Overnight(ln)	−0.058	0.012	−0.069	0.014	0.007	0.008
	(0.118)	(0.137)	(0.119)	(0.139)	(0.013)	(0.012)
New Construction(ln)	−0.012	−0.175	−0.062	−0.255	−0.004	−0.007
	(0.211)	(0.258)	(0.205)	(0.263)	(0.017)	(0.017)
Constant	1.688	3.695*	2.763	5.138*	−0.058	−0.020
	(1.553)	(2.078)	(2.359)	(3.049)	(0.195)	(0.197)
Observations	2251	2251	2251	2251	2251	2251
R-Squared	0.046	0.064	0.040	0.059	0.015	0.016
Adjusted R-Squared	0.018	0.036	0.011	0.030	−0.015	−0.015
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Regional F.E.	*District*	*District*	*District*	*District*	*District*	*District*
Cluster S.E.	*District*	*District*	*District*	*District*	*District*	*District*

This Table shows the effects of STR regulation on STR and LTR income growth. The dependent variable is long-term rental prices growth. The main variable of interest –Bnb_ban– is a dummy indicator for the period after the ban on short-term listings was activated. Ownership_rate is the ratio of long-term listings for sale over the total housing stock in postal code level. Variable –Supply– is a logarithmic transformation of the total supply of STR housing per postal code and month. Columns 2,4 and 6 illustrate heterogeneous pattern with regards to different levels of homeownership rates. The results derive from an unbalanced dataset of 186 postal codes by 16 monthly observations. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional autocorrelation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The effects of the regulation on long-term rental price growth

	(1)	(2)	(3)	(4)
	LTRgrowth			
Bnb_ban	0.009	0.010		
	(0.008)	(0.008)		
$STRgrowth_{t-3}$			0.002*	0.002*
			(0.001)	(0.001)
District Controls				
New Permits(ln)	−0.002	−0.002	−0.001	−0.001
	(0.004)	(0.004)	(0.004)	(0.005)
Overnight(ln)	0.001	0.002	0.003	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
New Construction(ln)	0.003	0.003	−0.003	−0.003
	(0.006)	(0.006)	(0.008)	(0.008)
Ownership_rate		0.020*		0.014
		(0.011)		(0.010)
Constant	−0.033	−0.050	−0.007	−0.019
	(0.037)	(0.037)	(0.034)	(0.035)
Observations	2422	2417	1729	1725
R-Squared	0.011	0.012	0.021	0.022
Adjusted R-Squared	−0.016	−0.015	−0.015	−0.015
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Regional F.E.	*District*	*District*	*District*	*District*
Cluster S.E.	*District*	*District*	*District*	*District*

This Table shows the effects of STR regulation on LTR prices, as well as the effects of STR income growth on LTR prices. The dependent variable is long-term rental prices growth. The main variable of interest –Bnb_ban– is a dummy indicator for the period after the ban on short-term listings was activated. Variable STRgrowth_{t-3} indicates the growth in short-term rental listings supply in the last quarter. The difference in the observation count between Column 1, 2 and 3, 4 is due to the limited time span if the STR data. The results derive from an unbalanced dataset of 186 postal codes by 16 monthly observations. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional autocorrelation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: The effects of the regulation on short-term rental income growth for rooms

	(1)	(2)	(3)
		STR growth	
Bnb_ban	−0.117	−0.119	0.058
	(0.124)	(0.117)	(0.192)
Ownership_rate			0.021
			(0.253)
Bnb_ban × Ownership_rate			−0.432
			(0.347)
Supply(ln)		−0.100***	
		(0.033)	
District Controls			
New Permits(ln)	−0.326	−0.028	−0.055
	(0.291)	(0.108)	(0.109)
Overnight(ln)	−0.250	−0.027	−0.092
	(0.231)	(0.078)	(0.072)
New Construction(ln)	0.413	0.087	0.081
	(0.310)	(0.106)	(0.101)
Constant	2.457	0.445	1.303
	(2.659)	(1.178)	(1.178)
Observations	2362	2362	2362
R-Squared	0.049	0.009	0.007
Adjusted R-Squared	0.022	0.006	0.003
Year F.E.	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*
District F.E.	*District*	*District*	*District*
Cluster S.E.	*District*	*District*	*District*

This Table shows the effects of STR regulation on STR income growth for listed rooms. The dependent variable is short-term rental income growth for rooms. The main variable of interest –Bnb_ban– is a dummy indicator for the period after the ban on short-term listings was activated. Ownership_rate is the ratio of long-term listings for sale over the total housing stock in postal code level. Variable –Supply– is a logarithmic transformation of the total supply of STR housing per postal code and month. The results derive from an unbalanced dataset of 186 postal codes by 16 monthly observations. Supply is the logarithmic transformation of the number of short-term rental listings in each postal code and month. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional autocorrelation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: The effects of the regulation on long-term purchase price growth

	(1)	(2)	(3)	(4)	(5)	(6)
	Long-term Sales Price Growth					
Bnb_ban	0.002	−0.009				
	(0.007)	(0.013)				
$STRgrowth_{t-3}$			0.000	0.001		
			(0.003)	(0.004)		
$STRsupplygrowth_{t-1}$					0.009	0.024
					(0.015)	(0.023)
District Controls						
New Permits(ln)	−0.005	−0.007	−0.001	−0.011	0.003	−0.007
	(0.004)	(0.008)	(0.010)	(0.012)	(0.004)	(0.010)
Overnight(ln)	−0.007*	−0.012*	−0.004	−0.006	−0.011***	−0.017**
	(0.004)	(0.006)	(0.009)	(0.010)	(0.004)	(0.007)
New Construction(ln)n	0.008**	0.016	0.001	0.001	0.006	0.020*
	(0.003)	(0.011)	(0.015)	(0.017)	(0.006)	(0.012)
Ownership_rate		0.020*		0.014		
		0.048**		0.041**		0.055**
		(0.023)		(0.019)		(0.027)
Constant	0.085	0.103	0.079	0.144	0.123**	0.124
	(0.063)	(0.078)	(0.116)	(0.148)	(0.052)	(0.091)
Observations	7060	2358	2307	1732	3821	2029
R-Squared	0.005	0.015	0.013	0.020	0.007	0.018
Adjusted R-Squared	−0.005	−0.013	−0.015	−0.017	−0.010	−0.015
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Regional F.E.	*District*	*District*	*District*	*District*	*District*	*District*
Cluster S.E.	*District*	*District*	*District*	*District*	*District*	*District*

This Table shows the effects of the STR regulation on long-term purchase prices for apartments. The dependent variable is long-term rental price growth. The main variable of interest –Bnb_ban– is a dummy indicator for the period after the ban on short-term listings was activated. Variables $STRgrowth_{t-1}$ and $\mathrm{STRgrowth}_{t-3}$ indicates the growth in short-term rental listings supply in the last month and quarter respectively. The results derive from an unbalanced dataset of 186 postal codes by 16 monthly observations. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional autocorrelation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: The effects of short-term rental supply on long-term rental prices - IV estimator

	(1)	(2)	(3)	(4)
	LTR prices		LTR price growth	
Supply (ln)	6.165***	5.617***	0.013***	0.013***
	(0.462)	(0.395)	(0.003)	(0.004)
New Permits		1.357***		0.001
		(0.353)		(0.003)
Overnight Stays		0.360		−0.001
		(0.639)		(0.003)
New Construction		0.456**		0.003*
		(0.203)		(0.002)
Observations	4884	4588	4883	4587
Kleibergen - Paap F-test	57.734	48.170	59.966	51.498
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Regional F.E.	*None*	*District*	*District*	*District*
Cluster S.E.	*District*	*District*	*District*	*District*

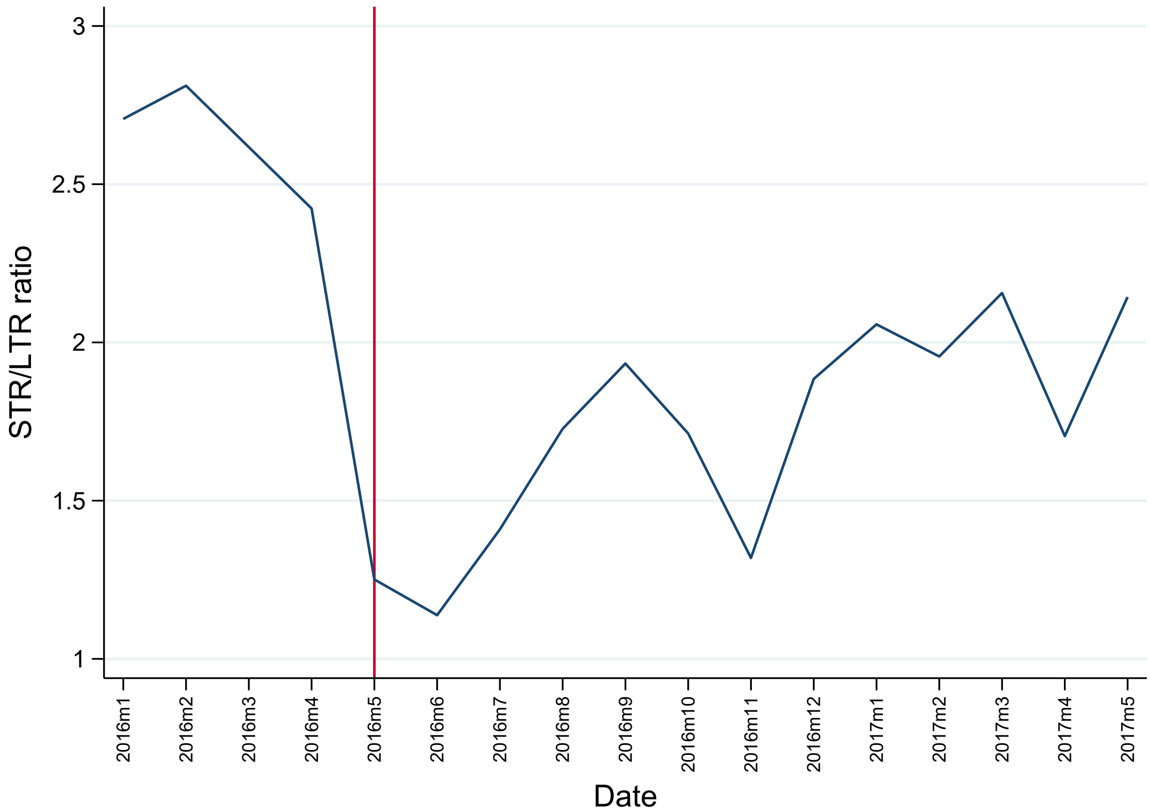
This table shows an IV regression of the effects of STR supply on long-term rental prices. The main variable of interest –Supply– is a logarithmic transformation of the total supply of STR housing per postal code and month. The dependent variable is the level of long-term rental prices per postal code (Columns (1) and (2)) and the long-term rental price growth (Columns (3) and (4)). The results derive from an unbalanced dataset of 186 postal codes by 16 monthly observations. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional autocorrelation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: The effects of the regulation on long-term rental price growth - GMM estimator

	(1)	(2)	(3)	(4)
	LTR prices		LTR price growth	
$LTRprices_{t-1}$	0.254***	0.251***	−0.488***	−0.487***
	(0.002)	(0.005)	(0.003)	(0.003)
$LTRprices_{t-2}$	0.134***	0.123***	−0.203***	−0.201***
	(0.002)	(0.005)	(0.003)	(0.003)
Supply (ln)	0.124***	0.049***	0.017***	0.015***
	(0.010)	(0.016)	(0.001)	(0.002)
New Permits		−0.262***		−0.021***
		(0.023)		(0.004)
Overnight Stays		4.282***		0.111
		(0.469)		(0.126)
New Construction		0.210***		0.025***
		(0.017)		(0.004)
Constant	6.015***	−55.443***	−0.028***	−1.692
	(0.040)	(6.764)	(0.002)	(1.815)
Observations	4247	3984	4247	3984
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Regional F.E.	*District*	*District*	*District*	*District*
Estimator	*GMM*	*GMM*	*GMM*	*GMM*

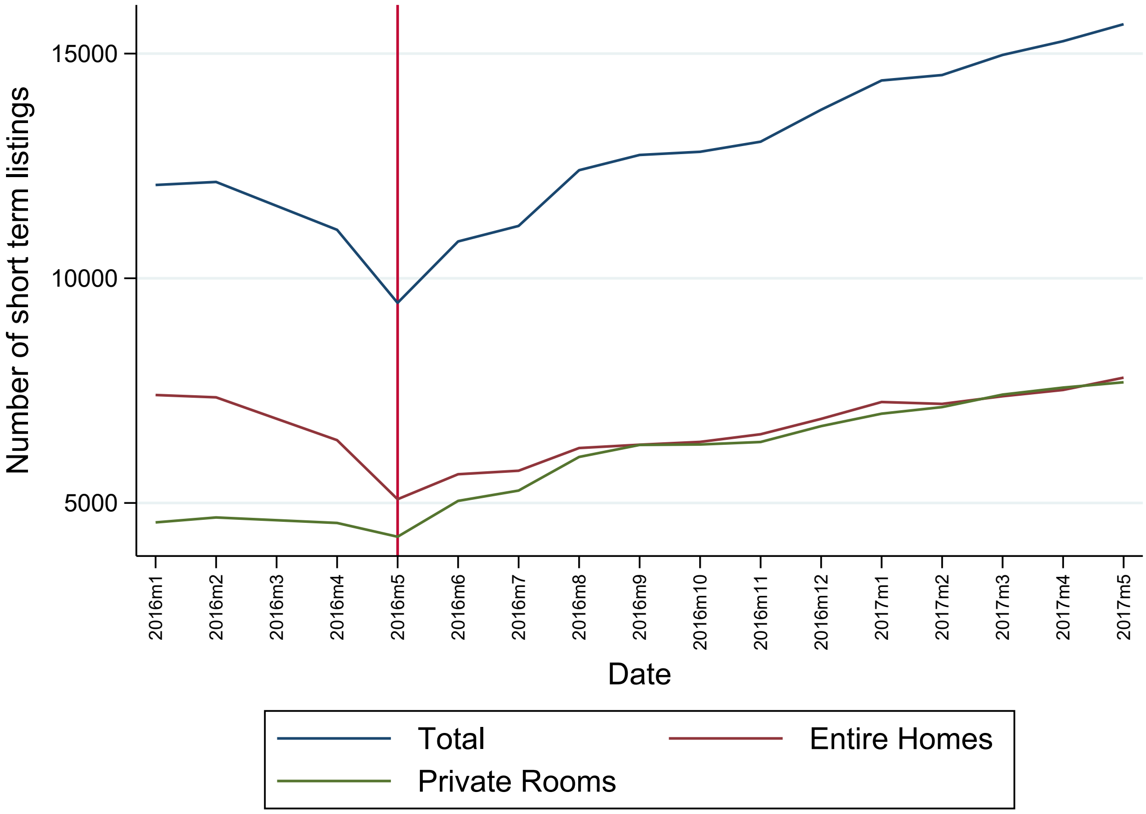
This Table shows the effects of STR regulation on STR income growth for listed rooms using a two-step GMM Arellano-Bond estimator. The dependent variable is long-term rental price growth, autoregressed by two lags. The main variable of interest –Supply– is the logarithmic transformation of the total number of short-term rental housing in each postal code and month. The results derive from an unbalanced dataset of 186 postal codes by 16 monthly observations. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional autocorrelation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Median short-term rental income and counterfactual median long-term rental income ratio



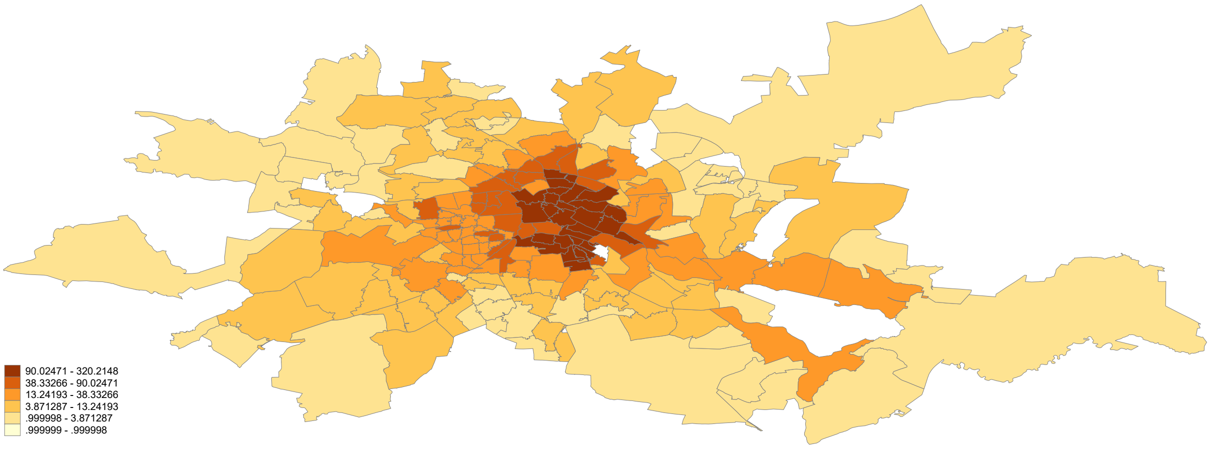
This figure illustrates the median short to long-term rental income from January 2016 to May 2017. The red vertical line indicates the activation of the ban on short-term apartment listings.

Figure 2: Total number of short-term listings per category



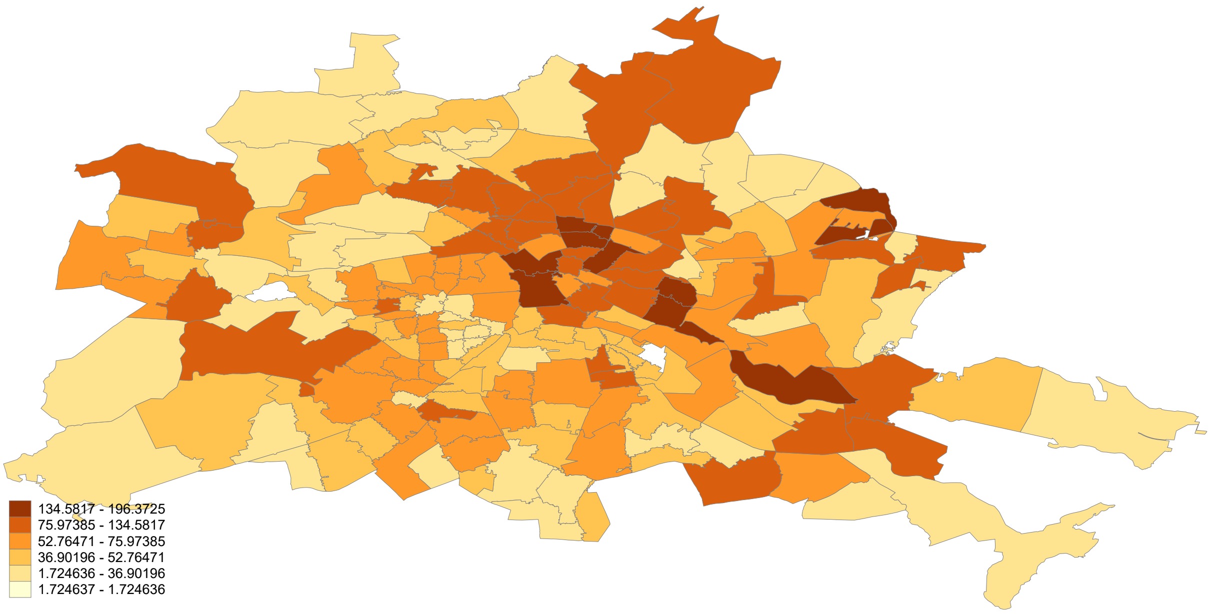
This figure illustrates the total number of short-term rental listings from January 2016 to May 2017. The red vertical line indicates the activation of the ban on short-term apartment listings.

Figure 3: Housing Stock Supply - Short-term entire apartments for rent



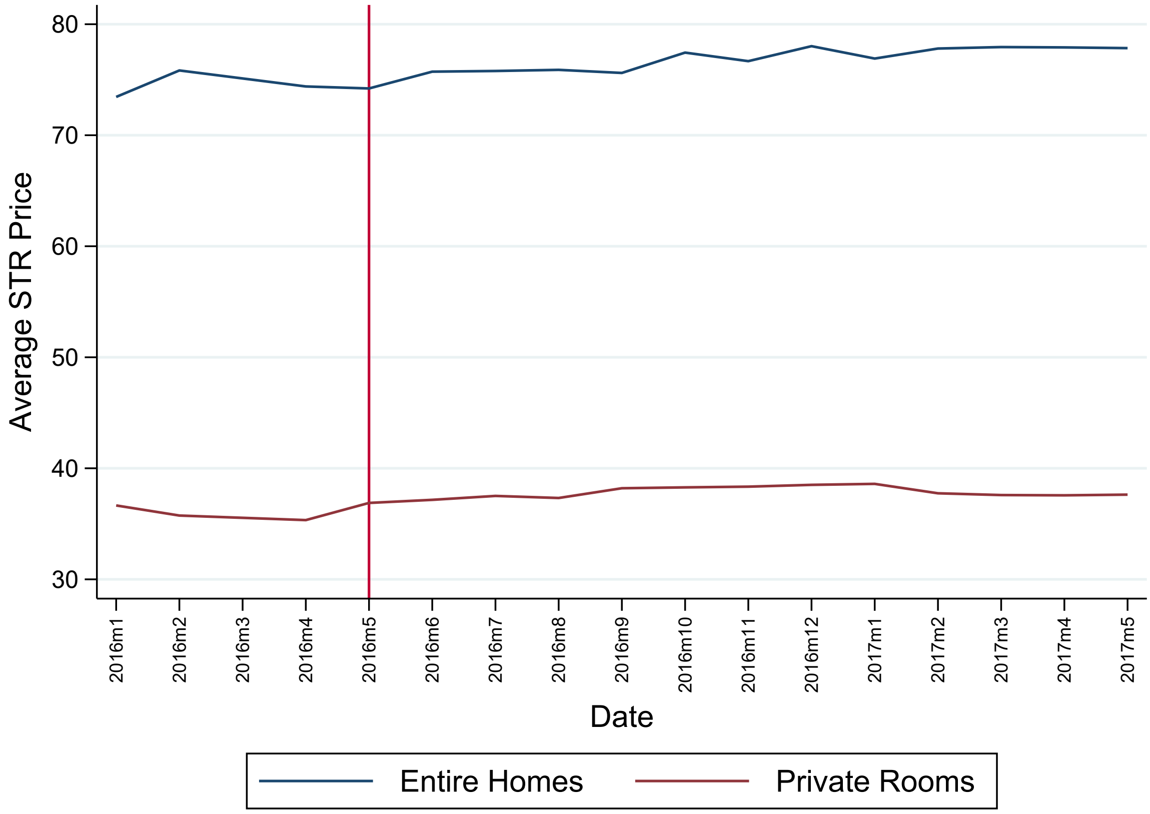
This map illustrates the average number of short-tert rental listings by postal code throughout the sample period of January 2016 to May 2017. Boxplot values are displayed in legend.

Figure 4: Housing Stock Supply - Long-term apartments for rent



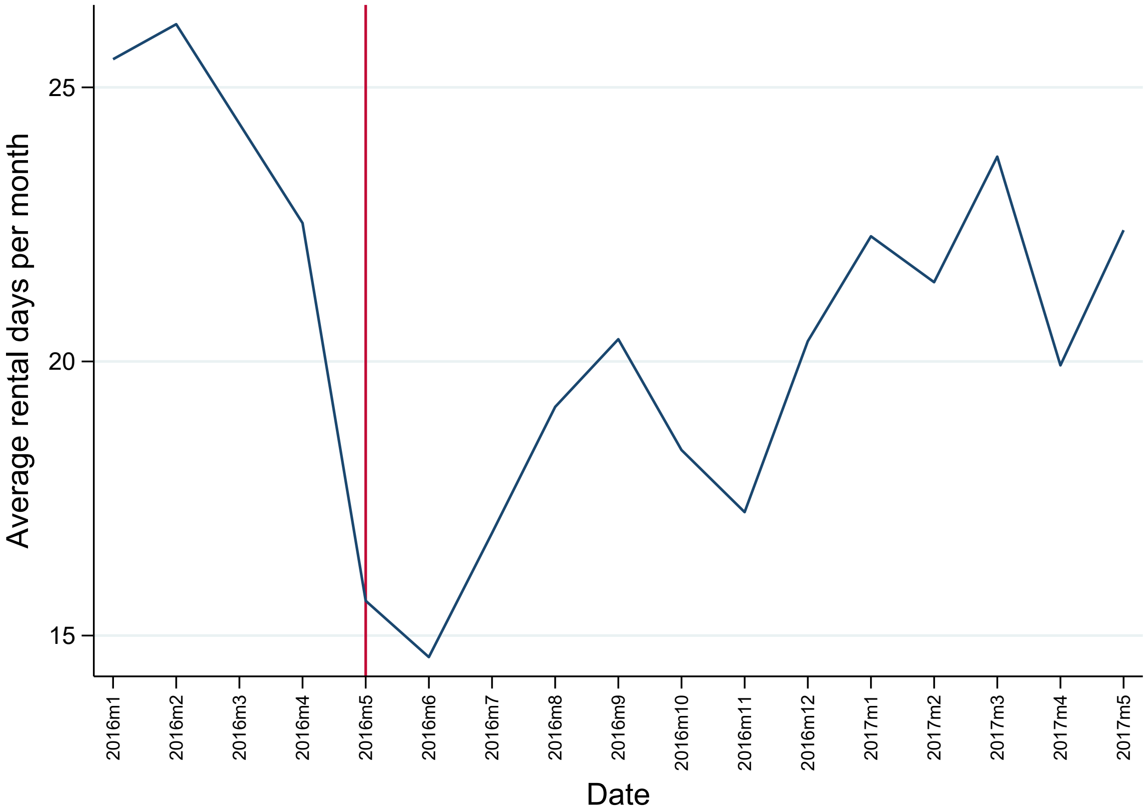
This map illustrates the average number of long-term rental apartments by postal code throughout the sample period of January 2016 to May 2017. Boxplot values are displayed in legend.

Figure 5: Average asking short-term rental prices



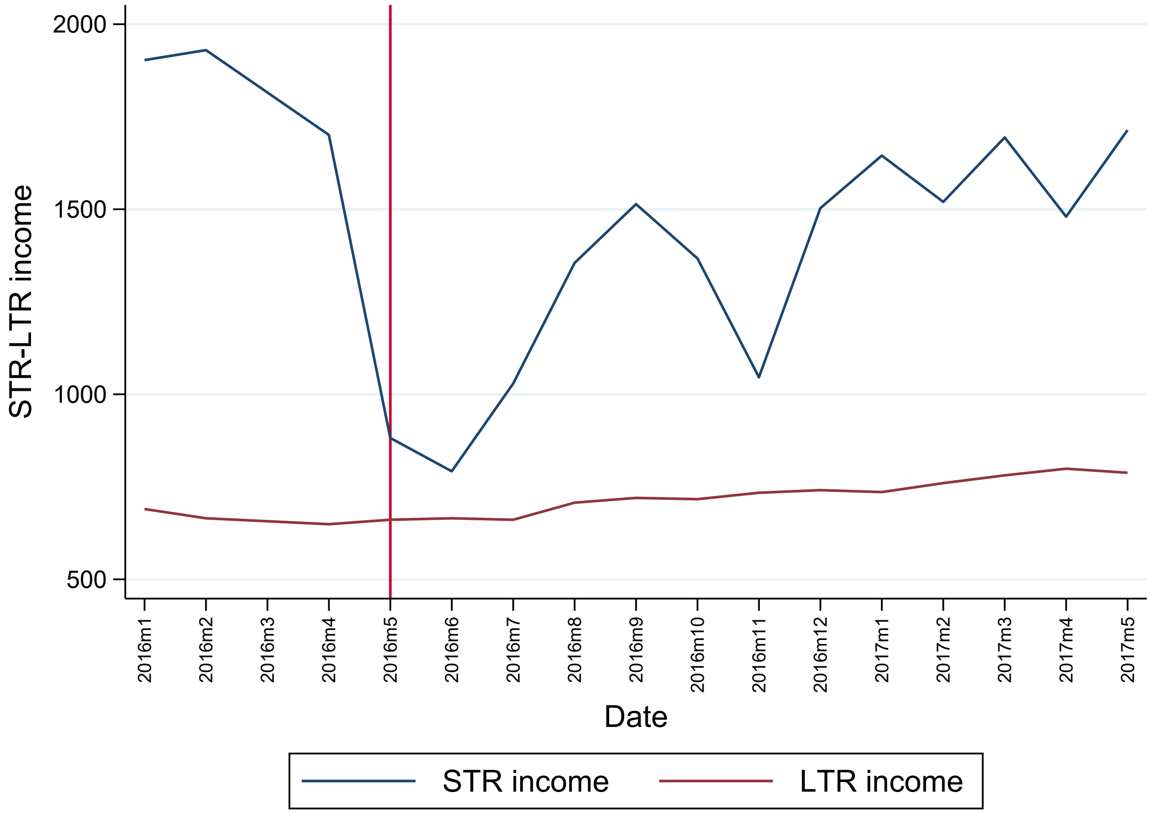
This figure illustrates the average asking short-term rental price from January 2016 to May 2017. The red vertical line indicates the activation of the ban on short-term apartment listings.

Figure 6: Average days rented by apartment as a housing demand indicator



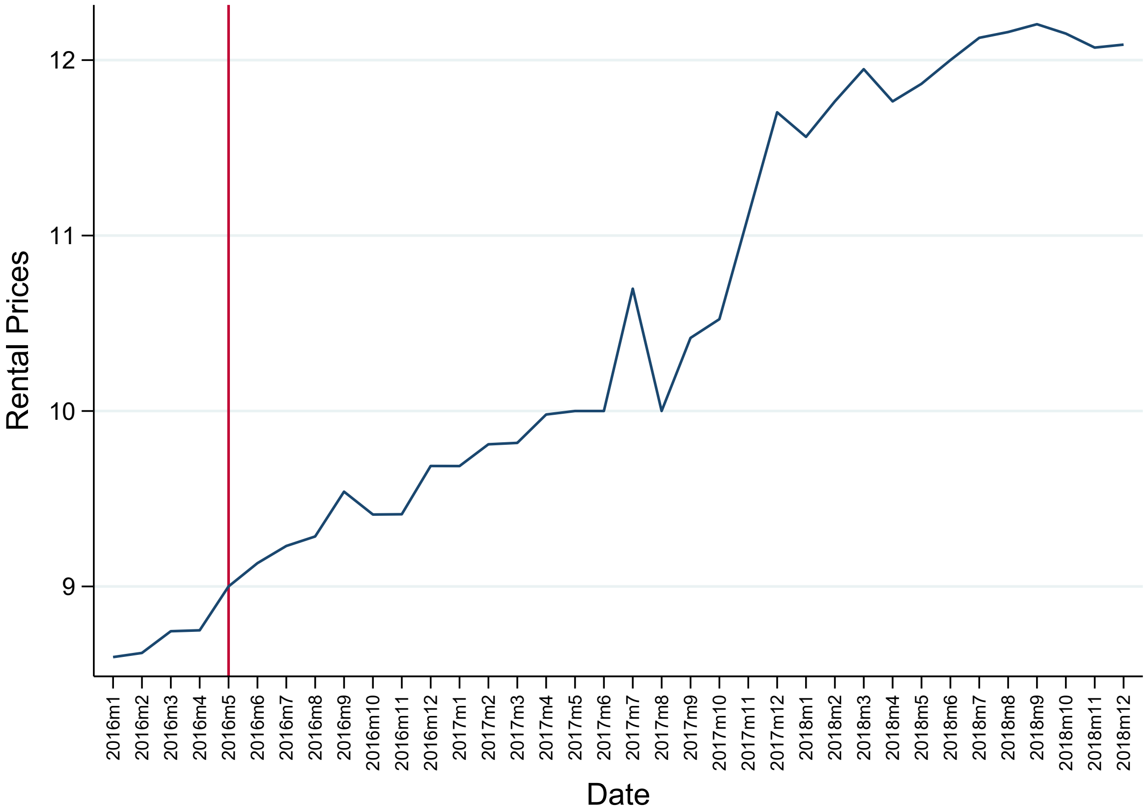
This figure illustrates the average short-term rented days from January 2016 to May 2017. The red vertical line indicates the activation of the ban on short-term apartment listings.

Figure 7: Average short and long-term rental income



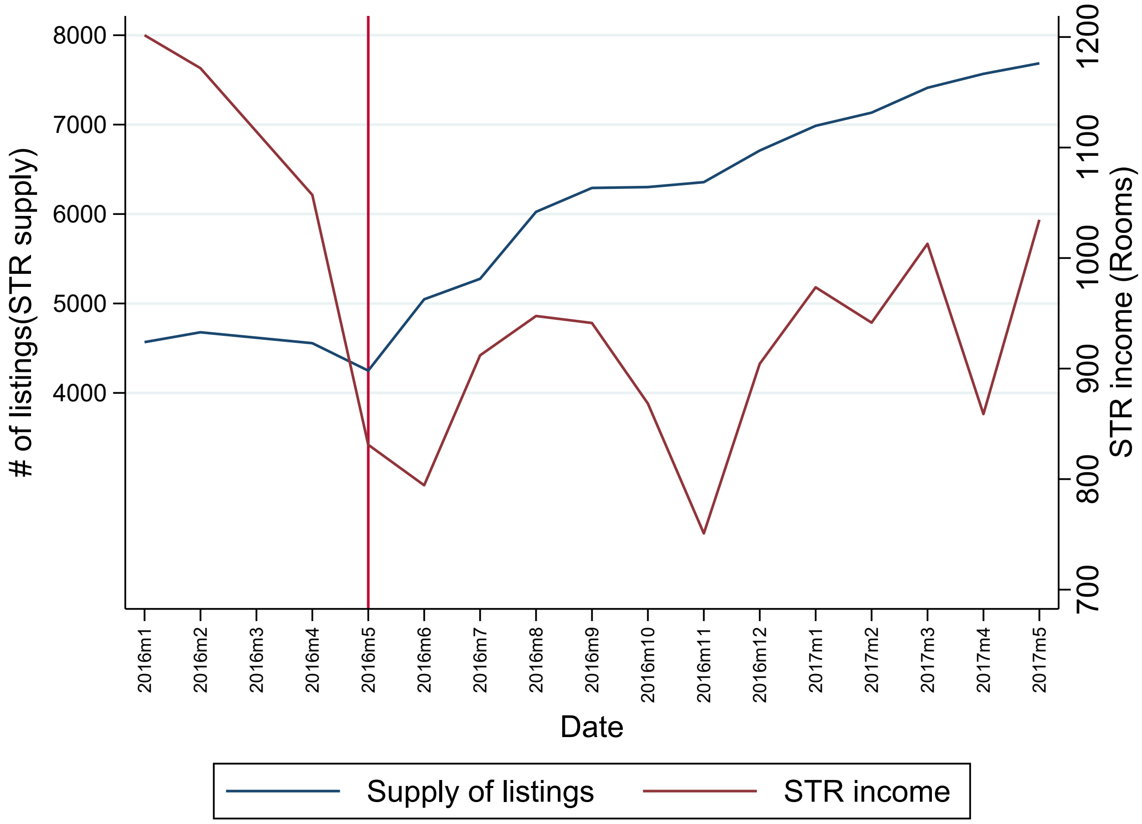
This figure illustrates the average short-term rental and average long-term rental income from January 2016 to May 2017. The red vertical line indicates the activation of the ban on short-term apartment listings.

Figure 8: Average long-term rental price levels



This figure illustrates the average long-term rental prices from January 2016 to May 2017. The red vertical line indicates the activation of the ban on short-term apartment listings.

Figure 9: Total supply of short-term rental rooms and average generated short-term rental income



This figure illustrates the average number of short-term rentals and average STR income for rooms from January 2016 to May 2017. The red vertical line indicates the activation of the ban on short-term apartment listings.

Figure 10: Average long-term sales price levels

This figure illustrates the average long-term purchase prices from January 2016 to May 2017. The red vertical line indicates the activation of the ban on short-term apartment listings.

Appendix

Robustness Checks

Table A.1: Short-term rental income growth on long-term rental income growth - Event analysis

	(1)	(2)	(3)	(4)
		Inc_growth		
STR growth$_{t-1}$	0.002*			
	(0.001)			
STR growth$_{t-2}$	0.000			
	(0.001)			
STR growth$_{t-3}$	0.001			
	(0.001)			
STR growth$_{t-4}$		−0.001		
		(0.001)		
STR growth$_{t-5}$		−0.000		
		(0.001)		
STR growth$_{t-6}$		−0.000		
		(0.001)		
STR growth$_{t-7}$			0.000	
			(0.001)	
STR growth$_{t-8}$			0.001	
			(0.001)	
STR growth$_{t-9}$			−0.001	
			(0.001)	
STR growth$_{t-10}$				−0.001
				(0.001)
STR growth$_{t-11}$				0.002
				(0.002)
STR growth$_{t-12}$				0.004
				(0.004)
District Controls				
New Permits(ln)	−0.000	−0.006	0.015	0.024
	(0.003)	(0.005)	(0.009)	(0.022)
Overnight(ln)	0.000	0.001	0.008	0.016
	(0.002)	(0.003)	(0.006)	(0.013)
New Construction(ln)	0.000	0.006	−0.016	−0.026
	(0.003)	(0.006)	(0.011)	(0.021)
Constant	0.009	−0.020	−0.064	−0.145
	(0.028)	(0.037)	(0.056)	(0.154)
Observations	1179	851	542	215
R-Squared	0.015	0.028	0.103	0.191
Adjusted R-Squared	−0.040	−0.047	−0.005	−0.069
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Regional F.E.	*District*	*District*	*District*	*District*
Cluster S.E.	*District*	*District*	*District*	*District*

This Table is a robustness check with regard to the choice of lag in Table 4. The dependent variable is long-term rental income(price) growth. The main variable of interest –STR growth– is the short-term rental income growth. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional auto-correlation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: The effects of the regulation on short and long-term income growth, conditional on the supply of short-term housing over population

	(1) SL growth	(2) STR income growth	(3) LTR income growth
Bnb_ban	0.658**	0.668***	−0.001
	(0.250)	(0.232)	(0.019)
Bnb_ban=0 × STRcapita	−0.019	−0.027	−0.004
	(0.030)	(0.029)	(0.003)
Bnb_ban=1 × STRcapita	−0.072**	−0.091**	−0.003***
	(0.029)	(0.037)	(0.001)
District Controls			
New Permits(ln)	−0.179	−0.185	0.020
	(0.128)	(0.132)	(0.013)
Overnight(ln)	−0.216**	−0.189**	0.010
	(0.094)	(0.089)	(0.007)
New Construction(ln)	0.160	0.117	−0.014
	(0.132)	(0.149)	(0.011)
Constant	2.624**	2.616**	−0.102
	(1.250)	(1.269)	(0.093)
Observations	2251	2251	2251
R-Squared	0.014	0.011	0.001
Adjusted R-Squared	0.010	0.006	−0.003
Year F.E.	*Yes*	*Yes*	*Yes*
Quarter F.E.	*Yes*	*Yes*	*Yes*
Regional F.E.	*District*	*District*	*District*
Cluster S.E.	*District*	*District*	*District*

This Table shows population weighted regressions of the effects of the STR regulation on the SL ratio ,STR and LTR income growth. The main variable of interest –Bnb_ban– is a dummy indicator for the period after the ban on short-term listings was activated. Variable STRcapita is a postal code specific measure of short-term listings per capita. The results derive from an unbalanced dataset of 186 postal codes by 16 monthly observations. We include yearly and quarterly fixed effects to account for time trends and seasonality and we cluster standard errors at the district level, in order to allow cross-sectional autocorrelation between residuals among postal codes in the same district. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Variable definition

Variable name	Source	Unit	Frequency	Level	Description
Primary dependent variables: House Prices					
STR income	insideairbnb.com	Euros	Monthly(01:2016-06:2017)	Postal code	Derived from Calendar dataset; See 3
LTR income	immobilienscout24.de;	Euros	Monthly(01:2007-12:2019)	Postal code	Asking rental prices for online adverts
Matching variables					
Postal code	InsideAirbnb, Immo	Integer	Monthly(01:2016-06:2017) Monthly(01:2007-12:2019)	-	5 digit Berlin postal codes
Date	InsideAirbnb, Immo	Date	Monthly	-	Dates need to be standardized across datasets
Bathrooms	InsideAirbnb, Immo	Integer	Monthly(01:2016-06:2017) Monthly(01:2007-12:2019)	Housing Unit	# of bathrooms
Bedrooms	InsideAirbnb, Immo	Integer	Monthly(01:2016-06:2017) Monthly(01:2007-12:2019)	Housing Unit	# of bedrooms
Bank Controls lagged by one quarter					
STR supply	insideairbnb.com	Integer	Monthly(01:2016-06:2017)	Postal code	# of STR listings per postal code
Sales/Total	immobilienscout24.de;	Ratio	Monthly(01:2007-12:2019)	Postal code	# of sales over total
Rentals/Total	immobilienscout24.de;	Ratio	Monthly(01:2007-12:2019)	Postal code	# of rentals over total housing

This Table shows definitions and sources of the variables.

Table A.4: Areas and districts in Berlin

Area	Code	District Code	Area	Code	District Code
Mitte	101	1011	**Berlin-Köpenick**	124	1243
	101	1017		124	1245
Friedrichshain	102	1024		124	1248
Friedrichsfelde	103	1031	**Berlin-Köpenick**	125	1252
	103	1036		125	1255
Prenzlauer Berg	104	1040		125	1258
	104	1043	**Berlin-Marzahn**	126	1261
Charlottenburg	105	1055		126	1262
	105	1058		126	1267
Berlin-Charlottenburg	106	1062		126	1268
Kreuzberg	107	1070	**Berlin-Gesundbrunnen**	130	1305
	107	1071		130	1308
	107	1077	**Berlin-Pankow**	131	1312
	107	1078		131	1315
Schöneberg	108	1082		131	1318
Neukölln	109	1096	**Berlin-Gesundbrunnen**	133	1334
	109	1099		133	1335
Berlin-Neukölln	120	1204	**Berlin-Gesundbrunnen**	134	1340
	120	1205		134	1343
	120	1209		134	1346
Berlin-Schöneberg	121	1210	**Berlin-Reinickendorf**	135	1350
	121	1215		135	1358
	121	1216		135	1359
Berlin-Lichterfelde	122	1220	**Berlin-Tegel**	136	1362
	122	1224	**Potsdam**	140	1405
	122	1227		140	1408
Berlin-Lichtenrade	123	1230	**Berlin-Wilmersdorf**	141	1410
	123	1234		141	1412
	123	1235		141	1416
				141	1419

This Table illustrates the areas and districts of Berlin included in the sample. A visualization of the above can be seen in Figure 3. There are in total 24 areas and 59 districts within our sample. Some areas share the same administrative name, but they are classified as different areas according to https://www.suche-postleitzahl.org/plz-gebiet/10.

References

Barron, K., E. Kung, and D. Proserpio (2018). The effect of home-sharing on house prices and rents: Evidence from airbnb. *Available at SSRN 3006832*.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics 90*(3), 414–427.

Coldwell, W. (2017). First venice and barcelona: Now anti-tourism marches spread across europe. *The Guardian 10*, 2017.

Dinkel, M. and B. Kurzrock (2012). Asking prices and sale prices of owner-occupied houses in rural regions of germany. *Journal of Interdisciplinary Property Research 13*(1), 5–25.

Dredge, D. and S. Gyimóthy (2017). *Collaborative economy and tourism: Perspectives, politics, policies and prospects*. Springer.

Duso, T., C. Michelsen, M. Schäfer, and K. Tran (2020). Airbnb and rents: Evidence from berlin.

Franco, S. F., C. Santos, and R. Longo (2019). The impact of airbnb on residential property values and rents: Evidence from portugal.

Garcia-López, M.-À., J. Jofre-Monseny, R. Martínez Mazza, and M. Segú (2019). Do short-term rental platforms affect housing markets? Evidence from airbnb in barcelona.

Gurran, N. and P. Phibbs (2017). When tourists move in: How should urban planners respond to airbnb? *Journal of the American planning association 83*(1), 80–92.

Guttentag, D. (2015). Airbnb: disruptive innovation and the rise of an informal tourism accommodation sector. *Current issues in Tourism 18*(12), 1192–1217.

Gyódi, K. (2017). Airbnb and the hotel industry in warsaw: An example of the sharing economy? *Central European Economic Journal 2*(49), 23–34.

Hatzopoulos, V. (2018). *The collaborative economy and EU law*. Bloomsbury Publishing.

Horn, K. and M. Merante (2017). Is home sharing driving up rents? Evidence from airbnb in boston. *Journal of Housing Economics 38*, 14–24.

Ioannides, D., M. Röslmaier, and E. van der Zee (2018). Airbnb as an instigator of 'tourism bubble' expansion in Utrecht's lombok neighbourhood. *Tourism Geographies*, 1–19.

Jefferson-Jones, J. (2015). Can short-term rental arrangements increase home values?: A case for airbnb and other home sharing arrangements. *A Case for Airbnb and Other Home Sharing Arrangements (June 1, 2015). The Cornell Real Estate REview 13*.

Kajuth, F. (2017). Assessing recent house price developments in germany: An overview. In *Monetary Policy, Financial Crises, and the Macroeconomy*, pp. 225–235. Springer.

Kholodilin, K. A. and C. Michelsen (2017). No germany-wide housing bubble but overvaluation in regional markets and segments. *DIW Economic Bulletin 7*(25/26), 255–264.

Kim, J.-H., T. C. Leung, and L. Wagman (2017). Can restricting property use be value enhancing? Evidence from short-term rental regulation. *The Journal of Law and Economics 60*(2), 309–334.

Koster, H., J. van Ommeren, and N. Volkhausen (2018). Short-term rentals and the housing market: Quasi-experimental evidence from airbnb in Los Angeles.

Lee, D. (2016). How airbnb short-term rentals exacerbate Los Angeles's affordable housing crisis: Analysis and policy recommendations. *Harv. L. & Pol'y Rev. 10*, 229.

Miller, S. R. (2014). Transferable sharing rights: A theoretical model for regulating airbnb and the short-term rental market. *Available at SSRN 2514178*.

Nieuwland, S. and R. van Melik (2018). Regulating airbnb: How cities deal with perceived negative externalities of short-term rentals. *Current Issues in Tourism*, 1–15.

Schultz, E. L., D. T. Tan, and K. D. Walsh (2010). Endogeneity and the corporate governance-performance relation. *Australian journal of Management 35*(2), 145–163.

Segú, M. (2018). Do short-term rent platforms affect rents? Evidence from airbnb in barcelona.

Ullah, S., P. Akhtar, and G. Zaefarian (2018). Dealing with endogeneity bias: The generalized method of moments (gmm) for panel data. *Industrial Marketing Management 71*, 69–78.

Wintoki, M. B., J. S. Linck, and J. M. Netter (2012). Endogeneity and the dynamics of internal corporate governance. *Journal of financial economics 105*(3), 581–606.