Essays on China's Real Estate Market

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Abstract

This thesis examines the factors that affect the real estate market in China from the perspectives of sentiment, place-based policies, and tariff shocks. The results are presented in three stand-alone empirical chapters.

Chapter 2 probes the influence of sentiment on house prices within China. We construct a novel social media sentiment index, which quantified the tone of Weibo posts relating to "housing market" from 2010 to 2020 across China's 35 largest cities. This index can predict house price changes up to six quarters ahead, even after factoring in economic fundamentals. These findings, robust to numerous checks, are not driven by announced policy modifications, unobserved fundamentals, or censorship bias and therefore reinforce theories of social learning and, to a minor degree, of animal spirits.

Chapter 3 investigates the impact of the merger of suburbs into urban districts on property prices, using Beijing as an example and utilizing a difference-in-differences approach, within an event study framework. The results show that such mergers lead to a substantial surge in house prices in the rezoned areas. In contrast, the non-rezoned border districts experience a decline, with localized impacts in both scenarios. The merger negatively affects the economically disadvantaged, evident by the pronounced decline in house prices for low-priced properties in non-rezoned border districts and a smaller increase in rezoned ones. Further analysis reveals that the merger has a positive spillover effect in surrounding counties, with the effect decreasing as the distance to the rezoned districts increased.

Chapter 4 analyzes the impacts of the US-China tariff war on commercial building rents across Chinese cities using Bartik-style tariff exposure proxies. This analysis finds a one percentage point increase in the US tariff exposure resulted in a 1.03 percent decrease in commercial building rent growth after one quarter, ceteris paribus. In contrast, China's retaliatory tariff has no significant impact on the growth of commercial building rents. Additionally, the analysis reveals differences in rent responses, with areas of elevated US dependence showing intensified detrimental effects, while superior financial conditions, societal stability, innovation, and geographical placement showing mitigated effects. Furthermore, the chapter reports that tariff exposures from the US and China exerted their influences through different channels, subtly affecting rent growth.

The insights derived from this thesis are pivotal for policy formulation in developing nations. Firstly, sentiment, prominently reflected through social media, exerts a tangible and foreseeable impact on real estate valuations. This indicates that policymakers should monitor public sentiment as a precursor for probable escalations or depreciation in property markets. Secondly,

urban planning and rezoning decisions can induce significant impacts on housing values in local and neighboring markets. Thus, it is imperative for policymakers to judiciously evaluate the implications of such initiatives, particularly their repercussions on less affluent demographics. Lastly, external economic disruptions, like the US-China tariff war, can profoundly influence commercial real estate rents, especially those cities intertwined with international trade. The adverse effects are palpable in both China and the US, indicating a need for policymakers to fortify the robustness of property markets against external perturbations by diversifying economic partnerships and instituting provisional strategies.

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CHAPTER 1 Introduction

1.1 Motivation

China's huge property industry has long been an important engine of economic growth. "More than two-thirds of urban households' wealth is tied up in property and the industry underpins a fifth of GDP."[1] Distress in the housing sector of China began to manifest in 2021, with the default of the second largest real estate developer in China, Evergrande, whose land reserves alone were large enough to house 10 million people in 2020. Since then the concern about market collapse has deepened. Despite the People's Bank of China (PBoC) enacting interventions to assure adequate credit for property developers to complete unfinished projects and to present potential buyers with favorable mortgage rates, such heightened intercessions underscore the mounting concerns pervading the housing market.

It is crucial to understand the factors that affect China's housing market to prevent its potential downturn or collapse. Over two decades, researchers have tried to understand what causes the remarkable housing boom in China. Fundamental factors, such as interest rates, employment rates, and income, have been applied to explain the boom. However, these factors only account for a small fraction of house price changes (Himmelberg et al., 2005; Lai & Van Order, 2010; Fang et al., 2016).

What other factors affect the housing market in China? This thesis answers the question from three perspectives: sentiment, policy shock, and an unexpected international trade shock: the US-China trade war. This thesis aims to answer the following three questions: First, it explores the role of social media sentiment in understanding house price changes. Second, it investigates the impact of place-based policies during urbanization on local and neighboring housing markets. Third, it examines the impact of the US-China tariff war on the commercial real estate rental market. This thesis analyses the research questions in three papers. The

[1] The Economist "China's property crisis has not gone away: it is getting worse"

empirical analyses of these questions help understand the housing market so that policymakers can establish appropriate policy settings to ensure the stability of society facing expected or unexpected shocks.

1.2 Social media sentiment and house prices: Evidence from 35 Chinese cities

Recent studies by Soo (2018), Walker (2014), Zheng et al. (2016),Zhu et al. (2022), Ben-David et al. (2019), Himmelberg et al. (2005) and others have highlighted the significance of sentiment in explaining variations in house prices, particularly where traditional economic factors might not fully elucidate the observed price behavior. The correlation between real estate market growth and economic expansion is particularly pronounced in China (Fang et al., 2016). However, since 2010, speculative behaviors like "down payment loans," "bridge loans," "Yin-Yang contracts," and "strategic divorces" have been progressively identified as areas of concern by policymakers (e.g., Agarwal et al., 2019; Alm et al., 2021). In Chapter 2, our research delves into the influence of sentiment on the Chinese housing boom, utilizing a novel sentiment index derived from a textual analysis of Weibo posts, one of the world's largest social media platforms.

Constructing a sentiment index from the vast corpus of Weibo posts, we observe its predictive capacity for house price changes across major Chinese cities between 2010 and 2020, up to six quarters in advance, even after controlling for fundamental factors. Notably, a 1 percent increase in the accumulated lagged sentiment index over six quarters correlates with a 0.81 percent appreciation in subsequent quarterly house price changes, ceteris paribus. This result is robust to a large battery of robustness checks.

We eliminated a number of plausible candidates that could spuriously drive our baseline results, leading us to conclude that the social media index reflects the effects of social learning. First, the sentiment index may contain information on unmeasured fundamentals in addition to the fundamental variables we already control for. We construct a series of social media "fundamental indexes" using our sentiment-index methodology and find that our results remain robust to these unobserved fundamentals. Second, we assess whether sentiment spuriously measures people's expectations of policy interventions. We evaluate this interpretation by studying the role of China's largest housing market policy intervention, a wave of home purchase restriction (HPR) policies. Using a difference-in-differences framework, we find that de-regulation HPRs do not change the predictive power of our sentiment index, neither in its significance nor in its magnitude. We interpret this as evidence against the notion that policy changes spuriously drive our benchmark results. We also provide evidence that our results are robust to censorship biases.

The literature (e.g., Chang et al., 2011; Piazzesi & Schneider, 2009; Soo, 2018; Gao et al., 2020; Lyócsa et al., 2020; Loughran & McDonald, 2011; Alm et al., 2021; Baker et al., 2021) identifies various cross-sectional drivers of price changes. For example, when investors have greater access to information media and the speculative demand is particularly high. We test these hypotheses by using the proportion of internet users in a given city population as a proxy for information accessibility and by using a round of home purchase restriction policies as a demand shock to speculators, respectively. We find that the sentiment effects on house prices are indeed stronger in cities with more widespread information access. However, we do not find support for the second channel.

Discarding these alternative explanations, our conclusion posits that social media sentiment chiefly mirrors the "animal spirits" driving housing market participants. This interpretation aligns with a broad array of theoretical frameworks, ranging from Keynes (1936) to Acharya et al. (2021). Additionally, our findings offer tentative support for social learning theories, wherein individuals' beliefs evolve through interactions (Bailey et al., 2018a; Bayer et al., 2021; Burnside et al., 2016).

1.3 Rezoning and house prices: Evidence from Beijing, China

Savills reports the global real estate value reached $326.5 trillion USD in 2020, about four times the world GDP ($85.11 trillion USD). Elevated house prices correlate with an amplified nontradable sector and total output. However, a pronounced housing surge can precipitate an economic bubble, negatively affecting long-term economic health by curtailing productive lending, diminishing investment and productivity, and promoting capital misallocation (Hau et al., 2018; Adelino et al., 2018). Notably, while substantial research has centered on developed nations (Busso et al., 2013; Chen et al., 2022, e.g.), developing countries remain underexplored. In Chapter 3, our analysis delves into how rezoning affects house prices in local and neighboring markets, using the 2015 merger of Miyun and Yanqing counties into Beijing districts as an example.

We employ a difference-in-differences identification with an event-study setting allowing us to examine the causal impacts of mergers with geographic variation (Lu et al., 2019; Zheng et al., 2017; Chen et al., 2022). We focus on testing two hypotheses in this chapter: First, the merger may lead to a decrease in house prices in old neighboring districts. The increased investor confidence in the economic prospects of the rezoned district will trigger a shift in purchases from the old neighboring districts to the new districts. As a result, negative spillover effects will be observed in the old neighboring areas, leading to a decline in house prices. Second, the merger may cause an increase or decrease in house prices in the new districts. Initially, there will be a surge in housing demand due to the shift in purchasing from the old neighboring districts. However, the merger can also lead to a decrease in housing demand in the new districts. Home purchase restrictions will prohibit out-of-town homebuyers from purchasing in the new districts after integrating into Beijing. The short-term inelasticity of housing supply further warrants investigation into the magnitude of the decrease and increase in demand within the new districts.

This study documents the economically significant reduction in house prices in old neighboring districts and a significant appreciation in house prices in newly merged districts. The study also conducts a comprehensive examination of potential heterogeneity in the impact of the merger and finds that the merger has a stronger impact on residential complexes located close to the border of new districts, in line with Saiz (2010). Additionally, the study finds that the merger has a stronger impact on relatively inexpensive houses in old neighboring districts and more expensive houses in new districts. Putting everything together, the heterogeneous impact of rezoning on high- and low-priced properties is in line with agglomeration economies and spatial mismatch hypotheses in urban economics that entail market failures and often predict overlap between poor economic performance and disadvantaged residents (Neumark & Simpson, 2015; Diamond & McQuade, 2019).

Overall, this study contributes to the growing literature studying the impact of mergers on the housing market by quantifying the potential impact at the residential complex level and highlighting the local and external impact of the merger. This information is crucial for policymakers as it may lead to a more accurate estimation of the welfare loss and gain associated with such mergers.

1.4 Snowball effect of the US-China trade war: Evidence from China's commercial building markets

The US-China trade war, which was initiated by the United States in 2018, has been a focal point of recent global policy shifts. Researchers have examined the effects of tariff shocks on different aspects of the economy, such as GDP, employment, productivity, and investment(e.g., Waugh, 2019; Flaaen & Pierce, 2019). However, the snowball effect of the trade war on commercial real estate in China, an essential component of the economy, remains relatively unexplored.

The commercial real estate market plays a pivotal role in facilitating economic activity, attracting both domestic and foreign businesses, and contributing to employment and revenue generation. Understanding the impact of trade policy changes on the commercial building market is crucial for assessing the broader implications of the trade war on China's economy. Notably, the rental market often provides a more immediate gauge of economic disturbances, such as trade war, compared to the property sales market. This chapter, therefore, aims to bridge this gap by investigating the indirect consequences of trade policy changes on the commercial rental market in China.

Chapter 4 examines the impact of the tariff shocks that unfolded over the period Q1/2018-Q3/2019 on the year-on-year growth in commercial building rents over Q2/2018-Q4/2019. We construct two Bartik (or shift-share) style variables for each city within China. To measure the US tariff exposure, we combine the initial composition of a city's exports with product-level US tariff changes, following Chor & Li (2021). Similarly, we construct China's tariff exposure by utilizing the initial imports and China's retaliatory tariff changes on the US inputs. Our baseline results demonstrate a significant negative relationship between US tariffs and commercial building rent growth: a one percentage point increase in the US tariff exposure is associated with a 1.03 percent decrease in commercial building rent growth after one quarter, ceteris paribus. In contrast, China's retaliatory tariff has a positive but insignificant impact on the growth of commercial building rents. We also identify heterogeneity in rent responses, cities of high US dependence amplifies the negative impact, whereas better financial situations, social stability, innovation, and location mitigate it.

This chapter contributes to the literature by assessing the impact of trade wars initiated by the Trump administration on China's commercial real estate rental market. Previous research has focused predominantly on impacts on the US(Fajgelbaum et al., 2020; Amiti et al., 2019;

Cavallo et al., 2021; Waugh, 2019; Flaaen & Pierce, 2019; Goswami, 2020; Chor & Li, 2021), little attention has been paid to impacts on China.

To conclude, our paper provides clear evidence that cities exposed to trade war tariffs experienced a negative commercial building rents shock which operated particularly through high US dependence, poor financial situations, social instability, low innovation and undesired location.

1.5 Structure of the Thesis

This thesis follows a publications style rather than a monograph style. It comprises five chapters, including an introduction, three independent empirical papers, and a conclusion. The empirical chapters each examine a particular real estate market issue. Therefore, each empirical chapter contains its own abstract.

It seems relevant to clarify a particular writing style followed in this thesis. The thesis uses 'we' in the relevant chapters as they are co-authored with my Ph.D. supervisors.

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STATEMENT OF CONTRIBUTION DOCTORATE WITH PUBLICATIONS/MANUSCRIPTS

We, the student and the student's main supervisor, certify that all co-authors have consented to their work being included in the thesis and they have accepted the student's contribution as indicated below in the Statement of Originality.

Student name:	Yiran Mao
Name and title of main supervisor:	Professor Martin Berka
In which chapter is the manuscript/published work?	Chapter 2
What percentage of the manuscript/published work was contributed by the student?	80%

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This form should appear at the end of each thesis chapter/section/appendix submitted as a manuscript/ publication or collected as an appendix at the end of the thesis.

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Doctoral Research Committee July 2022

CHAPTER 2 Social media sentiment and house prices: Evidence from 35 Chinese cities

Abstract

We develop a new social media sentiment index by quantifying the tone of social media posts on Weibo related to housing between 2010 and 2020 across the 35 largest cities in China. We find that the social media sentiment index significantly predicts house price changes for up to six quarters ahead after controlling for economic fundamentals. A one percent increase in an accumulated social media sentiment index results in a 0.81 percent increase in house price inflation the following quarter, ceteris paribus. Our results cannot be explained by announced policy changes, unobserved fundamentals, or censorship bias, and survive a battery of robustness checks. We show these results yield support to theories of social learning as in Burnside et al. (2016), Bailey et al. (2018a), Bayer et al. (2021) and, to a lesser extent, of animal spirits (Acharya et al., 2021).

2.1 Introduction

Internet users globally spend 147 minutes a day on social media, on average[1]. Although this widespread popularity of social media makes it amenable to measuring sentiment more precisely than traditional methods, few studies have used social media sentiment measures to date. Since proposed by Keynes (1936), sentiment and "animal spirits" more generally have been an integral part of economics. Sentiment that captures irrational expectations orthogonal to fundamentals has been shown capable of driving persistent aggregate fluctuations under rational expectations (e.g., Benhabib et al. (2015) and Acharya et al. (2021)). Recently, Soo (2018), Walker (2014), Zheng et al. (2016), Zhu et al. (2022) and others document how sentiment can help explain price changes in housing markets, where fundamental economic forces often fail to fully account for the observed price behavior (e.g., Ben-David et al., 2019; Himmelberg et al., 2005).

Nowhere has the real estate market development been more instrumental for economic growth than in China (Fang et al., 2016). Yet, especially since 2010, speculative behavior (such as "down payment loans", "bridge loans", "Yin-Yang contracts" or "strategic divorces") has been increasingly recognized as a concern by the policymakers (e.g., Agarwal et al., 2019; Alm et al., 2021). We study the role sentiment plays in the Chinese housing boom using a newly constructed sentiment index based on textual analysis of the universe of posts on Weibo, one of the largest social media networks in the world.

We construct a social media sentiment index using the universe of Weibo posts and find that it predicts changes in house prices of all major Chinese cities between 2010 and 2020 up to 6 quarters ahead, after controlling for the fundamental drivers of price changes. A one percent increase in six quarters of accumulated lagged sentiment index is associated with 0.81 percent appreciation in a quarterly future house price change, ceteris paribus. This result is robust to a large battery of robustness checks. For example, when we study the role of several waves of policy interventions by the Chinese government in housing markets in a difference-in-differences setting, we find that such policy interventions cannot explain the magnitude and significance of the social media sentiment index's predictive power.

While online censorship is widespread in China, we find that - while present in our results - it does not meaningfully change the key conclusions in a manner that supports the findings in Chen & Yang (2019). Our empirical results support several theories of social learning.

[1]Statista, Daily social media usage worldwide, as of Mar 21, 2022

In spite of their theoretical importance, it is the empirical assessment of the importance of the animal spirits or social learning channels that limits the progress of the literature (e.g., Soo, 2018). This assessment relies on a variety of methods to construct measures of sentiment. Participant surveys, while directly measuring the sentiment of market participants, face high administration costs and limited geographic and time coverage. Internet-search indices (e.g., Google trends) can measure general interest in a topic (Calomiris & Mamaysky, 2019; Swamy & Dharani, 2019) and benefit from broad coverage. However, they fail to capture the directionality and the extent of interest. For these reasons, research increasingly employs textual and sentiment analysis, often from newspapers and 10-K forms.[2] Because newspaper-based sentiment measures is tainted by the news writers' bias, it does not accurately measure the opinion of the society at large. Social media, on the other hand, presents an unfiltered source of public information on the sentiment of society regarding a particular topic (Luca, 2015). Although some research has been carried out social media data, city-level data has not yet been utilized to draw major insights, especially in China.[3]

We construct social media sentiment indices for 35 major cities in China by collecting over 800,000 posts on Weibo (a Chinese counterpart to Twitter with over 500 million monthly active users compared to Twitter's 330 million) between January 1, 2010 (Weibo's inception) and July 31, 2020.[4] Using an approach similar to Soo (2018), we construct the sentiment index by quantifying the tone of each post with a standard Chinese financial sentiment dictionary. Finally, we aggregate the measures to quarterly frequency at the city level to match the dimensionality of our other variables.

Our index is among the first studies discussing the role played by social media sentiment in Chinese cities and offers several methodological advances on Soo (2018)'s work.[5] First, we use social media posts rather than news articles. This has the advantage of canvassing stated opinions of a wide cross-section of society rather than a self-selected and narrow group

[2]For example, Tetlock (2007); Loughran & McDonald (2011); Heston & Sinha (2017); Ke et al. (2019) utilize text data constructed sentiment indexes from 10-K forms and newspaper stories to predict stock returns.

[3]McGurk et al. (2020) and Sun et al. (2016) find that investor sentiment extracted from social media posts has predictive power on stock returns. Zheng et al. (2016) show that confidence index based on search engine results can predict housing sales and price changes across Chinese cities. Recently, Zhu et al. (2022) conclude that social media sentiment can predict future movements of listed developers' stock prices and house prices in China. However, their paper focuses on constructing sentiment indexes, while our work's goal is the identification of the channels behind the observed predictive power of the sentiment index in house price changes.

[4]Source: Statista: Twitter: number of monthly active users and Number of monthly active users of Sina Weibo

[5]Zhu et al. (2022) and Li et al. (2022) show that sentiment index is correlated with house prices in China. However, their work is significantly more limited in scope.

of newspaper writers. Everyone with a mobile phone can express their sentiment on social media platforms in real-time for free.[6] Social media posts are, therefore, a significantly more accurate and comprehensive medium than newspapers when it comes to measuring sentiment. Second, in contrast to Soo (2018), who only considers a word list consisting of "increase", "rise", "decrease", and "fall", we use all sentiment words included in the standard Chinese Financial Sentiment Dictionary (CFSD) when constructing our social media sentiment index. By focusing on a financial vocabulary, we are able to measure specifically the financial sentiment as it relates to house prices.[7] We later show that our results are robust to the choice of dictionary.

We eliminate a number of plausible candidates that could spuriously drive our baseline results, leading us to conclude that our findings can only be interpreted as reflecting the effects of social learning. First, the sentiment index may contain information on unmeasured fundamentals in addition to the fundamental variables we already control for. We construct a social media "fundamentals index" using our sentiment-index methodology and find that our results remain robust to these unobserved fundamentals. Second, we assess whether sentiment spuriously measures people's expectations of policy interventions.

We evaluate this interpretation by studying the role of China's largest housing market policy intervention, a wave of home purchase restriction (HPR) policies. HPRs were first implemented in 2010 in Tier-1 (China's largest) cities and were consequently removed in those cities when house prices stabilized in 2014. With the rebound of house prices in 2016, many cities initiated HPRs to curb the surging prices. Using a difference-in-differences framework, we find that de-regulation HPRs did not change the predictive effect of our sentiment index, neither in its significance nor in its magnitude. We interpret this as evidence against the notion that policy changes spuriously drive our benchmark results.

The literature (e.g., Chang et al., 2011; Piazzesi & Schneider, 2009; Soo, 2018) identifies various cross-sectional drivers of price changes, for example, when investors have greater access to information media, and where the speculative demand is particularly high. We test these hypotheses using the proportion of internet users in a given city population as a proxy for information accessibility and using a round of home purchase restriction policies as a demand shock to speculators, respectively. We find that the sentiment effects on house prices are indeed

[6] There are 1.65bn mobile phone subscriptions in China, more than its current population.

[7] Loughran & McDonald (2011) find that the quality of words is fundamental to correctly measuring the tone in the financial text.

stronger in cities with more widespread information access. However, we do not find support for the second channel.[8]

We conclude with a battery of additional robustness checks. We relax the assumption of symmetry on the positive and negative social media sentiment and find that negative sentiment has a significantly higher impact on house prices. We then calculate gender-specific sentiment indexes and find that males' sentiment has a higher house price elasticity than females'. We offer an explanation driven by societal expectations in China that require males to buy an apartment to succeed in a highly competitive marriage market (e.g., Wrenn et al., 2019; Zheng et al., 2016). This asymmetry can be explained by the need of males to stay informed, supporting Baker et al. (2016). We try to construct an alternative sentiment index that weighs posts depending on various measures of "importance" and finds no change in the significance and magnitude of our main result.[9]

With the exclusion of these interpretations, we conclude that social media sentiment likely reflects the "animal spirits" of participants in the housing market. This interpretation is born from a wide spectrum of theoretical models, from Keynes (1936) to Acharya et al. (2021). Our results may also offer some weaker support to social learning theories where people shape their beliefs through interacting with others (Bailey et al., 2018a; Bayer et al., 2021; Burnside et al., 2016).[10]

The rest of this paper is organized as follows: Section 2.2 reviews previous literature in relation to the housing market, focusing on the role of sentiment, and then gives a brief background on China's housing market. Section 2.3 details and describes the data and presents a survey validation. Section 2.4 discusses the empirical strategy of the paper and presents the baseline results. Section 2.5 tests the three competing interpretations of sentiment effects on the housing market. Section 2.5.3 measures the cross-sectional effect of sentiment. Section 2.7 discusses further robustness checks and is followed by conclusions in Section 2.8.

2.2 Literature and Background

[8] It would be the best to identify supply and demand causes with ownership data. However, we are unable to collect either(i) the ownership of each social media posters; or (ii) city-level ownership or vacancy rate data (No official data available).

[9] We use the number of reposts, the number of comments on each post, or the number of followers each account has to construct different weighting schemes.

[10] However, this channel appears less prominent as it would imply that the most impactful social posts should play a more significant role in predicting prices. We find no evidence to support this prediction.

2.2.1 Related Literature

Keynes (1936) describes "animal spirits" as a "spontaneous urge to action" or "spontaneous optimism". The first systematic study of animal spirits by Akerlof & Shiller (2010) transformed animal spirits into a general psychological force that influences the entire economy. Since then, animal spirits have been included in economic models as various types of expectations. In the housing market, researchers find that the irrationality of investors holding excessively pessimistic or optimistic sentiments about the housing market, as well as their herd behavior, can explain changes in house prices (e.g., Case et al., 2005; Garriga et al., 2019; Guren, 2018; Malpezzi & Wachter, 2005; Piazzesi & Schneider, 2009; Shiller, 2015; Towbin & Weber, 2015).

Recent research studies the role of bounded rationality in housing markets, where individuals receive information from their daily interactions and search and learn from or imitate it (Glaeser & Nathanson, 2015). Froot et al. (1992) and Bikhchandani et al. (1998) discuss rational reasons behind early imitation in the setting of manias. DeCoster & Strange (2012) show how developers' imitating their competitors' behavior can be bounded rationality. Burnside et al. (2016) show how different expectations about long-run fundamentals, which are then adjusted by random social learning, decades-long house price booms and busts can emerge from gradual ebbs and flows of optimism and pessimism between rational atomistic market participants. Zheng et al. (2016) extend this model to accommodate online search.

Other authors, including Wheaton (1990) and Krainer (2001), present other search and matching models of housing markets where social interactions help explain house price dynamics.

The measurement of sentiment in the literature has evolved. Sentiment surveys of the housing market use declared beliefs, categorized as positive or negative sentiment based on house-purchase attitudes. Results using survey-based sentiment measures, though limited in scope, have shown that sentiment can explain some house price variation.[11] On the other hand, sentiment has also been inferred from stock market data, where common variation in some

[11] E.g., Piazzesi & Schneider (2009) use Michigan Survey of Consumers which contains two questions pertinent to beliefs around housing: "Generally speaking, do you think now is a good time or a bad time to buy a house?" and a follow-up "Why do you say so?". Similarly, Bork et al. (2020) construct a sentiment index as a linear combination of ten sentiment series extracted from the same survey. Case et al. (2012) use their own survey and construct a sentiment index for four US metropolitan housing markets from 2003 to 2014. They both find that survey sentiment explains a large share of house price changes during boom and bust cycles. In Hong Kong, Wong & Hui (2006) developed first a cross-sectional survey (November 2000) and then a quarterly longitudinal survey (December 2003 to June 2004) on the housing sentiment of homeowners and potential homebuyers. Their results again indicate that housing sentiment explains a large share of the time variation in house prices and that even the presence of a small number of optimistic investors can lead to large swings in house prices.

variables is attributed to market sentiment.[12] This implied investor sentiment has significant additional predictive power for stock returns. Internet search data has been used to construct indexes of attention since at least Lee et al. (2015).[13] With regards to the housing market, Zhang & Tang (2016) use Baidu search data to document the relationship between house prices and public attention to housing markets in 19 Chinese cities. However, in spite of its convenience and availability, internet search data is limited by its inability to measure either the direction or the magnitude of the observed attention.

Textual analysis and sentiment estimated from 10-K corporate filings and newspaper-based data have been used extensively in finance since at least Tetlock (2007)'s analysis of a Wall Street Journal opinion column. Early on, this literature tackled the difficulty of determining which words carry sentiment and how, thus enabling the attribution of direction and size (recently, e.g., in Heston & Sinha, 2017; Ke et al., 2019). Loughran & McDonald (2011) create a comprehensive list of positive and negative words to quantify the tone of financial text, and find that the negative word list captures the tone of 10-K reports better.[14] Glasserman & Mamaysky (2019) develop a method to measure the "unusualness" of the news based on more than 360,000 articles on 50 large financial companies over two decades. Defining "unusual" by using less-frequently used phrases compared to the prior month, they find that unusually negative news predicts an increase in stock price volatility. Their work is extended by Calomiris & Mamaysky (2019), who use the context and content of the news articles to show that sentiment can forecast one-year-ahead returns and stock drawdowns. Soo (2018) is the first to study the role of newspaper-based sentiment on house prices across 34 US cities. She measures sentiment by quantifying the tone of local housing market news and finds that it explains not only the current house prices but also predicts future price changes.

[12] Based on Baker & Wurgler (2006)'s work, Stambaugh et al. (2012); Huang et al. (2015) construct sentiment index using common variation in i.e., closed-end fund discount, share turnover, number and average first-day returns during IPOs, the equity share in new issues, and the dividend premium. Each such proxy is first orthogonalized to a set of economic variables, and the first principal component is attributed to the sentiment

[13] The literature focuses on the measurable impact of internet search intensity on stock market data (e.g., Gao et al. (2020)). Da et al. (2015) develop the Financial and Economic Attitude Revealed by Search (FEARS) index, which aggregates the volume of internet search queries (such as searches for "recession", "bankruptcy", and "unemployment") from the US In daily data between 2004 and 2011, FEARS can predict short-term equity return reversal. Amstad et al. (2020) construct a new COVID-19 risk attitude index for 61 markets using internet search data from Google and Baidu and show their index can capture investors' attitudes toward pandemic-related risks.

[14] This is because positive words are frequently negated: it is common to see the framing of negative news using positive words (e.g., "did not benefit"), while corporate communications rarely convey positive news using negated negative words ("not downgraded").

Research also discusses the limitations of the aforementioned methods of sentiment quantification. Surveys are costly, hard to replicate, and limited in geographic and time scope (Algaba et al., 2020). An internet search measures the total number of entries, making it too noisy to capture sentiment accurately (Jun et al., 2018) and unable to attribute tone correctly.[15] Newspaper-based sentiment measures are limited by the columnist's bias and imperfect market information (e.g., Dougal et al., 2012; Mullainathan & Shleifer, 2005; Reuter & Zitzewitz, 2006).

In spite of the increased awareness of the importance of social media in social interaction (e.g., Bailey et al., 2018a; Li et al., 2017a), only limited research on sentiment has used social media posts. Social media's decentralized nature and consequent popularity make it a popular place for social learning. In an important novel contribution, Bailey et al. (2018a) combines social media location information from Facebook with housing transaction details, they find that people whose geographically distant friends bought a house whose value consequently increased are more likely to transition from renting to owning, buy larger houses, and pay more for a given house.

Recent work additionally studies the role of higher moments in the distribution of beliefs and expectations in households' housing and mortgage choices. Kuchler & Zafar (2019) use the New York Fed's Survey of Consumer Expectations questions on expected future house price changes to find that more volatile local house price changes are associated with a wider distribution of expected national house price changes. Bailey et al. (2019) find that the mean and standard deviation of the house prices observed by one's friends shift the corresponding moments of the distribution of one's house price beliefs. Individuals with friends from counties with a wider variety of house price experiences report a wider distribution of expected house price changes, ceteris paribus.

Finally, some recent literature links the sentiment to expectation-formation. For example, in a static model, Benhabib et al. (2015) show how sentiments generate stochastic self-fulfilling rational expectations equilibria. Acharya et al. (2021) extend this work to a dynamic environment and show how self-fulfilling sentiments orthogonal to fundamentals can drive persistent aggregate fluctuations under rational expectations.

[15] An article might be counted twice as it contains both positive and negative words, and the effect will be offset.

2.2.2 Institutional Background

Privatization reformed China's housing market in the early 1990s by allowing commoditization of housing. Since then, and especially in the last decade, cities in China witnessed significant housing growth, partly driven by the growing popularity of investment in housing by everyday Chinese. For example, between 2003 and 2013, the average annual real growth rate of house prices in Tier-1 cities was 13.1%, while it was 10.5% and 7.9% in Tier-2 and Tier-3 cities, respectively (Fang et al., 2016).[16]

In response to the perceptions of overheating, the Chinese government implemented a series of macro-control policies to stabilize house prices. Two rounds of home purchase restriction policy (HPR) to curb speculative demand and price rises were implemented in Tier-1, Tier-2, and some Tier-3 cities in a staggered sequence. These policies subjected buyers to stricter requirements for accessing mortgages, in conjunction with imposed higher interest rates and larger down payments. Finally, limits were imposed on the number of properties an individual can purchase (Zheng et al., 2021). The HPR policies were first implemented in Beijing on 17 April 2010 and then in 46 other big cities by July 2010. Following that, some second and third-tier cities also initiated HPRs to curb the increasing demand for housing. As house prices stabilized, HPRs were removed everywhere except Beijing, Shanghai, Guangzhou, Shenzhen, and Sanya by the end of 2014. However, as house prices rebounded in 2015, a second round of HPRs was implemented in 2016. Li et al. (2017b), Chen et al. (2018) find that HPRs dampen speculative investment demand in the short term. However, Somerville et al. (2020) find this effect to diminish over time. Recent research also discusses the spatial heterogeneity of HPRs' effects on house prices (e.g., Cao et al., 2015; Jia et al., 2018; Li et al., 2020c; Sun et al., 2017; Wu & Li, 2018; Zhang & Wang, 2016). For example, Sun et al. (2017) find that house prices are more sensitive to HPR where the housing supply is inelastic.

[16]We follow Fang et al. (2016)'s classification of Chinese cities. The first tier includes four cities with the largest population and economic importance in China: Beijing, Shanghai, Guangzhou, and Shenzhen. The second tier is comprised of Tianjin and Chongqing (the two autonomous municipalities other than Beijing and Shanghai), the capital cities of 24 provinces, and nine additional industrial or commercially important centers. The statistical office has an official classification for 70 big and medium cities: Tier-1: Beijing, Shanghai, Guangzhou, and Shenzhen. Tier 2: 31 cities including Tianjin, Shijiazhuang, Taiyuan, Hohhot, Shenyang, Dalian, Changchun, Harbin, Nanjing, Hangzhou, Ningbo, Hefei, Fuzhou, Xiamen, Nanchang, Jinan, Qingdao, Zhengzhou, Wuhan, Changsha, Nanning, Haikou, Chongqing, Chengdu, Guiyang, Kunming, Xi'an, Lanzhou, Xining, Yinchuan, Urumqi. Tier-3: 35 cities including Tangshan, Qinhuangdao, Baotou, Dandong, Jinzhou, Jilin, Mudanjiang, Wuxi, Xuzhou, Yangzhou, Wenzhou, Jinhua, Bengbu, Anqing, Quanzhou, Jiujiang, Ganzhou, Yantai, Jining, Luoyang, Pingdingshan, Yichang, Xiangyang, Yueyang, Changde, Shaoguan, Zhanjiang, Huizhou, Guilin, Beihai, Sanya, Luzhou, Nanchong, Zunyi, Dali.

2.3 Data Description

2.3.1 Collecting Social Media Data

Our sentiment data is collected from the posts on Sina Weibo, one of China's biggest social media platforms. With 573 million active monthly users, which comprise around 40% of China's population, Weibo is an excellent place to search for publicly available expressions by citizens of China.[17] Its gender- and location-diverse user base provides a representative sample of the Chinese population.

We collect Weibo posts discussing house prices in the 35 largest Chinese cities between January 1, 2010, and July 31, 2020, using a Python program on its web and mobile terminals.[18,19] Our search progresses as follows. First, we search for posts containing a specific city name and "house price" ("fang jia" in Chinese). Second, we restrict the search for posts between January 1, 2010, and July 31, 2020, because Weibo was launched in the latter half of 2009, and the latest availability of quarterly macroeconomic data at the time of our analysis was Q2, 2020. For each post, we collect: its content, date, author's username, total number of likes, total number of forwards, and the total number of comments. We also extract all self-disclosed user information for every username, including gender, profile verification status, the type of verification, date of birth, university education level, location, credit score, and the total number of followers (see Appendix A2.A for more details). Our search yields a total of 845,930 posts, which cover 35 Chinese Tier-1 cities.[20] Table 2.1 reports the number of posts for each city in our sample. Understandably, large cities have the highest number of posts discussing house prices. The number of posts for Beijing, Shanghai, and Shenzhen is more than 100 times larger than the number of posts for the smallest city - Xining. This is both because people focus on house prices more

[17] See Weibo Financial Statements, the third quarter of 2021, for the number of active monthly users.

[18] The 35 cities are: Beijing, Changchun, Changsha, Chengdu, Chongqing, Dalian, Fuzhou, Guangzhou, Guiyang, Harbin, Haikou, Hangzhou, Hefei, Hohhot, Jinan, Kunming, Lanzhou, Nanchang, Nanjing, Nanning, Ningbo, Qingdao, Shanghai, Shenyang, Shenzhen, Shijianzhuang, Taiyuan, Tianjin, Urumqi, Wuhan, Xiamen, Xi'an, Xining, Yinchuan and Zhengzhou. Please see Table 2.8 for the list of cities and the number of posts for each city.)

[19] Weibo was launched in the second half of 2009. Python is commonly used to search social media data(e.g., Almatrafi et al., 2015; Jain, 2013). The first is to access its open platform API. The second is to search its web portal. The API is an official development tool of Weibo. While it is efficient and convenient, it is also restricted by Weibo to a maximum of 150 searches per day. The second approach combines APIs with web crawlers to obtain more data and is, therefore, a more popular method (Yuan et al., 2019). We use the latter of the two approaches.

[20] We collect all posts of Weibo reports for our search queries. While these constitute an unknown fraction of all the posts that exist for our search terms, we believe our search output is a representative sample of all posts with the aforementioned parameters on Weibo, as our procedure is the common method of collecting the universe of posts on Weibo (e.g., Bai et al., 2021; Li et al., 2020b,a).

intently in the large cities with booming housing markets and because those cities have more inhabitants.

[Table 2.8]

2.3.2 Preprocessing Social Media Data

Social media posts are rich in content and unstructured in format. They contain emoticons, pictures, web links, and symbols, requiring more preprocessing than newspaper articles. We follow Sarkar (2019) in performing necessary preprocessing.[21] We complete this preprocessing using a Tsinghua University Lexical Analyzer for Chinese (THULAC) program, a standard Chinese word-segmentation tool trained on a newspaper corpus containing more than 58 million words. Its segmentation accuracy is higher than 97%.[22]

[Figure 2.1]

Figure 2.1 plots the most frequent 300 positive and negative words (in Chinese) appearing in posts. Table 2.2 lists the most common 20, translated to English. The most frequently positive and negative words are "develop" and "fall". Four of the positive words are related to "increase" (including "increase", "skyrocket", "grow", "rise"), with a combined frequency of 8%. Eight of the top negative words are related to "decrease" (including "fall", "decline", "plummet", "slipping", "slumped", "restrain", "downward", "dropping"), with the total frequency of 24.11%. The total occurrence of positive words (697,936) is higher than that of negative words (512,434).

[Table 2.2]

[21] First, we delete Web links, GIFs, emoticons, videos, and pictures. Second, we remove "@ username" and "# topic #". "@ username" is used to call for other's attention while "# topic #" is used to clarify which special topic the post is related to. Because all our posts are under the same topic, "# housing market #", we remove it. Third, we convert traditional Chinese into simplified Chinese because traditional Chinese has little impact on sentiment and will greatly increase the difficulty of textual analysis. Fourth, we tokenize the posts. We split each post into sentences and then into tokens. Tokens are independent and minimal textual components with some definite syntax and semantics (Sarkar, 2019). In this process, sentences are split into their constituent words. While in English, spaces are used as delimiters between words, Chinese posts consist of characters without any obvious distinguishing mark between words, making word tokenization crucial to Chinese textual analysis. We then remove the stop words (words that end up occurring the most when aggregating a corpus of text based on singular tokens and checking their frequencies but have little or no significance. For example, words like "a", "the", and " me" are stop words, which substantially increase computing time and add noise to the results.).

[22] The THULAC Python package can be downloaded here

2.3.3 Measuring Housing Sentiment

We follow standard practice to quantify the sentiment of each post. Generally, sentiment words are classified as positive or negative using a sentiment dictionary. Second, the tone is measured by the ratio of negative or positive words to the total number of words in the document. The quality of the analysis then clearly depends on the quality of the sentiment dictionary.[23] While there are several sentiment dictionaries in the Chinese language (HowNeT, DLUTSD, and NTUSD), these are intended for general use. With no readily available dictionary built for sentiment in a housing market, we adopt a newly-developed Chinese Financial Sentiment Dictionary (CFSD) from Bian et al. (2021).[24] This dictionary is designed to measure sentiment in the financial market. It is built on several general-purpose dictionaries, including HowNet, DLUTSD, and NTUSD. CFSD amalgamated these dictionaries and added terms extracted from earnings conference call transcripts, IPO prospectus reports, and annual reports (Bian et al., 2021). We check all words in the dictionary and, in rare instances, correct the sentiment polarity. In total, there are 1119 positive words and 1474 negative words in the dictionary. Finally, following Su et al. (2014), 33 negative adverbs are added to our dictionary. When a negation word (e.g., not, no) is positioned before a sentiment word, the emotion is reversed. The calculation of our sentiment index is performed in Python following Soo (2018):

$$S\_pos_{jt} = \frac{\sum_{i=1}^{n} total\_pos_{ijt}/total\_words_{ijt}}{n} \tag{2.1}$$

$$S\_neg_{jt} = \frac{\sum_{i=1}^{n} total\_neg_{ijt}/total\_words_{ijt}}{n} \tag{2.2}$$

$$S_{jt} = S\_pos_{jt} - S\_neg_{jt} \tag{2.3}$$

where i, j, t represents post i discussing city j's housing market at time t. $total\_pos_{ijt}$ and $total\_neg_{ijt}$ are the total numbers of positive and negative words in post i for city j at time t,

[23] Many scholars use the Harvard-IV-4 psychosocial dictionary when analysing text data in English (e.g., Tetlock, 2007). On the contrary, others argue that the Harvard dictionary might not be proper for the financial market because many words that are classified as positive or negative might have different meanings in a financial context. Loughran & McDonald (2011) create a list of positive and negative words (hereafter referred to as the "LM list") based on the US 10-K forms from 1994 to 2008, and this LM list can capture the sentiment of 10-K reports better than the Harvard list.

[24] There are several alternative approaches. The first involves employing machine learning to construct a sentiment dictionary based on gathered social media posts. However, a potential issue arises with overfitting. While the dictionary may exhibit strong performance when tested against the corpus it was trained on, it may perform poorly when applied to a distinct corpus. The second approach involves using machine learning or a Large Language Model such as ChatGPT to directly generate sentiment. However, this approach is often criticized for hallucination. However, as broadly criticized, hallumination exist. Utilizing an external sentiment dictionary mitigates the risk of overfitting and facilitates cross-validation with diverse dictionaries.

while n is the number of non-neutral posts in a given city and time. S_{jt} is the average sentiment for the corresponding city and time.

Here is an example sentiment index calculation in a post (translated from Chinese into English): "Many real estate company bosses said that Beijing's house price is not going to increase this year.". First, after deleting the stop words after preprocessing, there are 9 words left: "real estate", "company", "bosses", "said", "Beijing", "house price", "not", "increase", "this year". The only sentiment word is "increase", which is a positive sentiment word according to the CFSD. However, the negation word "not" positioned before the sentiment word is detected, so the emotion of "increase" is reversed. The number of negative words is 1. The sentiment index for this post then is:

$$S = (0 - 1)/9 = -0.1111 \tag{2.4}$$

We finally calculate a city-specific sentiment index by averaging all posts referring to a specific city. Because our macroeconomic data are quarterly, we similarly aggregate our sentiment index to a quarterly frequency by averaging all sentiment indexes in a particular quarter. To understand the procedure, Figure 2.2 plots the histogram of sentiment for Beijing in the first quarter of 2010. There are over 2000 posts with sentiment indexes distributed between -0.3 to 0.3. The city-level sentiment index is the average of all posts, which is 0.0033 for this quarter.

[Figure 2.2]

2.3.4 Sentiment and Expectation

Recently, studies in behavioral finance have considered sentiment as a proxy for irrational expectations. Investigating the relationship between sentiment and expectation is, by nature, challenging , because beliefs are unobservable and hard to quantify. As discussed in the previous section, existing surveys in China are largely limited in geographic location and time scope and thus cannot be used at the city level. However, national-level validation is still available. China Urban Depositor Survey(CUDS) has been published by the People's Bank of China since 1999.[25] It surveys a nationally representative sample of 20,000 depositors from 50 cities (big, medium-sized, and small) each quarter on their attitudes towards economic performance, including those of the housing market. Specifically, the CUDS asks, "How do you think house

[25] The values for 2010q1 and 2012q3 are missing. We thus use the values generated by linear interpolation to replace those missing values

price is going to change next quarter?" Respondents answer "rise", "drop", "little change" and "unsure". The housing expectation index is calculated using the percentage of "rise".

Figure 2.3 plots the survey expectation with a national version of our sentiment index.[26] They share similar fluctuations. Both measures dropped rapidly from early 2012 to 2014, rebounded afterward, and peaked in early 2018. Survey expectations generally lag sentiment on average by 1 to 2 quarters. This is reasonable as surveys are always delayed, and our index reflects sentiment immediately. The correlation between the survey and our sentiment index equals approximately 0.33, statistically significant at 1% level. The correlation and parallel trends suggest that the sentiment index potentially captures part of the expectations on future house prices.

[Figure 2.3]

In addition, we also correlate the national sentiment with the consumer expectation index (CEI) extracted from the National Bureau of Statistics China. The CEI is calculated based on expectations for future income, employment status, economic situation, quality of life, etc. The expectation for home purchasing in two years is also included. When the CEI is higher, people believe there will be a higher income, greater employment opportunities, better life quality, and higher house prices. The correlation between our sentiment and CEI is expected to be positive. In fact, the coefficient of the two indices is 0.61, significant at 1%. Figure 2.4 plots CEI with sentiment index. The fluctuations are highly similar, and sentiment leads CEI for an average of 1 to 2 years. This could also be a support of our sentiment reflects part of expectations.

[Figure 2.4]

2.4 Social media sentiment and house prices

We estimate the effect of sentiment on house prices in a fixed effects panel regression.

$$\Delta p_{i,t} = \alpha + \sum_{j=1}^{m} \beta_j \Delta s_{i,t-j} + \sum_{k=1}^{n} \lambda_k \Delta p_{i,t-k} + \gamma \Delta x_{i,t-1} + city_i + \epsilon_{i,t} \tag{2.5}$$

p is the house price, and x is a set of control variables, including income, population, mortgage rate, unemployment rate, and economic uncertainty. i and t represent cities and

[26] We use the 35 cities' population as weights to compute the aggregate national sentiment index.

time, and n, m are the number of lags selected by standard criteria. All variables except mortgage rate are in logarithms.[27]

$city_i$ is the city fixed effect. We test for nonstationarity using the Fisher-type unit root test (Choi, 2001). This test performs a unit root test on each cross-section, respectively, then produces an overall test result. All variables, except for the social media sentiment index, accept the null hypothesis of unit root, indicating they are non-stationary. We therefore estimate the first differences, in line with Zhang & Tang (2016) and Soo (2018). In line with past studies (Case & Shiller, 1989; Soo, 2018; Guren, 2018), and following standard lag selection criteria, we include two-quarters of past price changes.[28] β_j are our key estimated parameters of interest. They capture the predictive impact of current and past sentiment on house prices that is orthogonal to the effects of the fundamentals and past price movements.

Table 2.3 presents the estimation results of equation (2.5). The first row reports the total accumulated effect of housing sentiment, β, on the current growth in prices. The subsequent rows break down the lagged effect of sentiment by each quarter. Our results show that a 1% increase in accumulated lagged sentiment results in a house price increase of approximately 0.81%. This result is significant at a 1% level of significance. A one standard deviation increase in one-and-a-half years of accumulated sentiment is associated with a 0.661% increase in future prices.

Columns 1 through 3 of Table 2.3 compare results with standard error clustered by different procedures. For comparison, we do not use robust standard error estimator in column 1. The other columns use robust standard error estimators to control for heteroskedasticity, cross-sectional dependence, and autocorrelation in the error terms.[29] Finally, in column 4, we examine the hypothesis of Soo (2018) that a national sentiment in news media accounts drives house prices at the local level. We include lagged national house price sentiment extracted from CUDS, as discussed in section 3. Doing so lowers our key elasticity to 0.57, but it remains significant at

[27] Following Soo (2018), we normalize our sentiment index to be positive ($S+100$), then we take the logarithm.

[28] Numerous studies have found that measures of momentum can often help to predict excess returns in finance market(Moskowitz et al., 2012; Cujean & Hasler, 2017). Indeed, Wang et al. (2006) reveal that lagged stock market return, a momentum measure, forecasts both implied and realized stock return volatility. Real estate, akin to financial assets, experiences periods of price appreciation and depreciation influenced by various economic factors and investor sentiments. Moreover, the liquidity and tradability of housing assets have increased over time, aligning more closely with financial assets. Given these parallels, exploring momentum in the housing market offers a valuable avenue for understanding the dynamics of real estate prices.

[29] To address this problem, we calculated Driscoll-Kraay standard errors. Following Soo (2018), we choose 12 lags. Lower lags are also tested, and the results remain consistent. This results in slightly higher standard errors of the aggregate sentiment, suggesting some heteroskedasticity or cross-sectional dependence. We further cluster standard errors by city and quarter in column 3, which reduces the standard errors of total sentiment.

1%. This result is consistent with Soo (2018)'s findings in the US that the national systematic component accounts for approximately 30% of the explanatory power of the local sentiment index.

[Table 2.3]

2.5 Interpretation

Although highly significant, our results that sentiment index changes predict house price changes up to 6 quarters into the future require interpretation. We first investigate the possibility that some of the measured sentiment reflects unmeasured fundamental forces of house prices. We then study the role housing market policies implemented in China over this period may play in our results. We study the role played by the implementation and removal of Home Purchase Restrictions the Chinese government imposed in a staggered fashion. We then look at the role of a variety of primarily cross-sectional effects in driving the results: the role played by access to information, speculative demand, sentiment tonal symmetry, as well as gender differences.

2.5.1 Sentiment and unobserved fundamentals

To test whether unobserved fundamentals drive sentiment, we borrow a method by Soo (2018) and create a social media fundamentals index. We construct the fundamentals index from Weibo posts by harvesting posts referencing fundamentals, such as rents, income, taxes, costs, etc.[30] To the extent that our sentiment index captures unobserved fundamentals, the inclusion of the media fundamentals index in our regression should result in a large drop in the significance and magnitude of our social media sentiment index.

Table 2.4 reports results that include social media fundamentals index to equation (2.5).[31] Our estimates remain robust - in fact, the key elasticity $\sum \beta$ is larger than before and still

[30] We construct five media fundamentals as shown in Table 2.5. "Media rents" refers to a city-level sentiment index of the posts discussing "rents"("zu" in Chinese). "Media user costs" is an index of posts discussing "taxes"("shui" in Chinese) and "costs"("chengben" in Chinese). "Media demographics" covers posts that discuss population("renkou" in Chinese), "income"("shouru" in Chinese) and "salary"("gongzi" in Chinese). "Media credit conditions" index is based on posts that contain "mortgage"("daikuan" in Chinese) and "interest rate"("lilv" in Chinese). "All media fundamentals" is the city-level index of posts that contains words of all these four sets of media fundamentals. In this section, we only include cities with a total number of posts larger than 15,000 to avoid small-sample bias by including cities with relatively few posts with relevant terms. There are 17 cities: Beijing, Changsha, Chengdu, Chongqing, Guangzhou, Hangzhou, Hefei, Nanjing, Qingdao, Shanghai, Shenzhen, Tianjin, Wuhan, Xiamen, Xi'an, and Zhengzhou.

[31] As with the fundamentals, we include contemporaneous values, as well as two lags as selected by lag-selection criteria.

significant at 1%. All components of the media fundamentals index have this magnifying effect on the key elasticity of the sentiment index. In column 1, we control for the index calculated from the posts discussing any words related to fundamentals, including rents, taxes, costs, population, income, salary, mortgage, and interest rate. Columns 2 through 5 add each media control sequentially to test the stability of the coefficient estimate for the overall sentiment index, β. The stability of our elasticity of interest following a sequential addition of controls indicates that bias from unobserved factors is less likely (e.g., Altonji et al., 2005).

[Table 2.4]

2.5.2 Sentiment and policy changes

Housing markets everywhere are highly regulated, and China's regulation evolved rapidly over the last two decades. The key measures include zoning changes on the supply side and purchase restrictions on the demand side of the housing market. These policies present the most likely unobserved driver of our results by simultaneously causing swings in social media sentiment, as well as directly driving house prices. In this section, we study the role of house purchase restrictions (HPR) in major cities. HPRs prohibit local residents (those with a registered permanent residence - Hukou) from buying more than two residential properties and non-resident purchasers without Hukou from buying more than one residential property.[32] HPR restrictions were implemented in three stages, first in 2010 in thirty-six major cities. Subsequently, the State Council issued "Five National Notices" on 20 February 2013, allowing municipalities to tailor HPRs to local conditions. After that, with the exception of Beijing, Shanghai, Guangzhou, and Shenzhen, all HPR-regulated cities withdrew from HPR by the end of 2014. When house prices rebounded in 2015, the second round of HPRs was implemented in 2016. We document the timing of the HPRs in Appendix A2.B. We can see clearly in Figure 2.5 that national house price growth subsided with the tightening of HPRs and rebounded when most were removed.

To separate the direct effect of sentiment on house prices from the effects induced by the HPRs, we perform a staggered Difference-in-differences (DID) exercise that focuses on the re-regulation policy in 2016.[33] According to Goodman-Bacon (2021), the average treatment effect from two-way fixed effects (TWFE) DID estimations is the variance-weighted averages of all

[32]Even non-Hukou buyers require supporting documents identifying them as being active in the locality, e.g., a certificate of local tax payments or social security records for a certain period.

[33]To do so, we shorten the data period to 2015q3 - 2020q2.

possible 2×2 DIDs. One problem is that there are "bad" comparisons.[34] Including those "bad" comparisons will lead to biased, sometimes even opposite, results in treatment effects even though the parallel assumption holds (Baker et al., 2022).

Stacked DID, proposed by Cengiz et al. (2019), recently further improved by Wing et al. (2024), is an approach to address the TWFE estimation bias. The idea is to create event-specific "clean" datasets, including the outcome variable and controls for the treated cohort and all other observations that are "clean" controls within the treatment window (e.g., not-yet-, last-, or never-treated units). For each clean dataset, we then generate a dataset-specific identifying variable. These event-specific data sets are then stacked together, and a TWFE DID regression is estimated on the stacked dataset, with dataset-specific unit- and time-fixed effects. The specification of our stacked Difference-in-differences (DID) model is given as follows:

$$\Delta p_{i,t} = \alpha + \sum_{j=1}^{m} \beta_j \Delta s_{i,t-j} + \sum_{l=-K}^{-2} \theta_l D_{i,t}^{l} + \sum_{l=0}^{L} \theta_l D_{i,t}^{l} + c_i + \tau_t + \epsilon_{i,t} \tag{2.6}$$

where $D_{i,t}^{l} = \mathbb{I}[t - E_i = k]$ is an indicator for a treatment city i in cohort E_i(the period of treatment) being k periods from the start of re-regulation. The first summation in the equation captures the periods leading up to the re-regulation ("leads"), and the second summation captures the periods following re-regulation ("lags"). The baseline is set to one-quarter before implementing re-regulation. c_i captures the city fixed effects while τ_t captures the time fixed effects. All other variables remain as in (2.5).

Table 2.5 reports the stacked DID results. The coefficient of the social media sentiment index remains robust and significant. Its size is much higher than in our baseline specification, indicating that sentiment's significance is unlikely to be caused by the changes in HPRs. Under this DID specification, a 1% increase in the lagged sentiment index over the past six quarters results in an increase in house price growth by 0.63%, ceteris paribus. We are therefore reassured

[34] Assuming there are three groups: an early treated group K which receives the treatment at time k; a later treated group L which receives the treatment at time l; and a never treated group U. All possible 2×2 DIDs when applying TWFE DiD estimations are as follows: a, the early treated group K compared with never treated group U; b, the later treated group L compared with never treated group U; c, the early treated group K compared with later treated group L, before L is treated at time l; d, early treated group K compared with later treated group L, after K is treated at time k. The ATT from TWFE DiD is simply the weighted average of the treatment effects from the four possible groups. It seems TWFE DiD works well for now. However, there is an implicit assumption that all treatment effects are constant or homogeneous. This assumption is hard to hold in empirical research. Once the treatment effects are heterogeneous, group d will become the "bad comparison". See Figure 2 in Goodman-Bacon (2021).

that HPR policy changes do not drive the social media sentiment result.[35] As expected, re-regulation harms future price changes. Again, this supports our main result that sentiment is more likely a social media sentiment is in itself a fundamental driver of house prices.

[Table 2.5]

2.5.3 Cross-sectional effects

We now study the cross-sectional sources of variation that could drive our result of social media sentiment raising local house prices. We then look at the roles played by tone and gender asymmetries.

The cross-sectional effect of sentiment on stock returns has been well documented (e.g., Baker & Wurgler, 2006; Baker et al., 2012; Ding et al., 2019; Smales, 2017). Baker & Wurgler (2006) suggest that sentiment traders shift from safe to speculative securities when sentiment rises and from speculative to safe securities when sentiment falls and that these sentiment-triggered demand shocks lead to mispricing in financial markets. They argue that the impact of sentiment on returns should be greater when the valuation difficulty and arbitrage constraints on stocks are particularly high, as it is difficult for investors to correct for stock mispricing through market sentiment. However, the sentiment channels through housing markets are less investigated. Chang et al. (2011) highlights the greater intensity of the effect in markets characterized by a higher level of collectivism and greater access to information media. On the other hand, Piazzesi & Schneider (2009) and Soo (2018) show that sentiment has a larger impact in markets with greater speculation. We discuss how two potential channels may amplify the impact of sentiment on price changes in housing markets: 1) where investors have greater access to information media. 2) where the speculative demand is particularly high.[36] We examine the first channel using city-level shares of internet subscribers as proxies for information access and the second channel using HPRs as an exogenous shock to speculative demand.

Information accessibility Following Gao et al. (2020), Soo (2018), Zheng et al. (2016) and Deng et al. (2022), we augment our empirical model to control for the informed share of the city population (measured by $C_{i,t}$, a percentage of internet users with broadband access to

[35] The parallel trends assumption holds. Please see Figure B1 in AppendixA2.B for more information. The baseline is set to one-quarter before implementing the re-regulation of HPRs (t-1).

[36] Using the Michigan Survey of Consumer, Piazzesi & Schneider (2009) find that "momentum" traders and other homebuyers are subject to sentiment and invest in houses because of an optimistic belief in increasing house prices. While Soo (2018) find empirically that US cities with a greater number of speculators have a higher sentiment impact on house prices.

city population).

$$\Delta p_{i,t} = \alpha + \beta_1 \sum_{j=1}^{m} \Delta s_{i,t-j} + \beta_2 \sum_{j=1}^{m} (\Delta s_{i,t-j} \times C_{i,t-j}) + \beta_3 C_{i,t} + \beta_4 \sum_{k=1}^{n} \Delta p_{i,t-k} + \beta_5 \Delta x_{i,t-1} + \tau_t + \epsilon_{i,t} \tag{2.7}$$

All other variables are as in (2.5). Our coefficient of interest is β_2. Suppose better internet access makes it easier for buyers to access information on social media sentiment. In that case, the sentiment elasticity should have a larger impact on house prices, implying a positive β_2. Indeed, in column 1 of Table 2.6, we report our estimate of a significantly positive β_2, which indicates that the role of social media sentiment is greater in cities with a higher percentage of internet users. A one standard deviation increase in the aggregate six quarters of sentiment index in the cities with the highest percentage of internet users will trigger a 0.181% higher increase in house price growth rate compared to the benchmark city with the lowest % internet users.

Speculative demand Chinco & Mayer (2016), Soo (2018), Gao et al. (2020) and others argue that the impact of sentiment should be stronger in the markets with more speculation. We test this channel using the second round of HPRs, which specifically curb the demand of housing speculators in Tier-1 and Tier-2 cities. We add interaction terms between the re-regulation and lagged sentiment into (2.7); however, now $C_{i,t} = 1$ if re-regulation is implemented in the city i and time t, and is now interacted with all included lags of media sentiment. All remaining variables remain the same with (2.5). Our main interest is the coefficient on the interaction term β_2: if re-regulation significantly reduces the speculative demand in regulated cities, sentiment elasticity should decline. Column 2 of Table 2.6 reports the results: the coefficient estimate of β_2 is negative but not significant, indicating only a weak support for the second channel of sentiment in China. The implementation of re-regulation did not significantly weaken the sentiment effect on future house price change.

Price growth rate Agents' reactions and sentiments may differ with high and low price growth periods. We divide city-level price growth data into high and low price growth rates and interact the high price growth dummy with contemporaneous sentiment indexes.[37] All other variables remain the same as (2.7), except that $C_{i,t}$ now represents six quarters of accumulated high price growth rate dummy. The results are reported in column 3 of Table 2.6. Our main

[37] We first calculate the average price growth rate for each city during the sample period; we then compare each city i's price growth rate at time t with city i's average price growth rate; if the growth rate at time t is larger, then high price dummy=1 for city i at time t, vice versa.

interest is the coefficient on the interaction term β_2. The negative and significant (at 10% significance level) coefficient (-0.304) indicates that sentiment matters more when price growth rates are low and vice versa. This result provides weak support to the assertation in the stock market that sentiment has a greater influence on market returns during recession (Lyócsa et al., 2020).

We have until now maintained an assumption of tone symmetry, namely that positive and negative social media sentiment have identical (but opposite) effects on house prices. Given the richness of our dataset, we now relax this assumption.

Positive and negative media sentiment Tetlock (2007); Loughran & McDonald (2011) argue that a negative word list captures the tone of text better.

We divide city-level sentiment data into positive and negative indexes as in equations (2.1) and (2.2), and re-estimate our results to test our earlier assumption of tone symmetry. The results are reported in column 3 of Table 2.6. We see that the accumulated elasticity of the negative sentiment index is larger in absolute value than the elasticity of the positive sentiment (-0.934 vs. 0.712). Both coefficients are statistically significant at 1% level and are not significantly different from each other in absolute value. This result supports the notion that negative sentiment has a larger role in driving house prices.

Gender China is characterized by a competitive marriage market. Males with houses are more competitive and achieve better outcomes in that market (e.g., Alm et al., 2021; Wrenn et al., 2019; Zheng et al., 2016). It is, therefore, rational for males to pay closer attention to house prices than females. We collect self-reported gender information from user profiles and separately re-calculate the social media sentiment index for male and female users. The results in column 4 of Table 2.6 confirm that the social media sentiment of males significantly predicts house price growth, but the sentiment of females does not. A 1% increase in the social media sentiment index of male posters results in an accumulated house price growth over a period of six quarters of approximately 0.97%. Because the marriage market forces males to buy houses, their sentiment more accurately represents the sentiment of the average buyer. This further confirms the result that buyer sentiment is a driver of house prices in China. If buyers feel positive about the housing market, they are more likely to buy, resulting in price increase due to higher demand. Since females are less likely to be real estate buyers in China, their social media sentiment about house prices has less predictive power for future house prices.[38]

[38] In Table A.2.F3, we present the number of posts, mean sentiment, mean positive sentiment, and mean negative sentiment categorized by gender. Additionally, we examine the disparities in sentiment across genders.

Popularity We finally explore whether considering the popularity of a post changes the predictive power of sentiment. On Weibo, every post can be "liked", reposted, or commented on by others. We use the data on the number of likes, reposts, and comments to measure the post's popularity. Using the same method as Baker et al. (2021) who uses Twitter data to construct an economic uncertainty index, we construct three new sentiment indices weighting each post with a number of likes, reposts, and comments, respectively:

$$S_{jt}^{*} = w \times S_{jt} \tag{2.8}$$

where w is the weight for each post, equal to $(1+\log(1+\#likes))$, $(1+\log(1+\#reposts))$, and $(1+\log(1+\#comments))$, respectively. We construct three new city-level sentiment indices using these three weighting methods. Columns 5 through 7 in Table 2.6 report the results of equation (2.5) with these indices. We see that changes in weighting do not significantly change elasticities from our baseline results in Table 4. A 1% appreciation in six quarters of accumulated lagged sentiment is associated with an increase in future quarterly price growth of approximately 0.83% when using "likes", 0.81% when using the number of reposts, and 0.8% when using the number of comments as weights, respectively. It appears that posts have a significant common sentiment, and weighting them by popularity does not improve the model's predictive power.

[Table 2.6]

2.6 Dispersion of sentiment and house sales

Dispersion of beliefs amongst market participants has been shown to predict trading volume and stock returns (e.g., Giannini et al., 2019; Cookson & Niessner, 2020; Bollerslev et al., 2018; Golez & Goyenko, 2022), but the evidence for such link in the housing market does not yet exist. We turn to this topic here.

Our empirical model follows the literature and is a close companion of equation (2.5), where we replace the left-hand side variable with the volume of house sales in city i at time t. We

The findings reveal that the quantity of posts authored by males is nearly three times that of females, and there exists a statistically significant difference in sentiment indices at the 10% level. Furthermore, males exhibit a tendency towards greater positivity compared to females, with this discrepancy being significant at the 1% level. These findings lend support to our assertion that males exhibit heightened attentiveness towards house prices and are inclined to purchase houses when experiencing positive sentiments, thereby contributing to the increase of house prices.

use two variables to quantify house sales: the number of sales and the total area sold (m^2). Following Bollerslev et al. (2018) and Egan et al. (2022), we use mean-aggregated daily standard deviation as a proxy for disagreement.[39]. All other variables remain the same as (2.5).

The results are reported in Table 2.7. We find that a 1% increase in accumulated four quarters lagged disagreement can predict a 1.007% decrease in the total number of sales and a 1.151% decrease in total area sold in the following quarter, ceteris paribus. These elasticities are jointly significant at 5%. In column (2) and column (4), we include the national economic policy uncertainty index to account for national-level disagreement. Doing so, the key elasticities are slightly lower (-1.002 and -1.135, respectively) but remain significant. The results indicate that the national systematic component of disagreement only accounts for a small fraction of the explanatory power of the local disagreement.

These results support the notion that house purchases are frequently seen as a form of investment in China. The observed speculative behavior in the market, such as "down payment loans", "bridge loans", "Yin-Yang contracts" and "strategic divorces", are highly sensitive to disagreement, and this is borne out in our results. When the disagreement is high, a decrease in aggregate speculative demand significantly lowers house transactions, both in their number and in the area sold.

Insert [Table 2.7] here

2.7 Robustness checks

2.7.1 Placebo test

We test the possibility that the spurious nature of our data drives the predictive power of sentiment by conducting a placebo test. We randomly assign city i's sentiment to non-i city.[40] We then measure the impact of this pseudo-sentiment variable on house prices in our baseline regressions. We repeat this procedure 1000 times in a bootstrap procedure to generate a distribution of the pseudo-sentiment coefficient in our baseline regression.

[Table 2.8]

[39] We first calculate the daily standard deviation of the sentiment using all posts in a day for each city. To construct the quarterly index, we then take the average of all daily standard deviations of sentiment within a quarter. The number of lags is selected following standard lag selection criteria

[40] Specifically, the steps are: We generate a random number for each city. Then, we keep the non-i city with the closest random number to the city i and replace city i's Δs_i with that non-i city's $\Delta s_{j,j\neq i}$.

The results of this analysis are reported in Table 2.8. For each of the pseudo-sentiment lag coefficients, we report a set of percentiles from the corresponding empirical distribution of pseudo-sentiment coefficient estimates. To facilitate comparison, we also report the actual sentiment coefficient estimates from Table 2.3. Table 2.8 shows that the value of 0 is between the 5th and 95th percentiles, implying that a pseudo-sentiment generated by randomly assigning city IDs is insignificant in our baseline regression. This further supports our claim that our baseline results are not due to spurious correlation.

2.7.2 Censorship bias

Online censorship is robust and widespread in China, and while it is generally focused on political discourse, it can potentially bias our results. First, findings of government-owned news media bias support the thrust of our effort to measure housing market sentiment using social media instead (Earl et al., 2022; Chen & Yang, 2019). Others find that an authoritarian government's interaction with social media is more complex: for example, (Qin et al., 2017) argue that China allows freer social media as a large number of posts on highly sensitive topics are published and circulated on social media.

If political online censorship in China filters out some of the negative posts on the housing market, then our estimated sentiment impact has an upward bias. To assess the extent of this bias, we exploit the limited short-term resource constraint created by the limited number of the censors. Censors pay less attention to the housing market at times of significant political-related events, which hinder people's confidence in the government. To assess these semi-natural experiments, we create a censorship dummy that coincides with the period that starts with the Hong Kong protests and interprets it as a proxy for the relaxation of censorship.[41] By including this censorship dummy and interacting it with our sentiment indexes, we can quantify the effect of censorship bias. Due to limited time resources, we expect a negative coefficient of the interaction term.

[Table 2.9]

Our results are reported in Table 2.9. The interaction coefficients are -0.533 (statistically significant at 10% level) with city fixed effects and 0.153 (and not significant) with city-year fixed effects.[42] Our key elasticity is only marginally smaller than our baseline estimate (0.797

[41] Hong Kong protest =1 if after 2019q1, =0 otherwise.

[42] This is our preferred method as we controlled for the variation with city and year at the same time.

vs. 0.81). We alternatively use a Cybersecurity Law dummy, which changes our baseline results even less. We believe these results support the notion of the limited impact of censorship on housing markets.

2.7.3 Structural break

We also test whether there is a structural break for sentiment elasticity. The Cybersecurity Law, issued in the last quarter of 2016 and enacted in the second quarter of 2017, specifies the real-name system on the Internet for the first time in law. The implementation of the law may result in a structural change in sentiment. We create a dummy variable for the Cybersecurity Law; it equals one after the second quarter of 2017 and equals zero otherwise.

The results are reported in the third and fourth columns of Table 2.9. The first row reports the coefficient of the interaction term, the second row reports the coefficient of accumulated six quarters of lagged sentiment, while the last row reports the coefficient of accumulated six quarters of lagged Cybersecurity Law dummy. The elasticity of interaction is insignificant (-0.316 with city fixed effects and -0.055 with city-year fixed effects), indicating that Cybersecurity Law does not statistically significantly change the impact of sentiment on house prices.[43]

2.7.4 Other robustness checks

In addition to the placebo test, censorship bias, and structural break, we consider additional regressions to establish the robustness of our analysis.

Although China's economic growth and the policies that facilitated it have progressed in waves from the East to the West of China, we run regressions for the Eastern, Central, and Western Chinese cities. All the results remain significant, but the sentiment index plays a smaller role the further one moves westwards from the Chinese seaboard. A lower use of social media and internet penetration in western China is the likely driver of this result. We additionally estimate a Weighted Least Squares regression to address heteroscedasticity using population weights. WLS results remain significant (both sets of results are reported in Table D1 in Appendix A2.D).

[43] We also test for a joint significance of the interaction and Cybersecurity Law dummy; the p-value of 0.905 rejects the null hypothesis (jointly significant) of the F test, again indicating that there is no structural break because of the Cybersecurity Law.

We next replace the mean-based measure of sentiment in each city with a median to assess the impact of posts with outlier sentiment scores on our baseline results. Results with median-based index remain largely unchanged (See Table D2). Our results are also insensitive to our criteria for selecting Weibo posts. If we limit the posts about the housing market in a given city to those by city residents only, our results do not change meaningfully (Table D2).[44]

According to Dragut & Fellbaum (2014), degree adverbs amplify the sentiment of text data (for example, 'very happy' expresses a stronger tone than "happy"). To capture this, we recompute the sentiment index using a list of degree adverbs from the HowNet Chinese Sentiment Dictionary (Fen et al., 2019; Xu et al., 2019) while assigning weights to the degree adverbs as in Zhang et al. (2018). Again, the inclusion of the degree adverbs does not change our baseline results (Table D2).

As another precaution against the possibility that our social media sentiment index captures fundamentals, we follow Baker & Wurgler (2006) and orthogonalize sentiment to macroeconomic factors (income, population, mortgage rate, unemployment rate, and economic uncertainty index) to avoid contamination of the former by the latter. We then extract the first principal component from the residuals from that auxiliary regression. We apply this alternative sentiment index in our empirical analysis and find the results are again qualitatively unchanged (Table D2).

Our sentiment index is constructed based on Bian et al. (2021). To ensure the robustness of our results to the choice of the dictionary, we reconstruct the sentiment index using a newly published Chinese Financial Sentiment Dictionary by Du et al. (2022). We find that our results are qualitatively unchanged (same significance at 1%), with a slightly reduced baseline magnitude of the elasticity of the accumulated sentiment index (0.595 vs. 0.776). We conclude that the prediction power of sentiment is not conditional on the selection of a sentiment dictionary.

Finally, we consider the differences in frequency in our data. Income, mortgage rates, and population are reported at annual frequency. To assess the possibility that our results are driven by the missing higher frequency variation in the fundamentals, we use linear interpolation to generate quarterly values for the annual variables and consequently re-estimate our main regression. Our baseline results are unchanged (Table D2). Similarly, our findings do not

[44] Because location-reporting is not compulsory, limiting analysis to posts by self-reported residents significantly lowers the number of observations.

change if we re-estimate all regressions with monthly (rather than quarterly) house price index and sentiment index (Tables D3 and D4).

We conclude that all robustness checks confirm a significant predictive power of sentiment on house price growth in Chinese cities after controlling for observable fundamentals.

2.8 Conclusion

We developed a new housing market social media sentiment index using over 800,000 daily posts between 2010 and 2020 from a leading Chinese social media platform, Weibo. We find that this sentiment index significantly predicts city-specific house price changes up to 6 quarters ahead across the 35 largest cities after controlling for the fundamental economic drivers of price changes. A 1% increase in the accumulated lagged sentiment results in a house price increase of approximately 0.81%, ceteris paribus, and is highly significant.

We first follow Soo (2018) to prove that our result is not a consequence of unmeasured fundamentals. Second, we conduct a difference-in-differences study of a series of home purchase restriction policies imposed by the Chinese government. We find that the imposition and later removal of these policies cannot explain the magnitude nor the size of our baseline results. Third, we explore several theoretically-founded drivers of cross-sectional effects. In line with the theoretical predictions, we find that the predictive power of sentiment is stronger in cities with better access to information. We find weak support for a theoretical prediction that the impact of sentiment is stronger in more speculative markets. Fourth, we find support for the arguments in Tetlock (2007); Loughran & McDonald (2011) that a negative tone of measured sentiment has a stronger predictive power on price changes. Fifth, we find support for the competitive Chinese marriage market hypothesis, in that social media sentiment by male participants significantly predicts the house price changes, while that by females does not.

We finish with a battery of further robustness checks. We weigh posts by their popularity when constructing the sentiment index. We use different methods of aggregating posts at the city-level. We use population weights in Weighted Least Squares regression. We use a different measure of the tone using adverbs. Finally, we orthogonalize our sentiment index to economic fundamentals to eliminate possible contamination as discussed in Baker & Wurgler (2006), and estimate using different data frequencies. None of these changes meaningfully alter our results.

We conclude that our results give support to hypotheses that social media sentiment can exemplify a measure of ‘animal spirits’ deemed important by a range of models from Keynes (1936) to Acharya et al. (2021), or that social media sentiment is a sign of social learning as people shape their own believes through interactions with others (Bailey et al., 2018a; Bayer et al., 2021; Burnside et al., 2016).[45]

[45]We also note that our results extend quantity of houses sold, not merely the average house price. See Table F1 in Appendix A2.F.

Tables and figures

(a) positive sentiment words

(b) negative sentiment words

Figure 2.1: Sentiment words in posts(in Chinese)

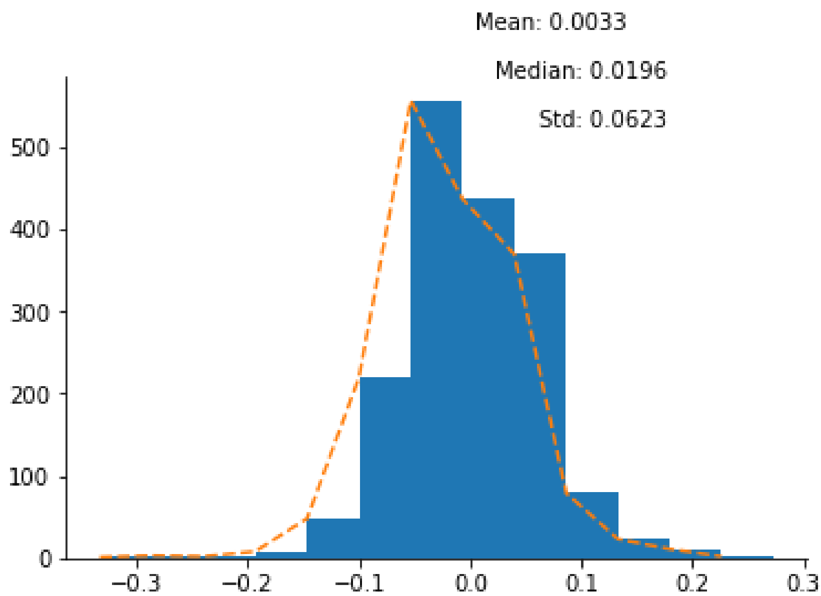


Figure 2.2: The distribution of sentiments(Beijing, 2010q1)

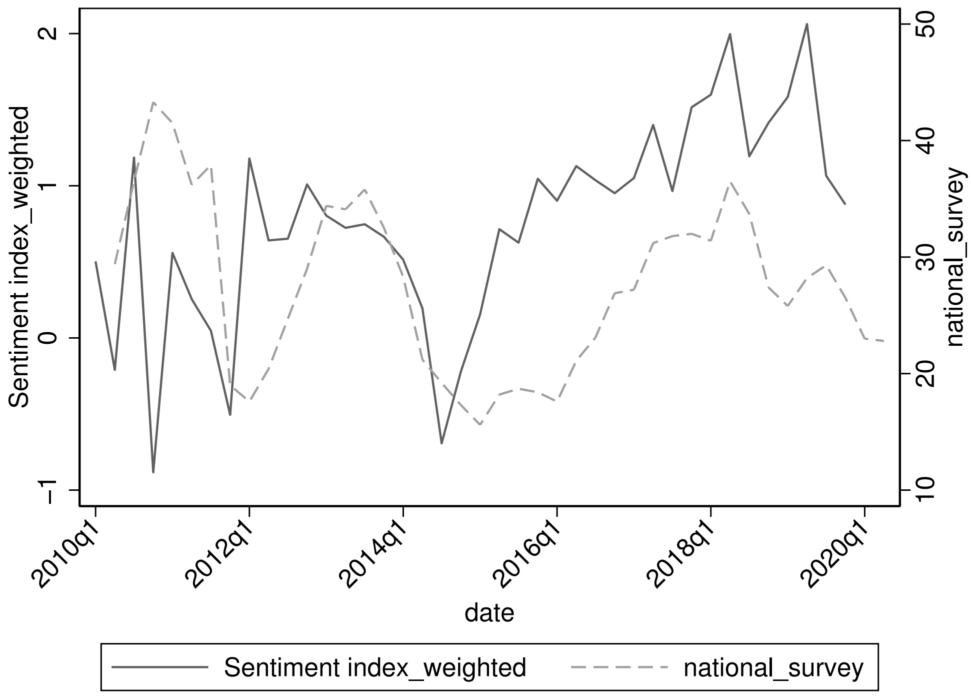


Figure 2.3: Validating sentiment with survey expectations

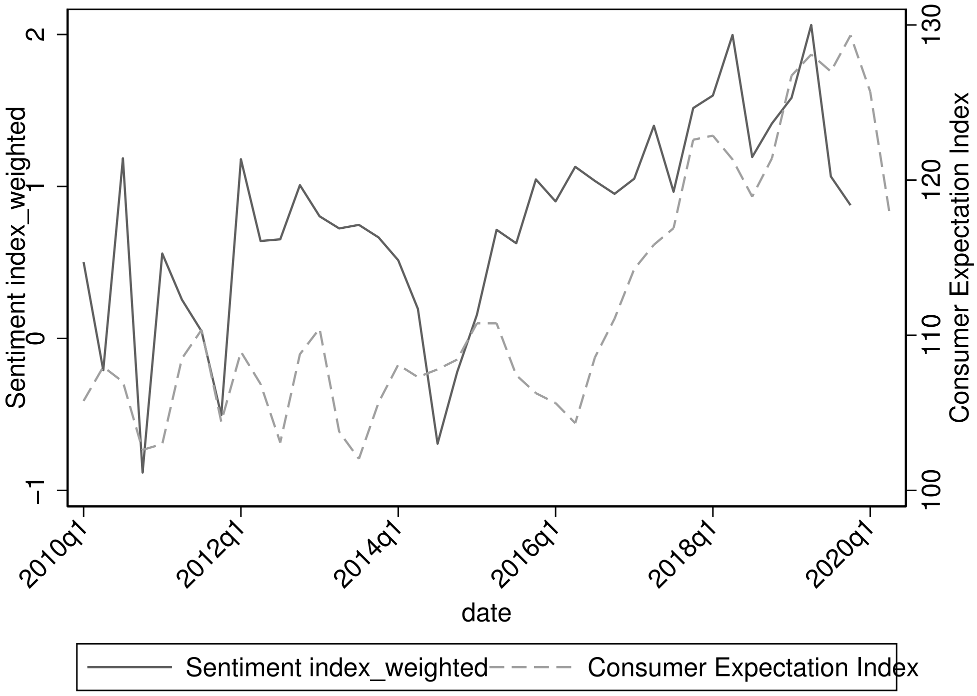


Figure 2.4: National sentiments and survey expectations

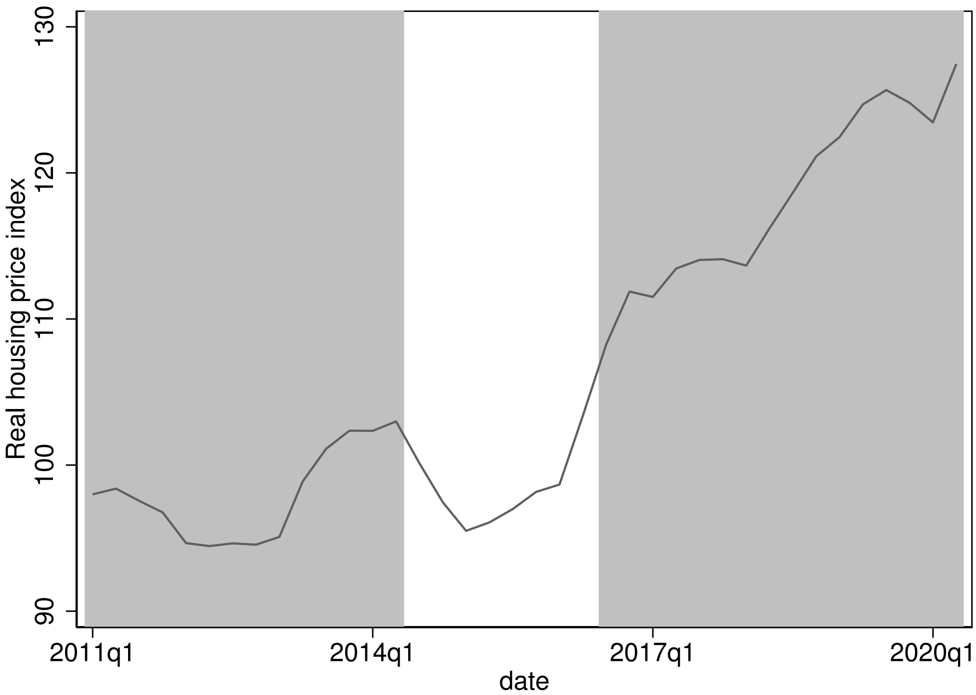


Figure 2.5: National real house prices and house-purchase restrictions

Table 2.1: Housing-related Social Media Posts by City

City	Number of posts	City	Number of posts	City	Number of posts
Beijing	108,278	Hefei	20,645	Shenzhen	113,525
Changchun	4,986	Hohhot	2,133	Shijiazhuang	9,677
Changsha	18,690	Jinan	22,515	Taiyuan	4,372
Chengdu	22,110	Kunming	7,687	Tianjin	16,628
Chongqing	22,402	Lanzhou	4,106	Urumqi	1,688
Dalian	8,457	Nanchang	12,212	Wuhan	46,807
Fuzhou	10,011	Nanjing	23,750	Xiamen	26,640
Guangzhou	63,934	Nanning	6,535	Xi'an	24,476
Guiyang	5,222	Ningbo	7,126	Xining	1,045
Harbin	3,144	Qingdao	32,765	Yinchuan	1,885
Haikou	5,695	Shanghai	120,861	Zhengzhou	36,442
Hangzhou	20,586	Shenyang	8,895		

Table 2.2: High Frequency Sentiment Words

	Positive	Frequency	% of pos words	Negative	Frequency	% of neg words
1	Develop	56062	3.82	Fall	113762	11.19
2	Highest	40209	2.74	Decline	78588	7.73
3	Become	39365	2.68	Stress	40545	3.99
4	Newest	34222	2.33	Bubble	29372	2.89
5	Grow	31576	2.15	unable	15775	1.55
6	Skyrocket	30437	2.07	Plummet	12689	1.25
7	Hotspot	29389	2.00	Slipping	12613	1.24
8	Reach	28582	1.95	Severe	12181	1.20
9	Reasonable	24857	1.69	Collapse	12040	1.18
10	Increase	21394	1.45	Less than	11653	1.15
11	Rise	20120	1.37	Cancel	11521	1.13
12	Talent	19796	1.35	Crazy	11365	1.12
13	Emphasis	19732	1.34	Plunge	9716	0.96
14	Stabilizing	18405	1.25	Restrain	9069	0.89
15	Ability	16598	1.13	Speculation	8992	0.88
16	Comprehensive	15880	1.08	Complain	8814	0.87
17	Core	15595	1.06	Downward	8689	0.85
18	Happy	15100	1.03	Limit	8417	0.83
19	Steady	14958	1.02	Violate	8205	0.81
20	Chance	13628	0.93	Reduce	8087	0.80

Note: The table is calculated using all posts collected. The Chinese words have been translated into English.

Table 2.3: Predicting house price growth with housing sentiment

	Pooled OLS				
Dependent variable: Δ log(housing price)$_t$	(1)	(2)	(3)	(4)	(5)
Sum of lagged sentiment index	0.805***	0.805***	0.805***	0.776***	0.568***
	(0.137)	(0.149)	(0.120)	(0.177)	(0.083)
Qtr 1 Δs_{t-1}	0.069**	0.069***	0.069***	0.063***	0.033*
	(0.028)	(0.016)	(0.013)	(0.024)	(0.017)
Qtr 2 Δs_{t-2}	0.142***	0.142***	0.142***	0.151***	0.084***
	(0.033)	(0.024)	(0.024)	(0.035)	(0.018)
Qtr 3 Δs_{t-3}	0.139***	0.139***	0.139***	0.150***	0.091***
	(0.036)	(0.035)	(0.028)	(0.044)	(0.016)
Qtr 4 Δs_{t-4}	0.155***	0.155***	0.155***	0.130***	0.136***
	(0.034)	(0.049)	(0.040)	(0.045)	(0.032)
Qtr 5 Δs_{t-5}	0.206***	0.206***	0.206***	0.176***	0.141***
	(0.031)	(0.032)	(0.039)	(0.039)	(0.028)
Qtr 6 Δs_{t-6}	0.095***	0.095***	0.095***	0.105***	0.083***
	(0.025)	(0.023)	(0.021)	(0.028)	(0.015)
Lagged price changes	√	√	√	√	√
Lagged fundamentals	√	√	√	√	√
Lagged national sentiment	.	.	.	.	√
SE: Driscoll-Kray	.	√	.	.	.
SE: double clustered by (i,t)	.	.	√	√	√
City FEs	√	√	√	.	√
City-Year FEs	.	.	.	√	.
R^2	0.662	0.662	0.662	0.785	0.704
N	1156	1156	1156	1156	1156

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) The fundamentals include: average urban household disposable income, population, mortgage rate, unemployment rate and economic policy uncertainty index. (4)All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). (5) The first row reports the accumulated impact of six quarters of sentiment growth on current quarterly price growth. The subsequent rows break down this total impact by quarter. Column 5 controls for an additional 6 quarter lags of national housing sentiment from the Peoples Bank of China Survey.

Table 2.4: Sentiment and unobserved fundamentals

	Pooled OLS				
Dependent variable: Δlog(housing price)$_t$	(1)	(2)	(3)	(4)	(5)
Sum of lagged sentiment	1.214***	1.215***	1.044***	1.107***	1.052***
	(0.172)	(0.230)	(0.249)	(0.326)	(0.308)
All media fundamentals	√	.	.	.	.
Media rents	.	√	.	.	.
Media user costs	.	√	√	.	.
Media demographics	.	√	√	√	.
Media credit conditions	.	√	√	√	√
Lagged price changes	√	√	√	√	√
Lagged fundamentals	√	√	√	√	√
SE: double clustered by (i,t)	√	√	√	√	√
City FEs	√	√	√	√	√
R^2	0.680	0.684	0.713	0.718	0.724
N	553	539	497	497	472

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) The fundamentals include: average urban household disposable income, population, mortgage rate, unemployment rate and economic policy uncertainty index. (4)All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). (5) The first row reports the accumulated impact of six quarters of sentiment growth on current quarterly price growth. (6) Media fundamentals are a series of social media indices constructed using post that mention particular fundamentals. “Media rents” refers to a city level sentiment index of the posts discussing “rents”(“zu” in Chinese). “Media user costs” is an index of posts discussing “taxes”(“shui” in Chinese) and “costs”(“chengben” in Chinese). “Media demographics” covers posts that discuss population(“renkou” in Chinese), “income”(“shouru” in Chinese) and “salary”(“gongzi” in Chinese). “Media credit conditions” index is based on posts that contain “mortgage”(“daikuan” in Chinese) and “interest rate”(“lilv” in Chinese). “All media fundamentals” is the city level index of posts that contains words of all these four sets of media fundamentals. (7) We only include cities with a total number of posts larger than 15,000 in this section to avoid small-sample bias by including cities with relatively few posts with the relevant terms. There are 17 cities included: Beijing, Changsha, Chengdu, Chongqing, Guangzhou, Hangzhou, Hefei, Nanjing, Qingdao, Shanghai, Shenzhen, Tianjin, Wuhan, Xiamen, Xi’an, and Zhengzhou. The number of observations are different across columns because media fundamental indices are unbalanced.

Table 2.5: Sentiment and policy changes

Dependent variable: Δlog(housing price)$_t$	Stacked DID
Sum of lagged sentiment	0.626***
	(0.217)
Sum of pre re-regulation	-0.056
	(0.037)
Sum of post re-regulation	-0.486**
	(0.218)
City-Time FEs	√
SE: double clustered by (it)	√
Ajusted R^2	0.574
N	382

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) House price index and sentiment index are at quarterly frequency. House price index is expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). (5) The first row reports the accumulated impact of six quarters of sentiment growth on current quarterly price growth. The second row reports the accumulated impact of pre re-regulation, while the third row is the accumulated post period impact. (6) Propensity score matching is used to 1:1 match treated city with untreated city using average disposable income, end of year population and unemployment rate. (7) Beijing, Shanghai, Guangzhou and Shenzhen are excluded as there were only one round of HPR. For implementation date of HPR, please see Table B1. (8) The parallel trends assumption holds. Please see Figure B1 in AppendixA2.B for parallel test and dynamic effects.

Table 2.6: House price growth and robustness sentiment indexes

Dependent variable: $\Delta\log(\text{housing price})_t$	Pooled OLS							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sum of lagged sent.	0.821***	1.181***	1.192***					
	(0.247)	(0.343)	(0.216)					
×% of internet users	3.764**							
	(1.736)							
×re-regulation		-0.444						
		(0.982)						
×high price growth			-0.304*					
			(0.182)					
Sum sent. (positive)				0.712***				
				(0.244)				
Sum sent. (negative)				-0.934***				
				(0.150)				
Sum sent. (male)					0.974***			
					(0.207)			
Sum sent. (female)					0.036			
					(0.062)			
Sum sent. (weight=likes)						0.828***		
						(0.162)		
Sum sent. (weight=reposts)							0.810***	
							(0.151)	
Sum sent. (weight=comments)								0.796***
								(0.140)
Lagged price changes	√	√	√	√	√	√	√	√
Lagged fundamentals	√	√	√	√	√	√	√	√
SE: double clustered by (i,t)	√	√	√	√	√	√	√	√
City FEs	.	.	√	√	√	√	√	√
R^2	0.663	0.710	0.449	0.664	0.670	0.664	0.664	0.664
N	1130	517	1156	1156	1040	1156	1156	1156

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) The fundamentals include: average urban household disposable income, population, mortgage rate, unemployment rate and economic policy uncertainty index. (4) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). (5) Each row reports the accumulated impact of a lagged one and half year of sentiment growth on future quarterly price growth. The second row reports the interaction term of accumulated sentiment and % of internet users, while the third row shows the interaction term of accumulated sentiment and re-regulation of HPR. The fourth row show the interaction term of accumulated sentiment and contemponeous high price growth dummy (High price growth for city i equals to one if the house price growth is higher than city i's average price growth during the sample period). The sentiment indices in the fifth and sixth row indicate positive and negative sentiment, respectively. The sentiment indices in the seventh and eighth row are calculated from posts written by male and female posters, respectively. Sentiment indices in the ninth, tenth, and the last row are constructed by weighing posts with the number of likes, reposts, and comments, respectively.

Table 2.7: Dispersion of sentiment and housing sales

Dependent variable:	log(total number of sales)$_t$		log(total area sold)$_t$	
	(1)	(2)	(3)	(4)
Sum of lagged dispersion of sentiment	-1.007**	-1.002**	-1.151**	-1.135**
	(0.465)	(0.469)	(0.486)	(0.501)
Qtr 1 $\Delta stddevs_{t-1}$	-0.121	-0.119	-0.172	-0.165
	(0.216)	(0.217)	(0.181)	(0.183)
Qtr 2 $\Delta stddevs_{t-2}$	-0.242*	-0.242*	-0.284*	-0.285*
	(0.136)	(0.138)	(0.158)	(0.164)
Qtr 3 $\Delta stddevs_{t-3}$	-0.418***	-0.416***	-0.449***	-0.443***
	(0.104)	(0.106)	(0.134)	(0.140)
Qtr 4 $\Delta stddevs_{t-4}$	-0.226***	-0.225***	-0.246***	-0.242***
	(0.071)	(0.071)	(0.078)	(0.080)
Lagged housing sales changes	√	√	√	√
Lagged fundamentals	√	√	√	√
Lagged EPU	.	√	.	√
SE: double clustered by (it)	√	√	√	√
City-Year FEs	√	√	√	√
Adjusted R^2	0.799	0.799	0.807	0.807
Observations	755	755	731	731

Notes: The first row reports the accumulated impact of four quarters of sentiment dispersion growth on current quarterly housing sales in logarithm. The subsequent rows break down this total impact by quarter. (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) The fundamentals include: average urban household disposable income, population, mortgage rate, unemployment rate. EPU represents for national economic policy uncertainty index. (4) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). Total number of sales, total area sold (measured in square meter) and sentiment dispersion are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). (5) Sentiment dispersion is calculated as follows: first, the daily standard deviation of the sentiment index is calculated, and then, the average of all daily standard deviations within a quarter is taken to construct the quarterly value. (6) The first row reports the accumulated impact of four quarters of sentiment dispersion growth on current quarterly housing sales growth. The subsequent rows break down this total impact by quarter. (7) The number of observations are different across columns because housing sales are unbalanced. In addition, Shenyang, Zhengzhou, Taiyuan and Urumqi are not included due to data unavailability.

Table 2.8: Placebo test

	Δs_{t-1}	Δs_{t-2}	Δs_{t-3}	Δs_{t-4}	Δs_{t-5}	Δs_{t-6}
1	-0.104	-0.104	-0.125	-0.115	-0.121	-0.083
5	-0.067	-0.080	-0.085	-0.079	-0.075	-0.059
25	-0.029	-0.037	-0.035	-0.033	-0.033	-0.027
75	0.027	0.033	0.037	0.034	0.029	0.022
95	0.064	0.076	0.085	0.083	0.076	0.059
99	0.099	0.117	0.122	0.123	0.103	0.086
mean	-0.001	-0.001	0.000	0.001	-0.002	-0.002
median	-0.001	-0.002	-0.001	-0.000	-0.003	-0.001
min	-0.132	-0.142	-0.158	-0.192	-0.201	-0.146
max	0.150	0.170	0.199	0.195	0.149	0.122
actual coefficient	0.063	0.151	0.150	0.130	0.176	0.105

Notes: This table presents the results of placebo tests. Δs is the quarterly average sentiment index calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). we randomly assign city i's sentiment to non-i city[46] We then measure the impact of sentiment based on the pseudo-sentiment variable instead of the true sentiment variable. We repeat this procedure 1000 times, thereby generating 1000 coefficient estimates of the pseudo-sentiment variable for each lagged sentiment employed in our analysis. The distribution of the sentiment coefficient under the scenario that the relation between sentiment and house prices is of a spurious nature. The results of this analysis are reported in Table 10. For each of the sentiment variables, we report a set of percentiles from the corresponding empirical distribution of pseudo-sentiment coefficient estimates. For comparison, we also report the actual estimate of coefficient, replicated from Table 2.3.

Table 2.9: Censorship bias

Dependent variable: $\Delta\log(\text{housing price})_t$	Hong Kong protest (1)	(2)	Cybersecurity Law (3)	(4)
Sum of sentiment × censorship dummy	-0.533*	0.153	-0.316	-0.055
	(0.321)	(0.327)	(0.214)	(0.393)
Sum of sentiment	0.797***	0.742***	0.810***	0.811***
	(0.134)	(0.176)	(0.156)	(0.200)
Sum of censorshop dummy	-0.002	-0.013 ***	0.002 **	0.008
	(0.002)	(0.003)	(0.001)	(0.004)
Lagged price changes	√	√	√	√
Lagged fundamentals	√	√	√	√
SE: double clustered by (i,t)	√	√	√	√
City FEs	√	.	√	.
City-Year FEs	.	√	.	√
R^2	0.667	0.790	0.655	0.714
Observations	1156	1087	1156	1087

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) The fundamentals include: average urban household disposable income, population, mortgage rate, unemployment rate. EPU represents for national economic policy uncertainty index. (4) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). Total number of sales, total area sold (measured in square meter) and sentiment dispersion are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). (5) There are two censorship dummies: the first dummy represents HongKong protest (=1 if after 2019q1, =0 otherwise), the seconde dummy represents Cybersecurity Law (=1 if after 2016q4, =0 otherwise). (6) The first row reports the accumulated impact of interacted six quarters of sentiment growth and censorship dummies on current quarterly price growth. The second row reports the accumulated impact of six quarters of sentiment growth; while the last row reports the accumulated impact of censorship.

A2.A Housing Fundamentals and Additional Variables

To identify the role of sentiment in predicting house prices, we need to control for other fundamental economic variables that influence the price of housing. We collect data on the following fundamentals:

1. *Income* Higher incomes cause higher house prices through a variety of demand-dominated channels documented in, i.e., Head et al. (2014). As we are not estimating a structural model, we are agnostic about individual channels.

   To calculate real incomes, we deflate the annual city-level average urban household disposable income (yuan) from the National Bureau of Statistics of China("NBS" hereafter) by CPI. Following the literature, we expect a positive relationship between income and house prices.

2. *Population* Population growth increases demand for housing and therefore the house price (e.g., Chinco & Mayer, 2016; Ortalo-Magne & Rady, 2006). We use city-level end-of-year urban population data between 2010 and 2020 from the NBS.

3. *Mortgage rate* There is considerable evidence that easier credit, facilitated by lower interest rates, results in increased housing demand and prices (e.g., Himmelberg et al., 2005; Mian & Sufi, 2009). Until 2019, commercial banks in China followed fixed mortgage rates published by the People's Bank of China (PBC). Since March 2019, when PBC released a new regulation to improve the loan prime rate (LPR) formation mechanism, commercial banks have referred to LPR when issuing loans. We use monthly nominal mortgage rates from 2010 to 2020 from NBS and convert them to real using CPI inflation. We follow Zheng et al. (2016) and lag mortgage rate by one quarter in the empirical analysis to reduce the endogenous problem because of interest regulation.

4. *Unemployment rate* The unemployment rate has a well-documented impact on housing demand Rosen (1979). We use city-level unemployment rates from the NBS.

5. *Economic policy uncertainty* According to Huang et al. (2020a), economic policy uncertainty(EPU) increases house price variation. Housing market risk grows when economic policies are less certain, which may affect house prices. We collect the monthly EPU index of China as constructed by Baker et al. (2016) and convert it into quarterly frequency.

6. *House prices* We have used the "NBS house price index" since 1997 for 35 large- and medium-sized cities on a quarterly basis, after adjusting it for CPI inflation.[47]

Figure 1 plots the national real house price index along with the SMSI. The sentiment index follows a similar general pattern as the house prices but with some significant differences in timing. It appears that the sentiment leads to the real house price.

Insert [Figure A2.A.1] here

Table 1 shows the statistical summary of all variables. The average and median sentiment indices are 0.86 and 0.94, respectively. Individuals are, on average, optimistic about the housing market, consistent with Fang et al. (2016).

Insert [Table A2.A.1] here

[47] In 2005, the total number of cities increased to 70 and the quarterly indices were replaced with monthly ones.

FIGURE A2.A.1: National real house prices and sentiment

Table A2.A.1: Summary of statistics

	Obs.	Mean	Std. Dev.	Min	Mdn	Max
Sentiment Index:						
sentiment index	1454	0.86	1.79	11.28	0.94	9.87
Housing Price Index:						
nominal price index	1454	125.78	26.30	90.97	115.45	231.35
real price index	1454	106.56	17.88	78.42	99.86	189.85
Fundamentals:						
real disposable income per capita(Yuan)	1454	30197.60	9023.47	10964.38	29249.57	59271.86
population(10 thousand)	1454	853.01	614.70	180.21	715.76	3205.42
real mortgage rate	1454	5.11	1.14	2.82	5.21	7.37
unemployment rate	1454	2.91	0.75	0.80	3.00	4.90
economic policy uncertainty	1454	170.64	97.62	75.91	126.695	444.53

Note: All control variables are at annual frequency, except for policy uncertainty and mortgage rate(quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. Sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD).

A2.B Social Media Data Collection

We use Sina Weibo accounts to log into the Weibo platform. We then call Weibo API using our search keywords and request the data. City names and "house price" are used to search all posts that meet the criteria. As the Weibo API limits data access (Rate Limits) per hour and day, building an efficient workflow is necessary. During repeated searches, we check the already-downloaded posts so as not to extract the same posts again. Each search will return bulks of posts, which are separated into many pages. To maximize the number of posts collected, we set the number of pages to 100. Moreover, the search will automatically stop when there is no more page. After that, we convert the documents retrieved to json format using ".json" command in Python. The content of posts, the date, the total number of likes, the total number of forwards, and the total number of comments are extracted from the results. Fourth, we collect the following username information: gender, whether they are verified or not, the type of verification, date of birth, university education, location, yangguang credit, and the total number of followers. [48].

Table A2.B.1: Home restriction policies

City	Initial regulation date	Deregulation date	Re-regulation
Beijing	4/30/2010	–	–
Shenzhen	9/30/2010	–	–
Xiamen	10/1/2010	8/15/2014	8/31/2016
Shanghai	10/7/2010	–	–
Ningbo	10/9/2010	7/30/2014	–
Fuzhou	10/10/2010	9/23/2014	10/6/2016
Hangzhou	10/11/2010	7/29/2014	9/18/2016
Dalian	10/11/2010	9/3/2014	–
Nanjing	10/12/2010	9/21/2014	9/25/2016
Tianjin	10/13/2010	10/17/2014	10/1/2016
Guangzhou	10/15/2010	–	–
Chengdu	11/21/2010	7/16/2014	10/1/2016
Lanzhou	11/25/2010	9/3/2014	–
Zhengzhou	12/31/2010	8/9/2014	10/2/2016
Taiyuan	1/11/2011	8/4/2014	–
Wuhan	1/14/2011	9/24/2014	11/14/2016
Kunming	1/18/2011	8/11/2014	–
Nanchang	1/20/2011	8/12/2014	10/8/2016
Jinan	1/21/2011	7/10/2014	10/2/2016
Hefei	1/25/2011	8/1/2014	10/2/2016
Changchun	1/26/2011	7/22/2014	–
Qingdao	1/26/2011	8/1/2014	–
Guiyang	2/11/2011	9/1/2014	–
Nanning	2/15/2011	9/30/2014	–
Harbin	2/18/2011	8/16/2014	–
Shijiazhuang	2/19/2011	4/26/2014	–
Yinchuan	2/22/2011	9/1/2014	–
Haikou	2/23/2011	7/23/2014	–
Shenyang	2/25/2011	9/12/2014	–
Xian	2/26/2011	8/28/2014	12/30/2016
Chongqing	2/28/2011	9/24/2014	–
Changsha	3/4/2011	8/6/2014	–
Urumqi	2/28/2011	10/23/2014	–
Hohhot	3/31/2011	6/26/2014	–
Xining	4/1/2011	9/5/2014	–

[48] Codes are available upon request.

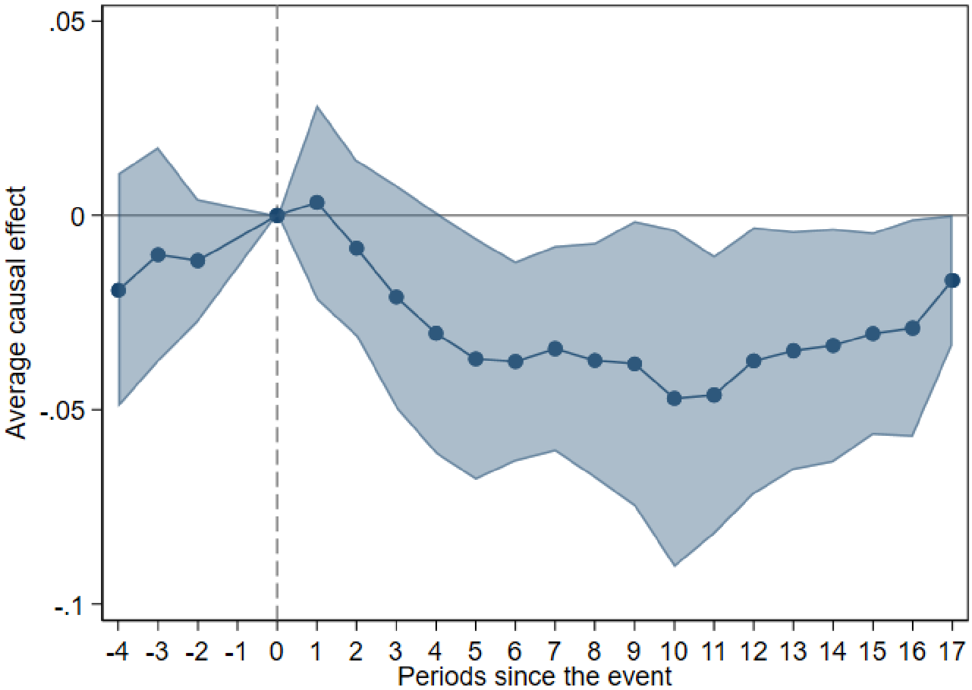


Figure A2.B.1: Parallel pre-trend test (Re-regulation)

A2.C Full regression results

Table A2.C.1: Predicting house price growth with housing sentiment

	Pooled OLS				
Dependent variable: Δlog(housing price)$_t$	(1)	(2)	(3)	(4)	(5)
Sum of lagged sentiment index	0.805***	0.805***	0.805***	0.776***	0.568***
	(0.137)	(0.149)	(0.120)	(0.177)	(0.083)
Δlog(sentiment)$_{t-1}$	0.069**	0.069***	0.069***	0.063***	0.033*
	(0.028)	(0.016)	(0.013)	(0.024)	(0.017)
Δlog(sentiment)$_{t-2}$	0.142***	0.142***	0.142***	0.151***	0.084***
	(0.033)	(0.024)	(0.024)	(0.035)	(0.018)
Δlog(sentiment)$_{t-3}$	0.139***	0.139***	0.139***	0.150***	0.091***
	(0.036)	(0.035)	(0.028)	(0.044)	(0.016)
Δlog(sentiment)$_{t-4}$	0.155***	0.155***	0.155***	0.130***	0.136***
	(0.034)	(0.049)	(0.040)	(0.045)	(0.032)
Δlog(sentiment)$_{t-5}$	0.206***	0.206***	0.206***	0.176***	0.141***
	(0.031)	(0.032)	(0.039)	(0.039)	(0.028)
Δlog(sentiment)$_{t-6}$	0.095***	0.095***	0.095***	0.105***	0.083***
	(0.025)	(0.023)	(0.021)	(0.028)	(0.015)
Δlog(housing price)$_{t-1}$	1.007***	1.007***	1.007***	0.616***	0.934***
	(0.028)	(0.067)	(0.135)	(0.047)	(0.111)
Δlog(housing price)$_{t-2}$	-0.394***	-0.394***	-0.394***	-0.293***	-0.359***
	(0.028)	(0.059)	(0.078)	(0.055)	(0.057)
Δreal mortgage rate$_{t-1}$	-0.007***	-0.007***	-0.007***	-0.004***	-0.007***
	(0.000)	(0.002)	(0.002)	(0.001)	(0.002)
Δlog(real income per capita)$_{t-1}$	0.182***	0.182***	0.182***	0.172***	0.158***
	(0.013)	(0.052)	(0.026)	(0.027)	(0.029)
Δlog(population)$_{t-1}$	0.203***	0.203**	0.203***	0.201***	0.178***
	(0.039)	(0.075)	(0.033)	(0.054)	(0.036)
Δunemployment rate$_{t-1}$	0.002	0.002	0.002	-0.000	0.002
	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
Δlog(policy uncertainty)$_{t-1}$	0.011***	0.011*	0.011*	0.008***	0.013***
	(0.002)	(0.006)	(0.006)	(0.002)	(0.005)
Δnational survey$_{t-1}$					0.001
					(0.000)
Δnational survey$_{t-2}$					-0.000
					(0.000)
Δnational survey$_{t-3}$					0.000
					(0.000)
Δnational survey$_{t-4}$					0.001***
					(0.000)
Δnational survey$_{t-5}$					0.000
					(0.000)
Δnational survey$_{t-6}$					-0.001***
					(0.000)
Constant	-0.000	-0.000	-0.000	0.001	0.001
	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)
SE: Driscoll-Kray	.	✓	.	.	.
SE: double clustered by (i,t)	.	.	✓	✓	✓
City FEs	✓	✓	✓	.	✓
City-Year FEs	.	.	.	✓	.
R^2	0.662	0.662	0.662	0.785	0.704
N	1156	1156	1156	1156	1156

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018).

Table A2.C.2: Is sentiment driven by unobserved fundamentals?

Dependent variable: Δlog(housing price)$_t$	Pooled OLS (1)	(2)	(3)	(4)	(5)
Sum of lagged sentiment	1.214***	1.215***	1.044***	1.107***	1.052***
	(0.172)	(0.230)	(0.249)	(0.326)	(0.308)
Δlog(sentiment)$_{t-1}$	0.052	0.029	0.030	0.063	0.127*
	(0.040)	(0.028)	(0.040)	(0.055)	(0.073)
Δlog(sentiment)$_{t-2}$	0.220***	0.224***	0.163***	0.179***	0.201***
	(0.054)	(0.073)	(0.038)	(0.043)	(0.037)
Δlog(sentiment)$_{t-3}$	0.208***	0.234***	0.180***	0.184***	0.164**
	(0.044)	(0.051)	(0.049)	(0.065)	(0.072)
Δlog(sentiment)$_{t-4}$	0.226***	0.211***	0.165*	0.178*	0.132
	(0.062)	(0.063)	(0.083)	(0.094)	(0.088)
Δlog(sentiment)$_{t-5}$	0.394***	0.390***	0.389***	0.394***	0.350***
	(0.058)	(0.064)	(0.086)	(0.092)	(0.083)
Δlog(sentiment)$_{t-6}$	0.114	0.127**	0.118**	0.110*	0.079
	(0.074)	(0.058)	(0.054)	(0.065)	(0.074)
Δlog(housing price)$_{t-1}$	1.035***	1.024***	1.017***	1.015***	1.004***
	(0.152)	(0.154)	(0.151)	(0.149)	(0.143)
Δlog(housing price)$_{t-2}$	-0.447***	-0.430***	-0.415***	-0.408***	-0.400***
	(0.088)	(0.089)	(0.089)	(0.087)	(0.082)
Δreal mortgage rate$_{t-1}$	-0.007**	-0.007**	-0.006***	-0.006***	-0.006***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Δlog(real income per capita)$_{t-1}$	0.140***	0.137***	0.146***	0.151***	0.142***
	(0.036)	(0.038)	(0.023)	(0.026)	(0.027)
Δlog(population)$_{t-1}$	0.283***	0.274***	0.309***	0.305***	0.295***
	(0.075)	(0.076)	(0.073)	(0.076)	(0.076)
Δunemployment rate$_{t-1}$	-0.001	-0.001	-0.006	-0.005	-0.005
	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)
Δlog(policy uncertainty)$_{t-1}$	0.012	0.011	0.009	0.008	0.007
	(0.011)	(0.011)	(0.008)	(0.008)	(0.008)
Δlog(all media fundamentals)$_{t-1}$	-0.030				
	(0.078)				
Δlog(all media fundamentals)$_{t-2}$	0.029				
	(0.047)				
Δlog(media rents)$_{t-1}$		0.028	0.026	0.030	0.030
		(0.030)	(0.022)	(0.027)	(0.026)
Δlog(media rents)$_{t-2}$		0.045	0.047**	0.051**	0.042*
		(0.031)	(0.020)	(0.022)	(0.023)
Δlog(media user costs)$_{t-1}$			0.050	0.052*	0.052*
			(0.032)	(0.030)	(0.028)
Δlog(media user costs)$_{t-2}$			0.047	0.042	0.036
			(0.039)	(0.033)	(0.031)
Δlog(media demographics)$_{t-1}$				-0.083	-0.103*
				(0.055)	(0.052)
Δlog(media demographics)$_{t-2}$				-0.039	-0.040
				(0.029)	(0.034)
Δlog(media credit conditions)$_{t-1}$					-0.015
					(0.028)
Δlog(media credit conditions)$_{t-2}$					0.001
					(0.023)
Constant	0.000	0.000	-0.000	-0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
SE: double clustered by (i,t)	√	√	√	√	√
City FEs	√	√	√	√	√
R^2	0.680	0.684	0.713	0.718	0.724
N	553	539	497	497	472

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) "Media fundamentals" is a social media index constructed using post that mention particular fundamentals (Media fundamentals include rents("zu" in Chinese), user costs such as property taxes and maintenance costs("shui" and "chengben" in Chinese), demographics such as local population and income("renkou", "shouru" and "gongzi" in Chinese), and credit conditions such as mortgage and interest rates("daikuan" and "lilv" in Chinese)) in its content, following Soo (2018).

Table A2.C.3: Is sentiment driven by policy changes?

Dependent variable: Δlog(housing price)$_t$	Difference-in-differences (1)	(2)	(3)
Δlog(sentiment)$_{t-1}$	0.002	0.002	0.002
	(0.052)	(0.033)	(0.063)
Δlog(sentiment)$_{t-2}$	0.148**	0.148***	0.148**
	(0.060)	(0.041)	(0.056)
Δlog(sentiment)$_{t-3}$	0.187***	0.187***	0.187***
	(0.067)	(0.045)	(0.056)
Δlog(sentiment)$_{t-4}$	0.155**	0.155**	0.155*
	(0.068)	(0.058)	(0.083)
Δlog(sentiment)$_{t-5}$	0.215***	0.215***	0.215***
	(0.064)	(0.055)	(0.054)
Δlog(sentiment)$_{t-6}$	0.073	0.073**	0.073*
	(0.052)	(0.033)	(0.042)
deregulation$\times$post$_t$	0.020***	0.020***	0.020
	(0.005)	(0.004)	(0.013)
reregulation$\times$post$_t$	0.006**	0.006***	0.006*
	(0.003)	(0.002)	(0.004)
Δhousing price$_{t-1}$	0.831***	0.831***	0.831***
	(0.045)	(0.073)	(0.142)
Δhousing price$_{t-2}$	-0.334***	-0.334***	-0.334***
	(0.043)	(0.034)	(0.114)
Δreal mortgage rate$_{t-1}$	-0.008***	-0.008*	-0.008***
	(0.002)	(0.004)	(0.002)
Δlog(income)$_{t-1}$	-0.023	-0.023	-0.023
	(0.041)	(0.023)	(0.043)
Δlog(population)$_{t-1}$	0.059	0.059	0.059*
	(0.053)	(0.040)	(0.034)
Δunemployment rate$_{t-1}$	-0.000	-0.000	-0.000
	(0.005)	(0.005)	(0.004)
Δlog(policy uncertainty)$_{t-1}$	0.014***	0.014***	0.014**
	(0.003)	(0.004)	(0.006)
Constant	-0.002	-0.027***	-0.002
	(0.005)	(0.005)	(0.004)
SE: Driscoll-Kray	.	√	.
SE: double clustered by (i,t)	.	.	√
City FEs	√	√	√
Quarter FEs	√	√	√
Year FEs	√	√	√
R^2	0.792	0.792	0.792
N	517	517	517

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018).

Table A2.C.4: Cross-sectional effect of sentiment: internet users and re-regulation

Dependent variable: $\Delta\log(\text{housing price})_t$	Pooled OLS (1)		(2)
Sum of lagged sentiment index	0.821***	Sum of lagged sentiment index	1.181***
	(0.247)		(0.343)
×% of internet users	3.764**	×re-regulation	-0.444
	(1.736)		(0.982)
$\Delta\log(\text{sentiment})\times\%$ of internet users$_{t-1}$	0.663*	$\Delta\log(\text{sentiment}) \times \text{reregulation}_{t-1}$	0.079
	(0.360)		(0.072)
$\Delta\log(\text{sentiment})\times\%$ of internet users$_{t-2}$	-0.791***	$\Delta\log(\text{sentiment})\times\text{reregulation}_{t-2}$	-0.139
	(0.219)		(0.188)
$\Delta\log(\text{sentiment})\times\%$ of internet users$_{t-3}$	-0.560	$\Delta\log(\text{sentiment})\times\text{reregulation}_{t-3}$	0.030
	(0.596)		(0.158)
$\Delta\log(\text{sentiment})\times\%$ of internet users$_{t-4}$	1.791***	$\Delta\log(\text{sentiment})\times\text{reregulation}_{t-4}$	0.084
	(0.547)		(0.134)
$\Delta\log(\text{sentiment})\times\%$ of internet users$_{t-5}$	0.899**	$\Delta\log(\text{sentiment})\times\text{reregulation}_{t-5}$	-0.046
	(0.413)		(0.189)
$\Delta\log(\text{sentiment})\times\%$ of internet users$_{t-6}$	0.037	$\Delta\log(\text{sentiment})\times\text{reregulation}_{t-6}$	-0.029
	(0.209)		(0.073)
Δ % of internet users	-0.019	reregulation	0.001
	(0.018)		(0.003)
$\Delta\log(\text{sentiment})_{t-1}$	0.035	$\Delta\log(\text{sentiment})_{t-1}$	-0.060
	(0.037)		(0.094)
$\Delta\log(\text{sentiment})_{t-2}$	0.087***	$\Delta\log(\text{sentiment})_{t-2}$	0.167**
	(0.027)		(0.064)
$\Delta\log(\text{sentiment})_{t-3}$	0.087*	$\Delta\log(\text{sentiment})_{t-3}$	0.131*
	(0.049)		(0.069)
$\Delta\log(\text{sentiment})_{t-4}$	0.062	$\Delta\log(\text{sentiment})_{t-4}$	0.129**
	(0.043)		(0.063)
$\Delta\log(\text{sentiment})_{t-5}$	0.123**	$\Delta\log(\text{sentiment})_{t-5}$	0.247***
	(0.050)		(0.061)
$\Delta\log(\text{sentiment})_{t-6}$	0.067***	$\Delta\log(\text{sentiment})_{t-6}$	0.088**
	(0.021)		(0.036)
$\Delta\log(\text{housing price})_{t-1}$	0.857***	$\Delta\log(\text{housing price})_{t-1}$	0.798***
	(0.106)		(0.131)
$\Delta\log(\text{housing price})_{t-2}$	-0.255***	$\Delta\log(\text{housing price})_{t-2}$	-0.291**
	(0.066)		(0.127)
$\Delta\text{real mortgage rate}_{t-1}$	-0.006**	$\Delta\text{real mortgage rate}_{t-1}$	-0.008***
	(0.002)		(0.002)
$\Delta\log(\text{real income per capita})_{t-1}$	-0.002	$\Delta\log(\text{real income per capita})_{t-1}$	0.003
	(0.017)		(0.032)
$\Delta\log(\text{population})_{t-1}$	0.062	$\Delta\log(\text{population})_{t-1}$	0.080**
	(0.042)		(0.038)
$\Delta\text{unemployment rate}_{t-1}$	0.003	$\Delta\text{unemployment rate}_{t-1}$	0.001
	(0.002)		(0.005)
$\Delta\log(\text{policy uncertainty})_{t-1}$	0.008	$\Delta\log(\text{policy uncertainty})_{t-1}$	0.013*
	(0.006)		(0.007)
Constant	-0.006	Constant	-0.009*
	(0.004)		(0.005)
SE: double clustered by (i,t)	√		√
Quarter FEs	√		√
Year FEs	√		√
R^2	0.735		0.781
N	1130		517

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018).

Table A2.C.5: Relaxation of symmetry: Predicting house price growth with positive, negative, male and female's sentiment

	Pooled OLS		
Dependent variable: Δlog(housing price)$_t$	(1)		(2)
Δlog(sentiment_pos)$_{t-1}$	-0.000	Δlog(sentiment_male)$_{t-1}$	0.074***
	(0.043)		(0.021)
Δlog(sentiment_pos)$_{t-2}$	0.120*	Δlog(sentiment_male)$_{t-2}$	0.189***
	(0.065)		(0.060)
Δlog(sentiment_pos)$_{t-3}$	0.149*	Δlog(sentiment_male)$_{t-3}$	0.205***
	(0.077)		(0.040)
Δlog(sentiment_pos)$_{t-4}$	0.155***	Δlog(sentiment_male)$_{t-4}$	0.191***
	(0.056)		(0.037)
Δlog(sentiment_pos)$_{t-5}$	0.221***	Δlog(sentiment_male)$_{t-5}$	0.206***
	(0.046)		(0.058)
Δlog(sentiment_pos)$_{t-6}$	0.068	Δlog(sentiment_male)$_{t-6}$	0.109***
	(0.045)		(0.030)
Δlog(sentiment_neg)$_{t-1}$	-0.125***	Δlog(sentiment_female)$_{t-1}$	-0.006
	(0.043)		(0.011)
Δlog(sentiment_neg)$_{t-2}$	-0.169***	Δlog(sentiment_female)$_{t-2}$	-0.013
	(0.019)		(0.018)
Δlog(sentiment_neg)$_{t-3}$	-0.140***	Δlog(sentiment_female)$_{t-3}$	-0.028
	(0.029)		(0.026)
Δlog(sentiment_neg)$_{t-4}$	-0.170***	Δlog(sentiment_female)$_{t-4}$	0.005
	(0.043)		(0.034)
Δlog(sentiment_neg)$_{t-5}$	-0.207***	Δlog(sentiment_female)$_{t-5}$	0.051***
	(0.065)		(0.018)
Δlog(sentiment_neg)$_{t-6}$	-0.123***	Δlog(sentiment_female)$_{t-6}$	0.025**
	(0.037)		(0.010)
Δlog(housing price)$_{t-1}$	1.008***	Δlog(housing price)$_{t-1}$	0.979***
	(0.135)		(0.132)
Δlog(housing price)$_{t-2}$	-0.396***	Δlog(housing price)$_{t-2}$	-0.387***
	(0.079)		(0.073)
Δreal mortgage rate$_{t-1}$	-0.007***	Δreal mortgage rate$_{t-1}$	-0.006***
	(0.002)		(0.002)
Δlog(real income per capita)$_{t-1}$	0.182***	Δlog(real income per capita)$_{t-1}$	0.175***
	(0.028)		(0.023)
Δlog(population)$_{t-1}$	0.204***	Δlog(population)$_{t-1}$	0.251***
	(0.041)		(0.042)
Δunemployment rate$_{t-1}$	0.002	Δunemployment rate$_{t-1}$	0.001
	(0.002)		(0.002)
Δlog(policy uncertainty)$_{t-1}$	0.011*	Δlog(policy uncertainty)$_{t-1}$	0.010
	(0.007)		(0.006)
Constant	-0.001	Constant	-0.000
	(0.001)		(0.001)
SE: double clustered by (i,t)	√		√
City FEs	√		√
R^2	0.664		0.670
N	1156		1040

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018).

Table A2.C.6: Predicting house price growth with sentiment (the number of likes, forwards and comments as weights)

Dependent variable: Δlog(housing price)$_t$	Pooled OLS weight=No. of likes	weight=No. of forwards	weight=No. of comments
Δlog(sentiment_weighted)$_{t-1}$	0.075***	0.075***	0.072***
	(0.011)	(0.014)	(0.012)
Δlog(sentiment_weighted)$_{t-2}$	0.138***	0.142***	0.138***
	(0.033)	(0.033)	(0.031)
Δlog(sentiment_weighted)$_{t-3}$	0.146***	0.145***	0.142***
	(0.034)	(0.033)	(0.030)
Δlog(sentiment_weighted)$_{t-4}$	0.164***	0.158***	0.158***
	(0.042)	(0.039)	(0.039)
Δlog(sentiment_weighted)$_{t-5}$	0.206***	0.194***	0.196***
	(0.044)	(0.038)	(0.039)
Δlog(sentiment_weighted)$_{t-6}$	0.098***	0.096***	0.090***
	(0.022)	(0.019)	(0.019)
Δlog(housing price)$_{t-1}$	0.996***	0.994***	0.995***
	(0.134)	(0.133)	(0.134)
Δlog(housing price)$_{t-2}$	-0.393***	-0.391***	-0.391***
	(0.078)	(0.077)	(0.078)
Δreal mortgage rate$_{t-1}$	-0.007***	-0.007***	-0.007***
	(0.002)	(0.002)	(0.002)
Δlog(real income per capita)$_{t-1}$	0.182***	0.182***	0.182***
	(0.025)	(0.025)	(0.025)
Δlog(population)$_{t-1}$	0.201***	0.202***	0.202***
	(0.034)	(0.033)	(0.033)
Δunemployment rate$_{t-1}$	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)
Δlog(policy uncertainty)$_{t-1}$	0.011*	0.011*	0.011*
	(0.006)	(0.006)	(0.006)
Constant	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)
SE: double clustered by (i,t)	√	√	√
City FEs	√	√	√
R^2	0.664	0.664	0.664
N	1156	1156	1156

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018).

Table A2.C.7: Dispersion of sentiment and housing sales

Dependent variable:	$\log(\text{selling quantity})_t$ (1)	(2)	$\log(\text{selling area})_t$ (3)	(4)
Sum of lagged daily average std dev of sentiment	-1.007**	-1.002**	-1.151**	-1.135**
	(0.465)	(0.469)	(0.486)	(0.501)
$\Delta\log(\text{sentiment\_dailystd})_{t-1}$	-0.121	-0.119	-0.172	-0.165
	(0.216)	(0.217)	(0.181)	(0.183)
$\Delta\log(\text{sentiment\_dailystd})_{t-2}$	-0.242*	-0.242*	-0.284*	-0.285*
	(0.136)	(0.138)	(0.158)	(0.164)
$\Delta\log(\text{sentiment\_dailystd})_{t-3}$	-0.418***	-0.416***	-0.449***	-0.443***
	(0.104)	(0.106)	(0.134)	(0.140)
$\Delta\log(\text{sentiment\_dailystd})_{t-4}$	-0.226***	-0.225***	-0.246***	-0.242***
	(0.071)	(0.071)	(0.078)	(0.080)
$\log(\text{sellings})_{t-1}$	0.072	0.072	0.057	0.059
	(0.112)	(0.111)	(0.112)	(0.112)
$\log(\text{sellings})_{t-2}$	-0.151**	-0.151**	-0.203***	-0.205***
	(0.070)	(0.070)	(0.064)	(0.064)
$\Delta\text{real mortgage rate}_{t-1}$	-0.019	-0.018	-0.022**	-0.020*
	(0.012)	(0.012)	(0.011)	(0.011)
$\Delta\log(\text{real income per capita})_{t-1}$	0.130	0.142	-0.030	0.012
	(0.470)	(0.468)	(0.455)	(0.454)
$\Delta\log(\text{population})_{t-1}$	0.196	0.181	0.605	0.556
	(0.786)	(0.783)	(0.913)	(0.923)
$\Delta\text{unemployment rate}_{t-1}$	0.177*	0.178*	0.179*	0.183*
	(0.092)	(0.092)	(0.094)	(0.095)
$\Delta\log(\text{policy uncertainty})_{t-1}$	-0.025		-0.068	
	(0.061)		(0.061)	
Constant	10.280***	10.275***	16.237***	16.232***
	(1.356)	(1.354)	(1.933)	(1.933)
SE: double clustered by (it)	√	√	√	√
City-Year FEs	√	√	√	√
Adjusted R^2	0.799	0.799	0.807	0.807
Observations	755	755	731	731

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018).

A2.D Additional Robustness Checks

Table A2.D.1: Predicting house price growth with housing sentiment: by region and using Weighted Least Squares

Dependent variable: Δlog(housing price)$_t$	Pooled OLS East	Pooled OLS Middle	Pooled OLS West	Weighted Least Square 35 cities
Sum of lagged sentiment index	1.042***	1.090***	0.379**	0.755***
	(0.146)	(0.331)	(0.158)	(0.165)
Δlog(sentiment)$_{t-1}$	0.087*	0.020	0.055***	0.057*
	(0.045)	(0.034)	(0.017)	(0.034)
Δlog(sentiment)$_{t-2}$	0.187***	0.136**	0.085*	0.145***
	(0.055)	(0.058)	(0.043)	(0.040)
Δlog(sentiment)$_{t-3}$	0.153*	0.282***	0.037	0.111***
	(0.077)	(0.087)	(0.039)	(0.043)
Δlog(sentiment)$_{t-4}$	0.220***	0.250**	0.046	0.132***
	(0.043)	(0.116)	(0.039)	(0.042)
Δlog(sentiment)$_{t-5}$	0.265***	0.279***	0.107***	0.207***
	(0.064)	(0.063)	(0.038)	(0.039)
Δlog(sentiment)$_{t-6}$	0.129*	0.124***	0.049*	0.103***
	(0.069)	(0.043)	(0.027)	(0.033)
Δlog(housing price)$_{t-1}$	1.034***	0.988***	0.952***	1.089***
	(0.167)	(0.146)	(0.104)	(0.027)
Δlog(housing price)$_{t-2}$	-0.432***	-0.428***	-0.237***	-0.432***
	(0.103)	(0.118)	(0.067)	(0.027)
Δreal mortgage rate$_{t-1}$	-0.007**	-0.006***	-0.006***	-0.008***
	(0.003)	(0.002)	(0.001)	(0.000)
Δlog(real income per capita)$_{t-1}$	0.149***	0.251***	0.189***	0.178***
	(0.039)	(0.016)	(0.024)	(0.013)
Δlog(population)$_{t-1}$	0.266***	0.354***	0.132***	0.185***
	(0.061)	(0.064)	(0.042)	(0.041)
Δunemployment rate$_{t-1}$	-0.000	0.004	0.002	-0.004
	(0.006)	(0.003)	(0.003)	(0.003)
Δlog(policy uncertainty)$_{t-1}$	0.013	0.014*	0.007	0.010***
	(0.010)	(0.008)	(0.004)	(0.002)
Constant	-0.000	-0.003*	-0.002	-0.002***
	(0.002)	(0.002)	(0.001)	(0.000)
SE: double clustered by (i,t)	√	√	√	
City FEs	√	√	√	
R^2	0.677	0.662	0.675	0.700
N	517	272	367	1130

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index and sentiment index are at quarterly frequency. House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018).

Table A2.D.2: Predicting house price growth with housing sentiment

Dependent variable: Δlog(housing price)$_t$	Pooled OLS (1)	(2)	(3)	(4)	(5)	(6)
Δlog(sentiment)$_{t-1}$	0.043**	0.063***	0.057***	0.063**	0.488***	0.081***
	(0.019)	(0.020)	(0.021)	(0.027)	(0.057)	(0.025)
Δlog(sentiment)$_{t-2}$	0.093***	0.141***	0.140***	0.140***	-0.327***	0.127***
	(0.026)	(0.029)	(0.032)	(0.039)	(0.056)	(0.032)
Δlog(sentiment)$_{t-3}$	0.090***	0.143***	0.130***	0.137***	0.093***	0.128***
	(0.028)	(0.031)	(0.042)	(0.051)	(0.031)	(0.031)
Δlog(sentiment)$_{t-4}$	0.073**	0.123***	0.109**	0.099**	0.165***	0.087**
	(0.029)	(0.031)	(0.043)	(0.049)	(0.046)	(0.038)
Δlog(sentiment)$_{t-5}$	0.101***	0.134***	0.141***	0.111***	0.135***	0.112***
	(0.024)	(0.029)	(0.037)	(0.042)	(0.048)	(0.032)
Δlog(sentiment)$_{t-6}$	0.069***	0.081***	0.076***	0.061*	0.169***	0.060***
	(0.020)	(0.020)	(0.024)	(0.032)	(0.050)	(0.021)
Δlog(housing price)$_{t-1}$	0.618***	0.611***	0.616***	0.623***	0.236***	0.585***
	(0.046)	(0.048)	(0.047)	(0.047)	(0.049)	(0.050)
Δlog(housing price)$_{t-2}$	-0.294***	-0.296***	-0.296***	-0.301***	0.124***	-0.290***
	(0.056)	(0.056)	(0.056)	(0.056)	(0.037)	(0.055)
Δreal mortgage rate$_{t-1}$	-0.004***	-0.004***	-0.004***	-0.004***	-0.005***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Δlog(real income per capita)$_{t-1}$	0.173***	0.165***	0.172***	0.173***	-0.031	0.163***
	(0.027)	(0.030)	(0.027)	(0.029)	(0.024)	(0.025)
Δlog(population)$_{t-1}$	0.203***	0.236***	0.201***	0.201***	-0.034	0.203***
	(0.053)	(0.062)	(0.054)	(0.053)	(0.131)	(0.049)
Δunemployment rate$_{t-1}$	0.000	-0.001	-0.000	-0.001	0.005	-0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.009)	(0.003)
Δlog(policy uncertainty)$_{t-1}$	0.008***	0.008***	0.008***	0.008***	0.007***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	0.001	0.001*	0.001	0.002**	0.007***	0.001*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
SE: double clustered by (i,t)	√	√	√	√	√	√
City-Year FEs	√	√	√	√	√	√
R^2	0.783	0.786	0.784	0.782	0.671	0.708
Observations	1087	1001	1087	1039	1151	1095

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All control variables are at annual frequency, except for policy uncertainty and mortgage rate (quarterly). House price index, mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). (4)The sentiment indices in the first column are constructed using the median value of each quarter. The sentiment indices in the second column are calculated from posts written by local people, according to self-disclosed location information. The sentiment indices in the third column include degree adverbs when calculating. The sentiment indices in the fourth column are orthogonalized to all macro-control variables in the regression. The fifth column transforms the annual fundamentals into quarterly using linear interpolation. The sentiment indices in the last column are calculated using a new Chinese Financial Dictionary constructed by Du et al. (2022).

Table A2.D.3: Predicting monthly house price growth with housing sentiment

Dependent variable: Δlog(housing price)$_t$	(1)	(2)	(3)	(4)	(5)
Sum of lagged sentiment index	0.287***	0.245***	0.230***	0.250***	0.341**
	(0.113)	(0.095)	(0.081)	(0.091)	(162)
Δlog(sentiment)$_{t-1}$	0.009	0.009	0.008	0.008	0.003
	(0.007)	(0.006)	(0.006)	(0.005)	(0.007)
Δlog(sentiment)$_{t-2}$	0.025**	0.024**	0.021**	0.023**	0.014
	(0.012)	(0.010)	(0.009)	(0.009)	(0.008)
Δlog(sentiment)$_{t-3}$	0.027**	0.024**	0.022**	0.024**	0.019**
	(0.013)	(0.012)	(0.010)	(0.011)	(0.010)
Δlog(sentiment)$_{t-4}$	0.032*	0.028*	0.025*	0.027*	0.030**
	(0.018)	(0.016)	(0.014)	(0.014)	(0.015)
Δlog(sentiment)$_{t-5}$	0.025	0.021	0.018	0.020	0.030
	(0.019)	(0.017)	(0.014)	(0.015)	(0.021)
Δlog(sentiment)$_{t-6}$	0.029*	0.025	0.024*	0.025*	0.038*
	(0.018)	(0.016)	(0.014)	(0.015)	(0.021)
Δlog(sentiment)$_{t-7}$	0.031**	0.027**	0.027***	0.027**	0.042**
	(0.013)	(0.011)	(0.010)	(0.011)	(0.019)
Δlog(sentiment)$_{t-8}$	0.023***	0.018***	0.018***	0.020***	0.033*
	(0.008)	(0.007)	(0.006)	(0.007)	(0.018)
Δlog(sentiment)$_{t-9}$	0.020*	0.016*	0.017**	0.018*	0.027
	(0.011)	(0.010)	(0.009)	(0.010)	(0.019)
Δlog(sentiment)$_{t-10}$	0.021	0.016	0.015	0.018	0.027
	(0.014)	(0.012)	(0.011)	(0.012)	(0.021)
Δlog(sentiment)$_{t-11}$	0.015	0.012	0.011	0.013	0.028
	(0.012)	(0.011)	(0.010)	(0.011)	(0.019)
Δlog(sentiment)$_{t-12}$	0.016	0.013	0.012	0.014	0.032*
	(0.013)	(0.012)	(0.011)	(0.011)	(0.017)
Δlog(sentiment)$_{t-13}$	0.013*	0.012*	0.011*	0.012*	0.019**
	(0.008)	(0.007)	(0.007)	(0.007)	(0.009)
Δlog(housing price)$_{t-1}$	0.608***	0.609***	0.609***	0.609***	0.619***
	(0.106)	(0.106)	(0.105)	(0.106)	(0.103)
Δreal mortgage rate$_{t-1}$	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Δlog(real income per capita)$_{t-1}$	-0.038	-0.038	-0.039	-0.039	-0.040
	(0.033)	(0.033)	(0.033)	(0.033)	(0.032)
Δlog(population)$_{t-1}$	-0.013	-0.013	-0.013	-0.013	-0.021
	(0.039)	(0.039)	(0.039)	(0.039)	(0.034)
Δunemployment rate$_{t-1}$	-0.004*	-0.004*	-0.004*	-0.004*	-0.005**
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Δlog(policy uncertainty)$_{t-1}$	-0.002	-0.003	-0.002	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.001	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
SE: double clustered by (i,t)	√	√	√	√	√
City FEs	√	√	√	√	√
R^2	0.376	0.376	0.376	0.376	0.387
N	3504	3504	3504	3504	3367

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) Sentiment indices, house price index, mortgage rate, and policy uncertainty are at monthly frequency. All other variables are at annual frequency. (4) The sentiment indices in the first column are constructed using the mean value of each quarter. The sentiment indices in the second, third, and forth column are constructed by weighing posts with the number of likes, reposts, and comments, respectively. The sentiment indices in the last column are orthogonalized to all macro-control variables in the regression.

Table A2.D.4: Is monthly sentiment driven by policy changes?

	TWFE Difference-in-differences		
Dependent variable: Δlog(housing price)$_t$	(1)	(2)	(3)
Sum of lagged sentiment index	0.492***	0.492**	0.492***
	(0.137)	(0.218)	(0.131)
Δlog(sentiment)$_{t-1}$	0.012	0.012	0.012
	(0.011)	(0.010)	(0.011)
Δlog(sentiment)$_{t-2}$	0.044***	0.044***	0.044*
	(0.014)	(0.017)	(0.023)
Δlog(sentiment)$_{t-3}$	0.053***	0.053***	0.053**
	(0.016)	(0.020)	(0.024)
Δlog(sentiment)$_{t-4}$	0.065***	0.065***	0.065***
	(0.017)	(0.020)	(0.021)
Δlog(sentiment)$_{t-5}$	0.058***	0.058**	0.058***
	(0.017)	(0.023)	(0.019)
Δlog(sentiment)$_{t-6}$	0.055***	0.055*	0.055***
	(0.017)	(0.029)	(0.007)
Δlog(sentiment)$_{t-7}$	0.057***	0.057**	0.057***
	(0.017)	(0.028)	(0.018)
Δlog(sentiment)$_{t-8}$	0.051***	0.051**	0.051***
	(0.017)	(0.023)	(0.009)
Δlog(sentiment)$_{t-9}$	0.046***	0.046**	0.046***
	(0.017)	(0.020)	(0.017)
Δlog(sentiment)$_{t-10}$	0.019	0.019	0.019
	(0.016)	(0.024)	(0.023)
Δlog(sentiment)$_{t-11}$	0.003	0.003	0.003
	(0.015)	(0.028)	(0.022)
Δlog(sentiment)$_{t-12}$	0.014	0.014	0.014
	(0.013)	(0.021)	(0.014)
Δlog(sentiment)$_{t-13}$	0.014	0.014	0.014
	(0.010)	(0.013)	(0.014)
deregulation*post$_t$	0.004***	0.004**	0.004*
	(0.001)	(0.002)	(0.002)
reregulation*post$_t$	-0.006***	-0.006**	-0.006**
	(0.001)	(0.002)	(0.002)
Δhousing price$_{t-1}$	0.549***	0.549***	0.549***
	(0.023)	(0.099)	(0.132)
Δreal mortgage rate$_{t-1}$	-0.001***	-0.001**	-0.001***
	(0.000)	(0.001)	(0.000)
Δlog(real income per capita)$_{t-1}$	-0.068***	-0.068	-0.068***
	(0.010)	(0.049)	(0.021)
Δlog(population)$_{t-1}$	-0.040*	-0.040	-0.040***
	(0.024)	(0.029)	(0.012)
Δunemployment rate$_{t-1}$	-0.005*	-0.005	-0.005**
	(0.002)	(0.004)	(0.002)
Δlog(policy uncertainty)$_{t-1}$	-0.001***	-0.001	-0.001
	(0.001)	(0.001)	(0.003)
Constant	-0.000	-0.000	-0.000
	(0.001)	(0.002)	(0.001)
SE: Driscoll-Kray	.	√	.
SE: double clustered by (i,t)	.	.	√
City FEs	√	√	√
Quarter FEs	√	√	√
Year FEs	√	√	√
R^2	0.560	0.560	0.560
N	1625	1625	1625

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) Sentiment indices, house price index, mortgage rate, and policy uncertainty are at monthly frequency. All other variables are at annual frequency.

A2.E Reverse Causality

Table A2.E.1: Predicting housing sentiment with past returns

Dependent variable: log(sentiment)$_t$	Pooled OLS			
	6 months	1 year	2 years	3 years
Cumulative price appreciation	0.024	0.002	0.000	-0.001
	(0.021)	(0.013)	(0.012)	(0.015)
log(rent/income)	0.456	0.009	-0.985	-1.220
	(0.630)	(0.791)	(1.053)	(1.222)
log(rent/price)	-0.900*	-0.585	-0.801	-1.043
	(0.528)	(0.553)	(0.862)	(1.021)
real mortgage rate	0.046	0.030	0.117	0.118
	(0.114)	(0.113)	(0.121)	(0.122)
log(real income per capita)	2.741**	1.606	2.572***	6.362***
	(1.149)	(1.000)	(0.996)	(2.271)
log(population)	-1.731	-2.418	-1.082	0.256
	(1.606)	(1.509)	(2.083)	(2.637)
unemployment rate	0.388**	0.521***	0.637***	0.705***
	(0.189)	(0.199)	(0.214)	(0.247)
log(policy uncertainty)	0.604*	0.851***	0.662**	0.101
	(0.313)	(0.285)	(0.288)	(0.367)
Constant	-16.365	-1.582	-21.991	-69.931***
	(19.374)	(16.885)	(19.965)	(23.227)
SE: double clustered by (i,t)	√	√	√	√
City FEs	√	√	√	√
R^2	0.177	0.166	0.192	0.223
N	1141	1072	933	793

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses.

Table A2.E.2: Predicting house price growth with sentiment

Dependent variable: Δlog(housing price)$_t$	Pooled OLS		
	(1)	(2)	(3)
Δlog(sentiment)$_{t-4}$	0.048*	0.048*	0.048*
	(0.025)	(0.026)	(0.028)
Δlog(sentiment)$_{t-5}$	0.137***	0.137***	0.137***
	(0.028)	(0.015)	(0.022)
Δlog(sentiment)$_{t-6}$	0.070***	0.070***	0.070***
	(0.024)	(0.014)	(0.018)
Δlog(housing price)$_{t-1}$	1.008***	1.008***	1.008***
	(0.028)	(0.068)	(0.123)
Δlog(housing price)$_{t-2}$	-0.386***	-0.386***	-0.386***
	(0.028)	(0.057)	(0.073)
Δreal mortgage rate$_{t-1}$	-0.006***	-0.006***	-0.006***
	(0.000)	(0.002)	(0.002)
Δlog(real income per capita)$_{t-1}$	0.172***	0.172***	0.172***
	(0.013)	(0.051)	(0.026)
Δlog(population)$_{t-1}$	0.229***	0.229***	0.229***
	(0.040)	(0.081)	(0.031)
Δunemployment rate$_{t-1}$	0.003	0.003	0.003**
	(0.003)	(0.003)	(0.001)
Δlog(policy uncertainty)$_{t-1}$	0.012***	0.012**	0.012*
	(0.002)	(0.006)	(0.006)
Constant	-0.000	-0.000	-0.000
	(0.003)	(0.002)	(0.001)
SE: Driscoll-Kray	.	√	.
SE: double clustered by (i,t)	.	.	√
City FEs	√	√	√
R^2	0.640	0.640	0.640
N	1169	1169	1169

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses.

TABLE A2.E.3: Will the US-China trade war drive up sentiment?

	TWFE Difference-in-differences		
Dependent variable: Δlog(housing price)$_t$	(1)	(2)	(3)
Sum of lagged sentiment index	0.294**	0.294***	0.294***
	(0.118)	(0.099)	(0.076)
Trade war	-0.006***	-0.006**	-0.006*
	(0.002)	(0.002)	(0.003)
Lagged price changes	√	√	√
Lagged fundamentals	√	√	√
SE: by (i)	.	√	.
SE: double clustered by (i,t)	.	.	√
City FEs	√	√	√
Quarter FEs	√	√	√
Year FEs	√	√	√
R^2	0.792	0.792	0.792
N	1156	1156	1156

Note: (1)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2)Standard errors in parentheses. (3)All control variables are at annual frequency, except for policy uncertainty and mortgage rate(quarterly). House price index and sentiment index are in quarterly frequency. House price index, mortgage rate and income are real value which have been adjusted by CPI. Sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized(s+100). Sum of lagged sentiment index is the sum of coefficients of 6 quarters of sentiment. (4)The first row reports the accumulated impact of a lagged one and half year of sentiment growth on future quarterly price growth. (5)$Tradewar$ equals to one if the city is in the treatment group and is after the first quarter of 2018. There are 9 cities(Xiamen, Shenzhen, Ningbo, Shanghai, Hangzhou, Qingdao, Guangzhou, Zhengzhou and Dalian) in the treatment group. They are 9 of the top 10 cities with highest foreign trade dependence in 2017.

A2.F Additional Results

Table A2.F.1: Predicting housing sales with housing sentiment

	Pooled OLS			
Dependent variable: log(housing sales)$_t$	(1)	(2)	(3)	(4)
Sum of lagged sentiment index	16.569***	16.569***	16.569***	16.569***
	(5.008)	(2.846)	(7.310)	(15.685)
Δlog(sentiment)$_{t-1}$	3.057***	3.057***	3.057*	2.122
	(1.045)	(0.946)	(1.667)	(1.322)
Δlog(sentiment)$_{t-2}$	3.353***	3.353***	3.353	2.412
	(1.230)	(1.038)	(2.092)	(1.713)
Δlog(sentiment)$_{t-3}$	2.528*	2.528**	2.528	2.422
	(1.306)	(0.974)	(1.866)	(1.922)
Δlog(sentiment)$_{t-4}$	1.543	1.543**	1.543	2.412*
	(1.293)	(0.662)	(1.027)	(1.302)
Δlog(sentiment)$_{t-5}$	3.777***	3.777***	3.777***	3.579***
	(1.156)	(1.206)	(1.114)	(1.230)
Δlog(sentiment)$_{t-6}$	2.312**	2.312***	2.312***	2.739***
	(0.947)	(0.508)	(0.535)	(0.835)
log(housing sales)$_{t-1}$	0.460***	0.460***	0.460***	0.404***
	(0.046)	(0.086)	(0.128)	(0.133)
log(housing sales)$_{t-2}$	0.097**	0.097**	0.097	0.163**
	(0.046)	(0.045)	(0.061)	(0.073)
Δreal mortgage rate$_{t-1}$	0.006	0.006	0.006	0.015
	(0.015)	(0.013)	(0.066)	(0.061)
Δlog(real income per capita)$_{t-1}$	0.898**	0.898	0.898	0.344
	(0.457)	(0.762)	(1.240)	(0.955)
Δlog(population)$_{t-1}$	2.210	2.210***	2.210**	2.244***
	(1.490)	(0.765)	(0.867)	(0.670)
Δunemployment rate$_{t-1}$	0.219*	0.219*	0.219**	0.198**
	(0.128)	(0.128)	(0.087)	(0.092)
Δlog(policy uncertainty)$_{t-1}$	-0.058	-0.058	-0.058	0.105
	(0.065)	(0.114)	(0.068)	(0.109)
Δnational survey$_{t-1}$				0.015*
				(0.008)
Δnational survey$_{t-2}$				-0.011**
				(0.005)
Δnational survey$_{t-3}$				-0.007
				(0.006)
Δnational survey$_{t-4}$				-0.008*
				(0.005)
Δnational survey$_{t-5}$				-0.000
				(0.006)
Δnational survey$_{t-6}$				-0.006
				(0.004)
Constant	4.208***	4.208***	4.208***	4.120***
	(0.410)	(0.586)	(0.933)	(0.726)
SE: Driscoll-Kray	.	✓	.	.
SE: double clustered by (i,t)	.	.	✓	✓
City FEs	✓	✓	✓	✓
R^2	0.770	0.770	0.770	0.782
N	738	738	738	738

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All variables are at quarterly frequency, except for real income per capita, unemployment rate, and population (annual). Mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). Housing sales represent the total selling area each quarter.

Table A2.F.2: Is sentiment driven by policy changes?

Dependent variable:log(housing sales)$_t$	TWFE Difference-in-differences (1)	(2)	(3)
Sum of lagged sentiment index	18.001***	18.001***	18.001***
	(5.851)	(4.153)	(2.267)
deregulation×post$_t$	0.419***	0.419***	0.419***
	(0.103)	(0.041)	(0.054)
reregulation×post$_t$	-0.054	-0.054	-0.054
	(0.060)	(0.092)	(0.091)
Δlog(sentiment)$_{t-1}$	1.370	1.370	1.370
	(1.181)	(1.150)	(1.237)
Δlog(sentiment)$_{t-2}$	2.901**	2.901*	2.901***
	(1.360)	(1.506)	(0.944)
Δlog(sentiment)$_{t-3}$	4.646***	4.646***	4.646***
	(1.509)	(1.316)	(0.642)
Δlog(sentiment)$_{t-4}$	5.832***	5.832***	5.832***
	(1.544)	(0.538)	(1.205)
Δlog(sentiment)$_{t-5}$	2.517*	2.517***	2.517**
	(1.471)	(0.599)	(1.076)
Δlog(sentiment)$_{t-6}$	0.734	0.734	0.734
	(1.185)	(0.551)	(1.347)
log(housing sales)$_{t-1}$	0.546***	0.546***	0.546***
	(0.054)	(0.042)	(0.142)
log(housing sales)$_{t-2}$	0.150***	0.150***	0.150**
	(0.052)	(0.037)	(0.065)
Δreal mortgage rate$_{t-1}$	-0.071*	-0.071**	-0.071
	(0.039)	(0.032)	(0.068)
Δlog(real income per capita)$_{t-1}$	0.225	0.225	0.225
	(0.968)	(0.419)	(0.281)
Δlog(population)$_{t-1}$	1.961	1.961**	1.961***
	(1.251)	(0.962)	(0.507)
Δunemployment rate$_{t-1}$	0.105	0.105	0.105
	(0.137)	(0.095)	(0.147)
Δlog(policy uncertainty)$_{t-1}$	0.177***	0.177***	0.177*
	(0.067)	(0.038)	(0.091)
Constant	2.491***	0.000	2.491**
	(0.462)	(.)	(1.081)
SE: Driscoll-Kray	.	√	.
SE: double clustered by (i,t)	.	.	√
City FEs	√	√	√
Quarter FEs	√	√	√
Year FEs	√	√	√
R^2	0.870	0.870	0.870
N	411	411	411

Notes: (1) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (2) Standard errors in parentheses. (3) All variables are at quarterly frequency, except for real income per capita, unemployment rate, and population (annual). Mortgage rate and income are expressed in real terms, deflated by CPI. The sentiment index is calculated using the Chinese Financial Sentiment Dictionary (CFSD) and has been normalized as (s+100) following Soo (2018). Housing sales represent the total selling area each quarter.

Table A2.F.3: Gender distribution

	Male			Female			Difference	
	Obs	Mean	Std dev	Obs	Mean	Std dev	b	t test
Sentiment Index	232,381	0.0088	0.0754	88,584	0.0075	0.0751	-0.0007*	(-2.51)
Positive Sentiment Index	139,634	0.0577	0.0431	54,511	0.0565	0.0425	-0.0012***	(-5.59)
Negative Sentiment Index	92,747	-0.0650	0.0491	34,073	-0.0657	0.0508	-0.0007*	(-2.35)

Notes: This tables shows the number of posts and mean sentiment by gender. It also tests the difference between mean sentiment by gender.

CHAPTER 3 Rezoning and house prices: Evidence from Beijing, China

Abstract

We study the impact of the merger of suburbs into urban districts on property prices, using evidence from Beijing. We use a Difference-in-Differences method with an event study setting and complex-level panel data to identify the impact of the merger on house prices in both newly merged and old neighboring districts. Our results show that the merger significantly increased house prices in the rezoned areas, while the opposite happened in the non-rezoned border districts. In both cases, the impact is localized. Additionally, we see the merger hurts poor people, as the decline in house prices is larger for low-priced properties in non-rezoned border districts, while the increase in house prices is smaller for low-priced properties in rezoned areas. Further analysis shows that the merger had a positive spillover effect in surrounding counties, with the effect decreasing as the distance to the rezoned districts increased. Our findings are important in understanding the impact of place-based policies on housing and their potential spillovers.

3.1 Introduction

"Housing is at the root of many of the rich world's problems".[1] Although a high house price is associated with a high nontradable sector and total output, there is no doubt that a dramatic housing boom will result in a bubble and can hurt the economy in the long run. Rising property prices can discourage productive lending, reduce investment and productivity, and lead to capital misallocation (Hau et al., 2018; Adelino et al., 2018). While there is a considerable amount of research focused on developed countries, more attention should be given to developing countries. The "undersized" Chinese cities and inefficient counties are causing significant efficiency and welfare losses (Au & Henderson, 2006a). The merger of cities and counties, transforming the latter into urban districts, yields numerous benefits for local governments. These include expanding the development space of central cities, fostering population agglomeration, accelerating the urbanization of industries, and stimulating the development of the real estate market (Fan et al., 2012).

Using the case of the merger (also referred to as rezoning in this paper) in Beijing, China, this study documents the economically significant reduction in house prices in old urban districts that are adjacent to newly merged districts (also referred to as old border districts), as well as a significant appreciation in house prices in newly merged districts (also referred to as new districts). The study also conducts a comprehensive examination of potential heterogeneity in the impact of the merger and finds that the merger has a stronger impact on residential complexes located close to the border of new districts. Additionally, the study finds that the merger has a stronger impact on relatively inexpensive houses in old neighboring districts and more expensive houses in new districts.

We study the merger in China as it presents three useful features for our purposes. Firstly, China possesses an unparalleled housing market in comparison to other developing nations. The housing market boom has continued over two decades in China, bringing about concerns of housing affordability and regional economic development, making it an ideal case study. Secondly, the abundance of intricate data available in China is immensely more than that found in other developing countries. Thirdly, Beijing, the capital city of China, is the epicenter of

[1] This is the title of a special report of the Economists, 2020. Real estate is the largest asset class in the world. In 2020, it made up around 68% of the world's non-financial assets, which includes plant and machinery as well as intangibles, such as intellectual property. According to Savills, global real estate value hits $326.5 trillion USD in 2020, nearly four times world GDP (85.11 trillion USD in 2020).

this merger and holds significant political significance, thus warranting an investigation from a political perspective.

Our hypothesis is informed by peer works: Deng et al. (2022) investigate the impact of out-of-town housing demand on house prices and consumer spending. They find that the imposition of purchase restrictions on local housing can lead to capital flight and significant abnormal increases in house prices in nearby unregulated cities. In addition, the merger turning local counties to Beijing urban districts affects the governmental services afforded to the area. Preferences for local government services play a large role in local house prices (e.g. Schönholzer (2023); Black (1999)). Inspired by their work, we believe the merger, which entails transforming neighboring counties into city districts, creates an external shock that significantly impacts the local and neighboring housing market. This external shock gives rise to two distinct effects, forming the basis of our hypotheses.

Hypothesis 1: The merger will lead to a decrease in house prices in old neighboring districts. The increased investor confidence in the economic prospects, expectation for better local amenities, and the fact of low house prices of the transformed district will trigger a shift in purchases from the old neighboring districts to the new districts. As a result, we should observe negative spillover effects on the old neighboring areas, leading to a decline in house prices.

Hypothesis 2: The merger will increase house prices in the new districts. First, there will be a surge in housing demand due to the shift in purchasing from the old neighboring districts, as the buyers will expect better amenities provided by the local government, and higher price in the future. However, the merger can also decrease housing demand in the new districts. Home purchase restrictions will prohibit out-of-town homebuyers from purchasing in the new districts after their integration into Beijing. However, we believe the increase in demand will be larger than the decrease. The short-term inelasticity of housing supply further warrants investigation into the magnitude of the decrease and increase in demand within the new districts.

Our results highlight the effects of municipal amalgamations on both the local and external housing markets. Little is known about the impact of the city-county merger policy on house prices in China. Instead, much research focuses on the effects of municipal amalgamations on the efficiency of local public goods provision (Reingewertz, 2012), and economic development (Jia et al., 2021). Despite the fact that the topic has received little attention in the literature, it is crucial to understand the effects of the merger on both the local and neighboring housing markets in order to inform policy decisions and evaluate the net benefits of potential mergers.

To identify the effect of the merger on house prices, we employ a Difference-in-differences identification with an event-study setting that is consistent with previous studies when examining causal impacts in zoning with geographic variation (Lu et al., 2019; Zheng et al., 2017; Chen et al., 2022). One of the key strengths of this study is that we employ recent residential complex-level data for the period from September 2013 to October 2017 in Beijing.[2]

The main empirical challenge is that the house market may be systematically different in both new and old districts. The merger is not randomly selected—a dimension of the site selection bias. For example, new districts tend to be located far from the central business area where farmland is more available, and the opportunity cost is lower. That nonrandom siting of the merger presents challenges in identifying their effects. We apply several estimation strategies and choose different combinations of control groups to analyze how rezoning affects old neighboring districts.

Another challenge in analyzing the effect of rezoning on the new districts is finding proper control groups. As all counties under the administration of Beijing have been merged into urban districts, we select nearby counties in Hebei province as the control group (Here and after as nearby counties). Since this may result in an issue of spillover effects on geographically close counties- a positive spillover will result in underestimating the treatment effect, and vice versa.

Home Purchase Restrictions (HPRs) prohibit local residents (those with a registered permanent residence - Hukou) from buying more than two residential properties, and non-resident purchasers without Hukou from buying more than one residential property.[3] Out-of-town homebuyers who initially intended to purchase in new districts are prohibited from doing so and, consequently, shift their purchasing activity to nearby counties. These counties, situated close to Beijing and sharing similar amenities and commute costs with the new districts, experience a surge in house prices due to HPR, resulting in a positive spillover effect. To solve the problem, we choose counties that are relatively far away from the merged areas as the control group. Theoretically, the more distant a county is from the merged districts, the higher the cost of moving to the county and purchasing a home, and the smaller the spillover effect(Kline & Moretti, 2014). However, it's important to note that the impact of the merger on new districts should still be interpreted as the lower bound due to this positive spillover.

[2]Residential complexes ("Xiaoqu" in Chinese) refer to a collection of buildings, often high-rise apartments. They are often built by developers and are designed to accommodate a large number of residents. These complexes are normally gated communities, typically including parks, playgrounds, community centers, and retail shops.

[3]Even non-Hukou buyers require supporting documents identifying them as being active in the locality, e.g., a certificate of local tax payments or social security records for a certain period.

We find evidence of a significant reduction in house prices in old neighboring districts as a result of the merger. Our estimates show that, for residential complexes in old neighboring districts, house prices decrease by 5.1 percent in the first year and 7.4 percent in the second year after rezoning. Furthermore, this decline increases closer to the new districts, with the impact being localized within 20 kilometers of the distance to the border. Our findings also indicate that the merger has a larger impact on low-priced properties than high-priced properties.

We also find that the merger is associated with an appreciation in house prices in new districts. Specifically, house prices increase by 6.7 percent for the first year and 11.3 percent for the following year after the merger. Furthermore, we find evidence of a positive spillover of the merger to nearby counties, with this spillover decreasing as the distance to the new districts increases. Our results also show that the merger has a stronger impact on high-priced properties than low-priced properties within the new districts. This indicates that the merger is hurting the poorer households.[4] Putting all together, the heterogeneous impact of rezoning on high- and low-priced properties is in line with agglomeration economies and spatial mismatch hypotheses in urban economics that entail market failures and often predict overlap between poor economic performance and disadvantaged residents (Neumark & Simpson, 2015). Diamond & McQuade (2019) also find similar results in the US housing market. The Low-Income Housing Tax Credit (LIHTC) increase house prices in low-income neighborhoods while decreasing house prices in high-income neighborhoods.

Similar results are obtained when using the semiparametric propensity score weighting method in a difference-in-differences setting following Callaway & Sant'Anna (2021) and a Two-way Fixed-effects method following Wooldridge (2021). We also allow for post-treatment violation of parallel trends by using the method proposed by Rambachan & Roth (2023). We find that treatment effects remain statistically significant even when the violation of post-treatment parallel trends is one time larger than the extrapolated pre-treatment trend. The results are robust to an alternative quarterly frequency panel and are robust with a balanced panel set as well as adding macro-covariates.

Overall, this study contributes to the growing literature studying the impact of mergers on the housing market by quantifying the potential impact at the residential complex level and highlighting the local and external impact of the merger. This information is crucial for

[4] The assumption here is that the poorer households have lower budgets on home purchases, and thus, their probability of owning lower-priced properties in Beijing is higher.

policymakers as it may lead to a more accurate estimation of the welfare loss and gain associated with such mergers.

The rest of this paper is organized as follows: Section 3.2 reviews previous literature and gives a brief background on China's housing market policy focusing on the city-county merger of Miyun and Yanqing. Section 3.3 discusses the empirical strategy of the paper. Section 3.4 details and describes the data. Section 3.5 presents the results along with robustness checks. Section 3.6 discusses policy implications followed by conclusions in Section 3.7.

3.2 Background

3.2.1 Housing market and housing policies in China

Since the early 2000s, the Chinese government has been implementing policies to address issues such as affordability, housing shortages, and speculation in the housing market. Some of the key policies include promoting affordable housing, expanding the mortgage market, and implementing home purchase restrictions (HPR).[5]

Among those policies, HPR imposes limits on the number of properties that an individual or household can purchase, stringent requirements for accessing mortgages, and requirements for the amount of money that must be paid as a down payment (Deng et al., 2022).[6]

Existing research disagrees on the effectiveness of HPR. Many believe that HPR can effectively dampen speculative investment demand in the short term (e.g., Cao et al., 2018).[7] Conversely, some find that HPR cannot effectively curb speculative demand (e.g., Sun et al., 2017). Glaeser & Luttmer (2003) argue that HPR impairs rational housing demands and distorts housing market mechanisms, resulting in resource misallocation and social welfare loss. Somerville et al. (2020) find little evidence that HPR can restrain the growth in house prices, although HPR has had a salient and diminishing effect in suppressing housing market activity.

[5] To promote the development of affordable housing and expand the mortgage market, the Chinese government implemented a series of policies (Yang et al., 2014; Wu & Li, 2018; He et al., 2018; Painter et al., 2022). These policies included the establishment of the Housing Provident Fund in 1991, the provision of tax incentives for developers of affordable housing in 2008, the construction of public rental housing announced in 2010, the introduction of mortgage products with lower down payment requirements, and the establishment of mortgage lending guidelines that are more favorable to borrowers.

[6] HPR was first implemented in Beijing in 2010 and later expanded to other cities. However, given the rebound of house prices in 2015, the second round of housing purchase restrictions was implemented in 2016. HPR is a demand-side policy. Previous research has investigated the role of supply-side constraints on delivering additional housing units to the market in accounting for high prices (Glaeser & Gyourko, 2018).

[7] But HPR has limited effects on speculative demand in the long run (Cao et al., 2018; Somerville et al., 2020).

Notably, HPR has a positive spillover effect on adjacent unregulated cities, stimulating the spillover of cash flow from the core to the peripheral housing market(e.g., Jia et al., 2018; Wu & Li, 2018). Housing investors subject to HPR seek capital gains in peripheral markets with undervalued prices, which might increase land and house prices in adjacent unregulated cities (Deng et al., 2022). This positive spillover effect of HPR not only drives up the house prices of adjacent unregulated cities but also increases the aggregate consumer spending (e.g., Flavin & Nakagawa, 2008).

3.2.2 City–county merger and its effects

So far, little research has been conducted on the impact of city-county mergers on house prices in developing countries. Instead, there is much research on place-based policies' effects on developed countries. Principal examples include Enterprise Zones, European Union Structural Funds, and Industrial Cluster policies. Such place-based policy commonly targets underperforming areas, such as deteriorating downtown business districts and disadvantaged regions (Neumark & Simpson, 2015). Chen et al. (2022) find the Opportunity Zones (OZ) program, America's largest new place-based policy in decades, does not generate significant price changes in OZ areas. Busso et al. (2013) argue that the Empowerment Zone (EZ) program generates essential changes in local price levels and behavior. Overall, the impact of such place-based policies remains uncertain, and further research is needed to clarify the effects of these policies.

China possesses a hierarchical administrative system which divides areas into several levels of administrative units. The highest level comprises provinces, autonomous regions, municipalities, and special administrative regions.[8] These units are further divided into smaller administrative units, such as prefecture cities, then counties, and then townships. A prefecture-level city, contains urban districts, rural hinterlands, and county-level cities. Counties represent a level of the administrative division that is below prefecture cities, while urban districts are considered to be on the same level as counties.[9] County-level cities are under the same administrative level as counties and are generally larger in size than typical counties but smaller than prefecture-level cities.

[8]In total, China is divided into 23 provinces, 5 autonomous regions, 4 municipalities, and 2 special administrative regions.

[9]Townships, on the other hand, are administrative divisions that are tasked with the responsibility of administering specific areas within a county. They are generally located in rural areas and comprise a group of villages. They are smaller in size than counties and are responsible for providing services such as education, healthcare, and public security to the population within their jurisdiction.

For decades, counties have been the main administrative units in China's rural areas, responsible for providing a range of public services, including education, health care, and social welfare. However, counties have become increasingly ineffective and inefficient, with many counties facing challenges such as low levels of economic development, poor infrastructure, and a lack of skilled workers, resulting in significant efficiency and welfare losses, as noted by Au & Henderson (2006a,b).

To address these issues, the Chinese government implemented the merger policy to transform counties into urban districts. This transformation process involves the expansion of administrative boundaries and the implementation of urban planning policies. The merger brings many benefits to local governments. Transforming counties into districts is conducive to expanding the development space of central cities, promoting population agglomeration, accelerating the urbanization process of industries, and promoting real estate market development (Fan et al., 2012). Much research focuses on the effects of municipal amalgamations on the efficient provision of local public goods (Reingewertz, 2012). Some papers also investigate how mergers promote economic growth (Jia et al., 2021).

3.2.3 The merger of Miyun and Yanqing, Beijing

This study examines the effects of integrating Miyun and Yanqing counties into the urban districts of Beijing in October 2015. We select four cities for our analysis: Beijing, where the merger occurred; Langfang, Zhangjiakou, and Chengde in Hebei province, which are geographically proximate to Beijing. We exclude Tianjin from our study as it has no counties under its administrative purview. Furthermore, we exclude Baoding due to the unavailability of data. Figure 3.1 (a) illustrates a map of Beijing, with the sample in the empirical section part 1 being restricted to the districts in this map, except for the new districts. Figure 3.1 (b) specifically focuses on the new districts and nearby counties to analyze the effects of the merger, with the sample in the empirical section part 2 being restricted to the districts in this map, with the exception of the old urban districts.

[Figure 3.1]

This merger may significantly impact the housing market in old neighboring districts-Huairou, Changping, Shunyi, and Pinggu. From a demand perspective, the merger will boost the investors' confidence in the economic prospects of the new districts and redirect potential buyers to the new districts. This shift in purchasing behavior will decrease demand for housing in the old border districts. In the short term, the housing supply is fixed; therefore decreasing demand will lead to a depreciation in house prices in old border districts.[10]

Another consequence of the merger is a shift in the distribution of housing within the old border districts, particularly in the vicinity of the boundaries shared with the newly merged districts. Potential home buyers who were previously considering purchasing properties near the boundaries may shift their focus to the new districts, given the similarities in geographic location and commuting costs around the border.[11] Thus, we assume that the effects of the merger will be more pronounced in residential complexes located close to the border.

The merger may also have a significant impact on the housing market structure in the new districts. Prior to the merger, house prices in new districts were typically lower compared to those in old urban districts within Beijing. The consolidation of administrative units will lead to an influx of individuals and businesses into the new districts, which will contribute to an increase in housing demand. As housing supply is fixed in the short term, the increasing demand will lead to price increases.[12] Meanwhile, HPR will restrict non-Hukou buyers from purchasing in the new districts, resulting in a decrease in house demand. House prices may drop in the new districts.

Recent studies show that spillovers between housing markets are important(DeFusco et al., 2018; Bailey et al., 2018b) and that out-of-town investors contribute significantly to surges in house prices(Badarinza & Ramadorai, 2018; Cvijanović & Spaenjers, 2021; Chinco & Mayer, 2016; Sakong, 2021). In line with this, the merger in Beijing may also have a spatial spillover effect on the nearby counties. As strict home purchase restrictions are implemented in the new districts, it is likely that out-of-town home buyers who do not meet the criteria will opt to purchase properties in these nearby counties. This may lead to a positive spillover- an increase in demand for housing in these areas. In the short term, with a fixed housing supply in these counties, an increase in demand may result in an appreciation of house prices.

[10] The merger may reallocate construction efforts from old border districts to the new districts. Developers may anticipate an increase in house prices in Miyun and Yanqing following the merger and subsequently redirect their construction efforts to these districts. This shift in construction may result in a decrease in housing supply in the old border districts. However, it is important to note that this decrease in supply will only appear after a few years. In the long term, as the decrease in housing supply continues to outpace demand, house prices may begin to rise, offsetting the initial depreciation.

[11] Saiz (2010) emphasizes the importance of location and commuting costs in shaping housing market dynamics.

[12] In the long run, the merger of Miyun and Yanqing may lead to an increase in housing supply in the new districts, as developers may rush to meet the increasing demand for housing. Over time, as the supply of housing increases to meet the demand, it is possible that house prices may stabilize.

3.3 Empirical model

3.3.1 For old neighboring districts

To evaluate the ramifications of rezoning on old neighboring districts, we begin by using an event-study framework. Let $y_{i,j,t}$ denote the log mean house price of residential complex i in group j in period $t = -1, 0, 1, 2$, where $t = -1$ denotes two periods prior to the treatment, and $t = 0$ is our base year- one period prior to the treatment. The merger occurs at the beginning of the period $t = 1$.[13] We use $j = 1$ to signify the treatment group (residential complexes in the old border districts, including Huairou, Changping, Shunyi, and Pinggu, in Beijing) and $j = 0$ to indicate the control group (i.e., residential complexes in old non-border districts in Beijing). The causal impact of rezoning is then estimated using a multi-period difference-in-differences (DID) specification used in Chen et al. (2022), as given below:

$$y_{i,j,t} = \alpha_i + \lambda_t + \sum_{t=-1,t\neq 0}^{2} \beta_s 1_{s=t,j=1} + \varepsilon_{i,j,t} \tag{3.1}$$

where α_i represents residential complex fixed effects, it also captures common variations in house prices across different districts and residential complexes due to macroeconomic or city-wide shocks and policy changes. λ_t are time s specific fixed effects. $1_{s=t,j=1}$ are indicators for each period (except the period of policy implementation) that interacted with the treatment effect indicator. Thus $\{\beta_s\}_{t=1}^{2}$ represent the treatment effects over time of rezoning, in percentage. The empirical estimates of these parameters capture the change in house prices in treatment areas relative to the control areas in each period after rezoning is implemented. Following convention, estimates of $\{\beta_s\}_{t=-1}$ will be used to assess whether any potential confounding pre-treatment trends are evident.

In the second design, we investigate whether the effects of rezoning vary with the proximity of residential complexes to the boundaries of the new districts. Initially, we evaluate the impact of the rezoning on residential complexes that are located in close proximity to the boundaries of the new districts, using equation 3.1. Subsequently, we conduct a more comprehensive examination that includes all residential complexes located in the old border districts. These districts are subsequently categorized into five distance categories. We employ the two-way fixed effects (TWFE) framework used in Zheng et al. (2017) to analyze the treatment effect and spillovers

[13] Our data is centered around October 2015, when the policy was announced.

on the non-merged distance categories surrounding the merged area. Specifically, we use the following specification:

$$y_{i,j,t} = \alpha_i + \lambda_t + \sum_{n=1}^{5}\sum_{t=0}^{2}\gamma_m 1_{m=n,j=1,t\geq 1} + \varepsilon_{i,j,t} \tag{3.2}$$

where α_i represents residential complex fixed effects, λ_t captures time fixed effects, and $1_{m=n,j=1,t\geq 1}$ is a dummy variable that indicates whether or not the treated residential complex i is located in the n^{th} distance quintile to the boundary of new districts in period t. γ_n measures the treatment effect on the n^{th} quintile distance category.

Finally, we examine the potential heterogeneity in the impact of rezoning between high- and low-priced properties. To accomplish this, we introduce an interaction term that considers the treated old neighboring districts with an indicator for properties with prices higher than the average property prices over the sample period. We use the following specification:

$$y_{i,j,t} = \alpha_i + \sum_{t=0,t\neq 0}^{2}\phi_s 1_{s=t} + \sum_{t=0,t\neq 0}^{2}\beta_s 1_{s=t,j=1} + \sum_{t=0,t\neq 0}^{2}\delta_s 1_{s=t,high=1} + \varepsilon_{i,j,t} \tag{3.3}$$

where all indicators remain the same as in equation 3.1, except high indicates high price properties in the treatment group.

3.3.2 For new districts

To investigate the impact of rezoning on house prices in the new districts of Miyun and Yanqing, we anticipate an empirical challenge related to the potential spillover effect. Due to the home purchase restrictions imposed on out-of-town buyers, these buyers will likely purchase homes in nearby counties surrounding Beijing, leading to an increase in demand and rising house prices in these counties. This positive spillover may lead to an underestimation of the treatment effect, and thus, we must carefully consider the selection of the appropriate control group.

The estimated equation is the same as equation 3.1. We now use $j = 1$ to signify the treatment group- residential complexes in new districts-Miyun and Yanqing, and $j = 0$ to indicate the control group. To construct control groups, we use geographic information to calculate the minimum distance (in kilometers) of each residential complex outside Beijing to the border of new districts. We then select complexes in nearby counties that fall within specific quartiles of this distance to serve as our control groups.

We compare the closely located nearby Beijing counties, which are assumed to be affected by a positive spillover from the rezoning, with those that are farther away to quantify the magnitude of spillover. Theoretically, we expect the spillover effect to decrease as the distance to the new districts increases, with the magnitude of the spillover between geographically distant counties assumed to be negligible (Kline & Moretti, 2014). By comparing close counties and far away counties, we can have an initial understanding of the magnitude or at least the presence of spillover.

In this design, the estimated equation remains the same as equation 3.1. However, we have three treatment and control comparisons, each of which corresponds to a different quartile of distance from the new districts. We compare residential complexes outside of Beijing for which the distance to the boundary of new districts is within the first quartile with those in the fourth quartile and compare residential complexes in the first and second distance quartile with the rest.

Butts (2023) argues that classical difference-in-differences estimation may yield biased estimates for the average treatment effect when the effects of treatment cross over borders. The bias arises from two factors related to spillover effects: (1) the control group no longer represents the counterfactual trend because the treatment influences their outcomes, and (2) changes in the outcomes of treated units reflect not only their own treatment status but also the effects of the treatment status of "close" units. To address these biases, he proposes a new estimation strategy that can eliminate both sources of bias.

To identify and remove the bias of spillover, we divide all residential complexes in nearby counties (control group) into five distance categories. The treatment group remains the same - residential complexes in the new districts of Miyun and Yanqing in Beijing. Control categories 1 - 4 represent residential complexes in nearby counties for which the distance to the boundary of new districts is between 1-4 distance quintile. Residential complexes for which the distance is in the fifth quintile are dropped as the reference group. This enables us further to examine the relationship between distance and spillover effect.

The estimated model follows Butts (2023), which is slightly different from equation 3.2 as given below:

$$y_{i,j,t} = \alpha_i + \lambda_t + \sum_{n=1,n\neq5}^{5} \sum_{t=0}^{2} \gamma_m 1_{m=n,j=0,t\geq1} + \sum_{t=0}^{2} \theta_j 1_{j=1,t\geq1} + \varepsilon_{i,j,t} \tag{3.4}$$

where γ_m captures the percentage change in house prices in different control distance categories after rezoning, compared with the furthest distance group. θ represents the treatment effect of rezoning on the rezoned Miyun and Yanqing.

In line with our previous analysis, we aim to investigate whether the impact of rezoning on house prices in the new districts differs between high-priced and low-priced properties. To accomplish this, we follow the previous approach and employ a dummy variable measuring whether mean property prices in the new districts were high during the sample period. The empirical model employed in this analysis is identical to that presented in equation 3.3.

3.4 Data

In our analysis, we utilize a comprehensive dataset on house prices. The primary source for this data is CityRE, a reputable national real estate data provider.[14] CityRE covers more than 90 percent of online listings of housing transactions, which offers a substantial sample for our estimations. To construct the monthly complex average price, CityRE calculates the average price per square meter ($yuan/m^2$) of all newly online listed apartments within a given complex for that month. Our data also includes complex-specific characteristics, such as the year of construction, the type of complex, the greening ratio, the volume ratio, the property management fee, the number of units, the land area, and the building-up area.

To account for the influence of location on house prices, we collect the latitude and longitude of each complex from CityRE and calculate the shortest distance from each complex to each district boundary using the statistical software Stata.[15] Additionally, we gather location information for points of interest (POI), such as malls, parks, subway stations, tertiary hospitals, and schools, utilizing the Baidu map API. We then calculate the distance of each complex to all POIs in order to consider the complex-specific characteristics.[16]

We use a DiD approach with an event study setting to examine the impact of the rezoning on house prices in the new and old border districts. Our analysis covers four years of data

[14] This dataset has been used in many recent papers, such as Deng et al. (2022).

[15] To accomplish this, we divided the boundary shape file into segments no longer than 100 meters and calculated the distance of each residential complex to its nearest segment, which serves as a proxy for the shortest distance to the boundary. Codes for this process are available upon request.

[16] It should be noted that both public and private hospitals in China are classified in a 3-tier system that recognizes a hospital's ability to provide medical care, medical education, and conduct medical research. In this system, hospitals are classified as Primary (I class), Secondary (II class), or Tertiary (III class) institutions, with higher tiers indicating a higher level of care.

from October 2013 to September 2017.[17] We verify the common trend assumption by utilizing data from 1-2 years prior to the policy announcement and designated the 0-1 year prior to the announcement as the reference period. We then assess the impact of the policy by analyzing the two years following the announcement.

We test our hypothesis that rezoning may negatively impact house prices in old neighboring districts using equation 3.1 and panel data analysis spanning October 2013 to September 2017. Our treatment sample includes 3,335 observations from residential complexes located in the old border districts of Huairou, Changping, Shunyi, and Pinggu, divided into four periods: 753 and 821 observations for 1-2 years and 0-1 year prior to the policy announcement, and 888 and 873 observations for 0-1 year and 1-2 years post-policy announcement. The first control group consists of 4,883 observations representing all residential complexes in old neighboring districts-Mentougou, Fangshan, Daxing, and Tongzhou), which are similarly divided into four periods with 1,122, 1,192, 1,295, and 1,274 observations, respectively. Thus, our first analysis includes a total of 8,218 observations.[18]

In our second analysis sample, we expand the control group to a larger area while the treatment group remained the same. This results in a control group including only five districts (Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng), with 9,246 observations from October 2013 to September 2017 (unbalanced panel).[19]

In control group 3, we further extend the control area by adding another district- Shijingshan, while keeping the other districts the same as in control group 2. In total, there are 9,898 observations. To ensure that there is no selection bias, we include all districts in control groups 1, 2, and 3 in control group 4, resulting in a total of 22,137 observations.

Table 3.1 presents an overview of the simple difference in mean property prices, both in levels and logs, between the old neighboring districts (treatment group) and other old non-neighboring districts (control group) in Beijing. The four sample sets used in this analysis are consistent with those previously discussed.

[17] It is worth noting that after September 30, 2016, Beijing and 16 other cities implemented new policies to tighten their property markets with unprecedented intensity. The six restrictions of "restrictions on the purchase, loans, sales, prices, land auctions and conversion of commercial to residential" were implemented, and real estate financing was tightened across the board. Therefore, we limited the sample period from October 2013 (two years prior to the policy announcement) to September 2017 (two years post-policy announcement).

[18] The data used in this analysis are obtained directly from cityRE and used without restriction.

[19] One concern is that individuals may shift from purchasing houses in nearby old districts to the newly merged rezoned areas, and this spillover may affect longer distance than we assumed, resulting in an underestimation of the impact of rezoning. To address this concern, in the second control group, we excluded those districts in the control group that were adjacent to the new districts.

[Table 3.1]

In Table 3.1, Panel A, the first nine rows provide detailed information on house prices and differences in the first sample set. Columns (1) and (2) show the two-year average house prices before and after the policy announcement, respectively. The average house price in the treatment group (old border districts) is 21,266 $yuan/m^2$ before and 29,488 $yuan/m^2$ after the policy announcement. The difference in the treatment group's average prices pre- and post-policy announcement, as shown in Column (3), is 8,222 $yuan/m^2$, which is statistically significant at the 1% level. This indicates that the average house prices in the treatment group increased by 8,222 $yuan/m^2$ following the policy announcement. Similarly, the first control group experiences an increase in average house prices of 10,338 $yuan/m^2$, which is also statistically significant at the 1% level. We see the same pattern when we employ other control groups for comparison.

In the seventh row of Table 3.1, labeled as 'Difference', we see that prior to the policy announcement, the difference in average house prices between the treatment group (old border districts) and the control group (other old districts in Beijing) is 700 $yuan/m^2$, while post-policy announcement, this difference became -1,416 $yuan/m^2$. The difference-in-differences between the treatment and control groups, pre- and post-policy announcement, is -2,116 $yuan/m^2$, statistically significant at the 1% level. This indicates that the merger significantly decreased average house prices in the old border districts compared to the first control group.

Panel B of Table 3.1 presents similar results to Panel A, but expresses the data in logarithm. We prefer logarithm data in this context as it allows us to focus on the percentage changes (Roth & Sant'Anna, 2023; Sant'Anna & Zhao, 2020). The difference-in-differences in all control groups are consistently negative and statistically significant in both levels and logs, indicating that rezoning significantly decreases house prices in the old border districts.

Similarly, to examine the hypothesis that rezoning may positively impact house prices in the new districts, we conducted an analysis utilizing equation 3.1. Our sample consisted of 648 observations from a panel data set spanning from October 2013 to September 2017, representing all residential complexes located in the new districts of Miyun and Yanqing.

Our analysis include four sets of control groups to capture the potential spillover effects of rezoning on house prices in nearby counties. Control group 1 consists of 756 observations of residential complexes in nearby counties, for which the distance to the boundary of the rezoned areas is within the first quartile (33.9km). Control groups 2, 3, and 4 consist of 572, 640, and 378 observations, respectively, of residential complexes in nearby counties, for which the distance to the boundary of the rezoned areas falls in the second quartile (33.9km to 91.3km), third quartile (91.3km to 110.8km), and fourth quartile(110.8km to 177.6km), respectively.

The furthest distance control group, control group 4, is expected to have the smallest spillover impact, and thus provides a clearer picture of the impact of rezoning on house prices. Therefore, we prefer this set of samples. There are 1,026 observations in total.

Table 3.2 presents a brief overview of the difference in average property prices (in levels and logs) between the new districts (treatment) and nearby counties (control). The difference-in-differences between the treatment and control group, pre- and post-policy announcement, are negative when the control groups are geographically close to the new districts, while they become positive as the distance increases. This change in signs suggests a potential spillover effect of rezoning to the closely located counties. We prefer the last set of data (control group 4) as it captures the smallest spillover effect. The difference-in-differences between the treatment and control, pre- and post-policy announcement, is 3 percent (4,149 $yuan/m^2$ in level). This indicates that the merger roughly increases average house prices in the new districts (treatment group) by 3 percent compared to the change in average house prices in the furthest control group.

[Table 3.2]

As previously discussed, our analysis utilizes a four-year data set spanning from October 2013 to September 2017, with the policy announcement of rezoning taking place in October 2015.[20] To estimate the impact of rezoning on house prices in the old border districts and the new districts, we utilize the second year before the policy announcement to test the parallel trends assumption. One year prior to the policy announcement is dropped as a reference period.

To estimate the impact of rezoning on house prices varies with the distance to the boundary of new districts, we employ equation 3.2. As the impact of rezoning can be localized and it can be difficult to find common trends, we exclude one year of data from our sample. Our sample period spans from October 2014 to September 2017, and we use the one year prior to the policy announcement as the reference period.

[20] It is important to note that each empirical analysis presented in the study utilizes a different sample period. For more detailed information, please refer to the notes below each table.

3.5 Results

3.5.1 Old neighboring districts

Table 3.3 presents the results of the analysis on how rezoning affects property prices in Beijing's old border districts (treatment) using equation 3.1. We begin by examining Column (1), representing our preferred analysis sample. The first row of the table shows the difference in house prices between the old border districts (treatment) and the control group before the policy announcement. The results indicate that the average house prices in the old border districts were 0.4 percent lower than in the control group 1. This difference is not only small in magnitude but also statistically insignificant, supporting the parallel trends assumption. This allows us to identify the treatment effect in a difference-in-differences design. The second and third rows of the table report the impact of rezoning on the treatment group one year and two years after the policy announcement, respectively. The results show that rezoning significantly decreases house prices in the old border districts by 5.1 percent in the first year and 7.4 percent in the second year.[21]

[Table 3.3]

In Columns (2), (3), and (4), we present the results obtained using control groups 2, 3, and 4, respectively. The parallel trends assumption is satisfied, and the results are consistent with those presented in Column (1) regarding magnitude and statistical significance. Rezoning significantly decreases the average house prices in the old neighboring districts, similar to control group 1. However, the impact of rezoning in the second year is lower than control group 1. This may result from Beijing implementing new policies on September 30, 2016, to tighten their property markets with unprecedented intensity.

This result is consistent with our hypothesis that rezoning will shift potential buyers from purchasing and building in the old border districts to purchasing in the new districts, leading to decreasing demand for houses in the old border districts.[22]

Our findings are broadly consistent with other scholarly work on the externalities of place-based policies, such as the findings of Moretti (2010) who argue that Enterprise Zone policies

[21] We consider statistical significance at 5% level in this paper. The R^2 is large as we controlled for complex fixed effects.

[22] rezoning can also shift developers to the new districts and increase house supply in the long run. However, the decrease in demand caused by the rezoning may outpace the decrease in supply in the short run, resulting in a depreciation of house prices in the old border districts.

can attract labor and reduce house prices in neighboring areas and Hanson & Rohlin (2013) who find negative externalities of the Empowerment Zones program in the US on census tracts that are geographically or economically close to zone tracts. These studies support our conclusion that rezoning can hurt the housing market in the old neighboring districts.

In this study, we have examined the relationship between the impact of rezoning on residential complexes and the distance of such complexes to the boundary of new districts. Table 3.4 presents a breakdown of the treated complexes according to their distance to the boundary within the first quintile. Panel A pertains to complexes located within the 5th percentile distance to the boundary, while Panel B, C, and D pertain to complexes located within the 10th, 15th, and 20th percentile distance to the boundary, respectively.

[Table 3.4]

Column (1) of Table 3.4 demonstrates that, for complexes located within the 5th percentile distance to the boundary of new districts, the average price significantly decreases by 11.4 percent in the first year after rezoning, with a decrease of 17.3 percent in the following year. Similarly, complexes located within the 10th percentile distance to the boundary experience a decrease of 10.1 percent in the first year and 14.7 percent in the following year. As the distance to the boundary increases to the 15th and 20th percentile, the impact of rezoning on property prices decreases, with decreases of 8.8 percent and 7.5 percent in the first year, respectively, and 12.3 percent and 8.7 percent in the following year. Notably, the negative impact of rezoning on treated complexes declines in absolute value as the distance to the boundary increases. Columns (2), (3), and (4) of Table 3.4 present the results obtained from utilizing control groups 2, 3, and 4, respectively. These results are consistent with those obtained from control group 1 in that rezoning decreases house prices in the old border districts.

The results presented in Table 3.4 provide further support for our hypothesis, which posits that rezoning reshapes the distribution of housing in the old border districts around the boundaries shared with new districts. Specifically, the results indicate that residential complexes share similar locations, amenities, and commute costs around the boundary. As a result, home purchasers will likely shift from buying in the old border districts to new districts, thereby leading to a stronger impact (in absolute value) when compared to the results obtained in Table 3.3.

The current findings align with previous research in urban studies that have highlighted the impact of the spatial mismatch of place-based policies on house prices. Studies such as that of

Favilukis & Van Nieuwerburgh (2021) have demonstrated the importance of commute cost in the home purchase decisions of households, particularly for low-skilled workers for whom commuting expenses are a significant portion of their income. Rothenberg (2013) also emphasizes the significance of transportation infrastructure in determining where firms are located and how economic activity is distributed spatially.

In this study, we sought to investigate whether the declining impact of rezoning on house prices is localized or extends to a broader area. To this end, we utilize Table 3.5, which reports the results obtained from equation 3.2 with five distance categories.

[Table 3.5]

Contrary to our initial expectation of a decreasing impact of rezoning in absolute value as the distance category increases, we find that the impact of rezoning on different treatment categories decreases initially and then increases. Specifically, Column (1) of Table 3.5 shows that rezoning decreases prices of complexes located within the first quintile (the first distance category) to the boundary of rezoned districts by 6.4 percent, while the decrease is 4.0, 5.0, 7.1, 7.7 percent for the later quintiles.

We obtain similar results in Columns (2), (3), and (4) of Table 3.5. We observe a negative and significant impact of rezoning on all distance categories. However, the absolute value of this negative impact decreases initially and then increases as distance categories increase. These results show that rezoning mainly reshapes the distribution of housing in the old border districts around the boundaries shared with new districts. The declining trend in the absolute value of rezoning is localized and cannot be extended to a broader area.

Finally, Table 3.6 presents the results of our estimation of the heterogeneous effect of rezoning on high-priced and low-priced properties, as specified in equation 3.3. We find that property prices drop after rezoning, but the decline is smaller for more expensive properties.

[Table 3.6]

Column (1) of Table 3.6 presents our preferred results. In the first year of rezoning, the decrease in prices of expensive properties is 3.3 percent smaller compared to low-priced properties. In the second year of rezoning, the decrease in high-priced properties is 1.1 percent lower than in low-priced properties.

The results in Columns (2), (3), and (4) are similar to the results in Column (1). Rezoning decreases house prices for all properties in the old border districts. However, it decreases house prices more for low-priced properties than high-priced ones. This result aligns with our hypothesis that rezoning substantially impacts low-priced properties in the old border districts.

This could be due to poor households shifting to newly merged districts to purchase homes, as the prices are lower there, and the commute cost can be expected to be lower in the future because the rezoned areas are merged into Beijing, where more constructions on the road network will be implemented soon. Diamond & McQuade (2019) found similar results that the Low Income Housing Tax Credit (LIHTC) increases house prices in low-income neighborhoods while decreasing house prices in high-income neighborhoods. In another related study, Akbar & Duranton (2017) investigated the travel cost in India and found that, given that households typically devote between 10 and 20 percent of their income to transport expenses and one to two hours a day per person to travel, the potential loss from less efficient urban transport is large. This highlights the significance of transportation and neighborhood amenities in home purchasing decisions.

In summary, the findings from Table 3.3 to Table 3.6 suggest that rezoning has a negative impact on house prices in the old border districts in Beijing. Specifically, the results show that within the first quintile to the shared boundary of new and old neighboring districts, the impact is higher closer to the shared boundary. However, this trend is localized and does not extend to broader distance categories. Additionally, the results reveal that although prices of all properties in the non-rezoned border districts decrease, there is a larger decrease in house prices of low-priced properties compared to high-priced properties. These results provide insight into the localized and heterogeneous effects of rezoning on house prices and highlight the importance of considering the specific context of rezoning policies and the surrounding area when evaluating their impact on housing markets.

3.5.2 New districts

In this section, we investigate the impact of rezoning on the new districts. Table 3.7 presents the results of our analysis, which tests whether rezoning increases property prices in the new districts or not, utilizing equation 3.1. The treatment group in this analysis includes residential complexes located in the new districts of Miyun and Yanqing. The four control groups consist of residential complexes in nearby counties surrounding Beijing, with the distance to the boundary of rezoned areas within the first, second, third, and fourth quartile, respectively.

[Table 3.7]

As previously discussed, the potential spillover effect of rezoning on nearby counties is expected to be smallest furthest away from the new districts. Therefore, our preferred results are reported in Column (4). The parallel trends assumption is satisfied, as the difference between the treatment and control group prior to the policy announcement is 0.016, which is not only small in magnitude but also statistically insignificant. The results show that rezoning increases house prices in the new districts by 6.4 percent in the first year and 11.3 percent in the following year.

However, Table 3.7 reveals that rezoning significantly decreases house prices in the new districts when utilizing control groups 1-3. Specifically, the difference in property prices between the treatment and control group prior to the policy announcement is 0.037 and 0.049 in Columns (1) and (2), respectively, both of which are statistically significant at the 1% level. However, it is important to consider these findings in conjunction with the preferred results in column (4) and consider the specific externalities of rezoning in nearby counties.

Taken together, the results presented in Table 3.7 support our hypothesis that rezoning has a positive impact on property prices in the new districts. The results show that rezoning attracts individuals to purchase properties in new districts as opposed to other districts in Beijing. Given that construction is relatively fixed in the short run, representing a fixed supply, an increase in demand for housing in these areas leads to an appreciation in property prices.

This finding aligns with previous studies on the impact of place-based policies on house prices. For instance, research by Koster & Van Ommeren (2019) suggests that beneficial place-based policies lead to an increase in house prices. Similarly, Busso et al. (2013) identify significant positive impacts of Empowerment Zones in the United States on house prices, which they estimate to be 28-37%. Additionally, Chaurey (2017) examines the micro-level effects of a location-based tax incentive scheme in India and finds notable increases in employment, overall output, fixed capital, and the number of firms, indicating that place-based policies can have broader economic impacts.

Further analysis in Table 3.8 confirms the spillover effect of rezoning on nearby counties. Specifically, in the first column of Table 3.8, we compare the impact of rezoning on complexes that are closest to the new districts (in the first quartile), with those that are furthest away (in the fourth quartile). The results show that rezoning increases the prices of complexes close to the new districts by 22.1 percent in the first year and 34.9 percent in the second year.

[Table 3.8]

Column (2) divides complexes in old counties into two groups according to distance: within the first and second quartiles and within the third and fourth quartiles. The results are similar to those in column (1), with a smaller coefficient, which is consistent with our expectation- the spillover effect should be most significant for the closest complexes.

Due to home purchase restrictions in Beijing, the rezoning policy makes it difficult for out-of-town home buyers to purchase in the new districts. As a result, these buyers are forced to purchase in nearby counties surrounding Beijing, leading to an increase in house prices in those counties. This positive spillover effect is in line with previous literature on the topic. For example, Deng et al. (2022) investigate the home purchase restrictions in China and find that these restrictions not only effectively curb the surge in local house prices, they also induce capital flight and sharp abnormal increases in house prices in nearby unregulated cities. Additionally, Zheng et al. (2017) analyze the impact of 43 state-level and 67 provincial-level industrial parks in China and find that they induce positive spillover effects on surrounding areas.

The results in Table 3.9 provide evidence that the positive spillover effect of rezoning is strongest for the closest counties surrounding the new districts. We divide all residential complexes in nearby counties (control group) into five distance categories. Then, we drop category 5 (complexes in the fifth quintile distance) as the reference group. We find that rezoning increases house prices by 36 percent in the closest control complexes, while it increases house prices by 37.9 percent, 23.4 percent, and 15.7 percent for control distance categories 2, 3, and 4, respectively, in the two years following the rezoning. At the same time, rezoning increases house prices in the new districts by 9.1 percent from our estimation.

[Table 3.9]

The results confirm our expectations. Although the impact of rezoning for Category 2 is similar to Category 1, it shows the trend in general- the positive spillover decreases as the distance category increases. This suggests that rezoning reshapes the distribution of housing demand in nearby counties, with home buyers seeking out geographically proximate locations to purchase homes when blocked from purchasing in new districts by home purchase restrictions.

In Table 3.10, we test the heterogeneous impact of rezoning on high- and low-priced properties in the new districts using our preferred control group- the furthest complexes located at the fifth quintile distance. The results show that, comparing to low-priced properties, house prices increase 2.4 percent larger for high-priced properties in the first year of rezoning and 4.2 percent larger in the following year.

[Table 3.10]

This heterogeneous effect most likely result from home buyers shifting from other districts in Beijing to prefer purchasing better quality, more expensive houses in the new districts. This is consistent with what we observed from Table 3.1 and 3.2. House prices in the new districts of Miyun and Yanqing before rezoning are the lowest: 15,513 yuan/m^2 (See Table 3.2), while prices are over 20,000 yuan/m^2 for other districts (See Table 3.1). Those buyers who shift to purchase in Miyun and Yanqing have significantly large budgets, and, unsurprisingly, they prefer to purchase higher-priced properties. In previous literature, Krupka & Noonan (2009) have parallel findings that the Empowerment Zone areas, on average, became more attractive, but also suggest that the program's benefits likely did not accrue to the lower-income residents of the Zone. Diamond & McQuade (2019) also argue that the Low Income Housing Tax Credit (LIHTC) increases house prices in low-income neighborhoods while decreasing house prices in high-income neighborhoods.

Taken together, the results from Table 3.7 to 3.10 indicate that rezoning positively impacts property prices in the new districts. Furthermore, we find evidence of a positive spillover effect on housing markets in nearby counties. However, this spillover effect diminishes as the distance to the boundary of new districts increases. Importantly, even when accounting for these spillover effects, the positive impact of rezoning on new districts remains substantial. Our findings align with previous literature on place-based policies, which suggest that spillovers may not be a primary concern in certain contexts. Additionally, our results indicate that rezoning has a greater impact on relatively expensive properties, a finding that is consistent with previous studies such as that of Krupka & Noonan (2009).

3.5.3 Robustness Check

There is increasing debate about the reliability of TWFE estimator under staggered treatment timing (Callaway & Sant'Anna, 2021; Sant'Anna & Zhao, 2020; Baker et al., 2022). However, we employ TWFE for our analysis as there are three advantages. First, the TWFE method is useful for impact analysis as the approach is somewhat resilient to missing data problems (Wooldridge, 2010, 2019). Second, The TWFE estimator is consistent and asymptotically normal without strict exogeneity, and the method can allow for heterogeneous trends. It is also easy to modify the TWFE estimator to allow for spillovers (Hansen, 2007). Third, if one allows treatment effects to be heterogeneous in the TWFE model, using TWFE is advantageous as the method is consistent for unbalanced panels even when selection is correlated with additive, unobserved heterogeneity, compared to the estimators that include time-constant cohort indicators and time effects (Wooldridge, 2021). Since we are dealing with only one (not staggered) intervention and using an unbalanced panel with the possibility of spillover, we primarily used a DiD method with TWFE.

However, we conducted a bunch of robustness checks to examine how the estimated impact varies with the use of other methods. The results are given in this section.

Propensity-score-weighted difference-in-differences. Our baseline identification may face the challenge that treatment and control groups differ in observable characteristics. Unbalanced characteristics suggest that parallel trends may not hold. We tackle this challenge with a propensity-score weighting approach (Abadie, 2005). Our results in Table A.1 and Table A.8 repeat Table 3.3 and Table 3.7 respectively, with Callaway & Sant'Anna (2021)'s method. The results are consistent with what they are in the baseline model. Rezoning decreases house prices in old border districts and increases house prices in the new districts.

Two way fixed effects by Wooldridge (2021). Wooldridge (2021) proposes a method that provides more efficient estimations of treatment effects when applied appropriately, compared to Callaway & Sant'Anna (2021). Furthermore, this method ensures robustness against potential biases stemming from using different control groups. We thus repeat Table 3.3 and Table 3.7 with Wooldridge (2021)'s method and present the results in Table A.2 and Table A.9. The results are robust.

Allowing for post-treatment violation of parallel trends. One concern is the potential post-treatment violation of parallel trends, which may undermine the validity of our causal conclusions based on the DID estimator. We incorporate the method proposed by Rambachan & Roth (2023). We re-estimate Table 3.3 and Table 3.7 using honestdid command in Stata [23]. The results are plotted in Figure 3.2 through Figure 3.6. The figures suggest that the significant

[23] We choose $M = 1$ as this bounds the worst-case post-treatment difference in trends by the equivalent maximum in the pre-treatment period, which is suggested when equal numbers of pre and post-years are available. Please see page 12 of Rambachan & Roth (2023) for more details

result is robust to allowing for violations of parallel trends up to one time as big as the max violation in the pre-treatment period [24].

With quarterly house prices. We re-estimate equation 3.1 with quarterly house prices.[25] The quarter prior to the policy announcement is dropped as the reference group. Table A.3 repeats Table 3.3 with quarterly house prices, while Table A.10 repeats Table 3.7. Again, we observe no violation of the parallel trends assumption. The results are consistent with our baseline model.[26]

Including control variables. We include a set of local controls to examine how a confounding policy or variable could drive the treatment effects.[27] Our results in Table A.4 and Table A.7 repeats Table 3.4 and Table 3.6 respectively, with macro controls. And Table A.11 and Table A.14 repeats Table 3.7 and Table 3.10, respectively. The results in those tables show that including demographic and geographic variables in the regression has no impact on the estimated treatment or period fixed effects.

Using alternative distance cutoffs. We find that the heterogeneous impact of rezoning on old border districts is localized (see Table 3.5). To avoid selection bias, in Table A.6, we repeat the analysis with four distance categories. The results are robust. In Table 3.9, we find that the positive spillover of rezoning on nearby counties decreases as the distance to the rezoned areas increases. Again, we repeat Table 3.9 with four distance categories and drop the distance category four as the basement group. The results in Table A.12 confirm our argument.

With event-study setting. In Table A.6 and Table A.13, we re-estimate Table 3.5 and Table 3.9 with event study setting. The sample period is from one year prior to policy announcement to two years post-policy announcement. One year prior to policy announcement is dropped as the reference group. The results remain robust.

Balanced data set. To date, all results are from unbalanced panel data. One concern is that the results may differ with balanced panel data. We thus re-estimate all regressions in the baseline model with a balanced panel in Robustness Check B. The results remain robust.

[24] In all cases, the result of the honestdid shows a robust confidence interval for different values of M. We see that the "breakdown value" for a significant effect is $M \approxeq 1$.

[25] The panel is, again, unbalanced.

[26] Though the coefficient is not always significant at the 5% level, we believe it is because quarterly data is noisy, and it is hard to capture the real impact of rezoning. Moreover, it will not hurt our main conclusion.

[27] These controls include per capita gross regional product (GRP), local government general public budget revenue, and local government general public budget expenditure. The data for these variables are sourced from the China County Statistical Yearbook by the National Bureau of Statistics of China.

3.6 Policy implications

Our empirical analysis reveals that implementing rezoning policies in Beijing has led to notable changes in local property prices. However, it must be noted that the available data is insufficient to fully evaluate the welfare consequences of these changes. Furthermore, our analysis is limited to the impact of the rezoning on residential housing within Beijing and does not take into account the effects on unoccupied or industrial land. Given the inelastic nature of the housing supply in the short term, we have assumed it to be fixed. We then compare the total decrease in house prices of the four old border districts because of rezoning with the total increase in house prices in new districts due to rezoning. Nonetheless, we will now do two simple calculations estimating the total increase in housing wealth in new districts and the total decrease in the old border districts.

In order to estimate the total decrease in housing wealth in the old border districts, we utilize the floor area of residential buildings per capita in Beijing as a proxy for residential area per capita. According to data from the Beijing Municipal Bureau of Statistics, Beijing's average residential area per capita was 31.31 square meters in 2013, with slight variations in the following years.[28,29] The population of each district is presented in Table 9. Before the policy announcement, the total housing area was 118,651,095 square meters, while the floor area post-announcement was 127,224,615 square meters. The merger decreased house prices by 7 percent compared to the counterfactual scenario, as indicated by Table 3.1. House wealth is determined by the total residential area in each district multiplied by the corresponding house prices in that period. Therefore, the merger decreased house wealth by 0.26 trillion Chinese yuan compared to the counterfactual scenario. This value is the lower bound of wealth loss in the old border district as we did not consider the spatial spillover of old border districts to other old non-border districts.

Similarly, we calculate the total increase in house wealth in the new districts. From Table 3.1, the merger increased house prices by 3 percent, equating to roughly 4,026 Chinese yuan. This increases house wealth by 0.11 trillion yuan in the new districts compared to the counterfactual scenario.[30] In total, the merger resulted in a decrease in house wealth in the new and old border districts by a minimum of 0.15 trillion yuan in the two years following the rezoning.

[28] This value is 31.54, 31.69, 31.38, and 32.56 in 2014, 2015, 2016, and 2017.
[29] Website of Beijing Municipal Bureau of Statistics `http://tjj.beijing.gov.cn/`.
[30] However, this increase in wealth is underestimated due to the positive spillover to surrounding counties.

The merger of Miyun and Yanqing has led to a significant decline in house prices in old border districts, resulting in a decrease in total house wealth for residents in these areas. This highlights the importance of considering the potential negative effects of place-based policies on housing markets and the potential spillover effects on neighboring areas. Policymakers should consider this when implementing similar policies in the future and consider ways to mitigate negative impacts on affected communities. This could include providing compensation or support for residents who may have lost value in their homes or implementing policies to encourage development and investment in neighboring areas to mitigate the negative effects of rezoning on house prices. Additionally, policymakers should also consider ways to increase the supply of affordable housing in new districts to ensure that low-income residents are not priced out of these areas.

It is worth to note that although the merger has resulted in a decrease in housing wealth, it does not necessarily imply a decrease in overall welfare in Beijing. The integration of two counties into Beijing has expanded the availability of affordable housing. In particular, house prices in Miyun and Yanqing are considerably lower compared to other urban districts in Beijing. Consequently, homebuyers have more disposable income to allocate towards other consumption goods and may experience an overall improvement in their financial well-being. Additionally, the merger has redistributed demand within Beijing, leading to increased affordability of housing in old neighboring districts where house prices are now more competitive. In summary, this policy change may enhance the overall welfare of Beijing residents.

3.7 Conclusion

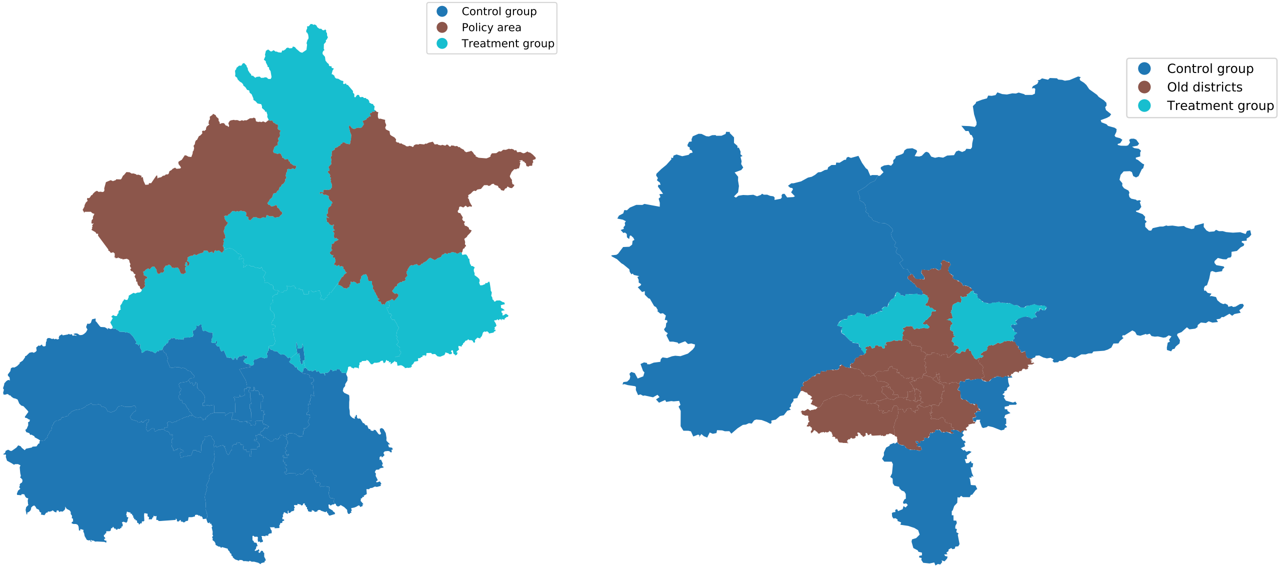
The city-county merger is China's most important national spatial policy since the county-to-city upgrade program. It is intended to address the ineffective and inefficient county system, where low levels of economic development, poor infrastructure, and a lack of skilled personnel lead to huge efficiency and welfare losses. This program hopes to generate neighborhood revival, yet we find conflicting evidence to support this view.

Using a difference-in-differences approach, we found that house prices have gone up in new districts after their enactment in 2015, but at the same time, house prices have decreased in old border districts. We also found evidence suggesting that the impact of rezoning increases as the distance to the border increases, but this impact is localized, within the first quartile distance, and does not extend to a broader area. We also found suggestive evidence that rezoning increased prices of more expensive houses in the new districts and reduced prices of cheaper houses in old border districts, presumably because potential buyers in Beijing are under different financial conditions- home purchasers that shifted from old border districts to new districts may have limited budget. In contrast, buyers in new districts shifting from other districts in Beijing may have a higher budget than the cheap property prices and prefer more expensive properties. The results are robust to various specifications and estimation techniques.

Our study also highlights the importance of considering spillover effects when evaluating the impact of place-based policies on housing markets. We find that rezoning has a positive spillover effect on housing markets in nearby counties, with the spillover effect decreasing further from the boundary.

In conclusion, our study provides valuable insights into the impact of the merger on house prices. Our results show that rezoning has significantly impacted the distribution of housing.

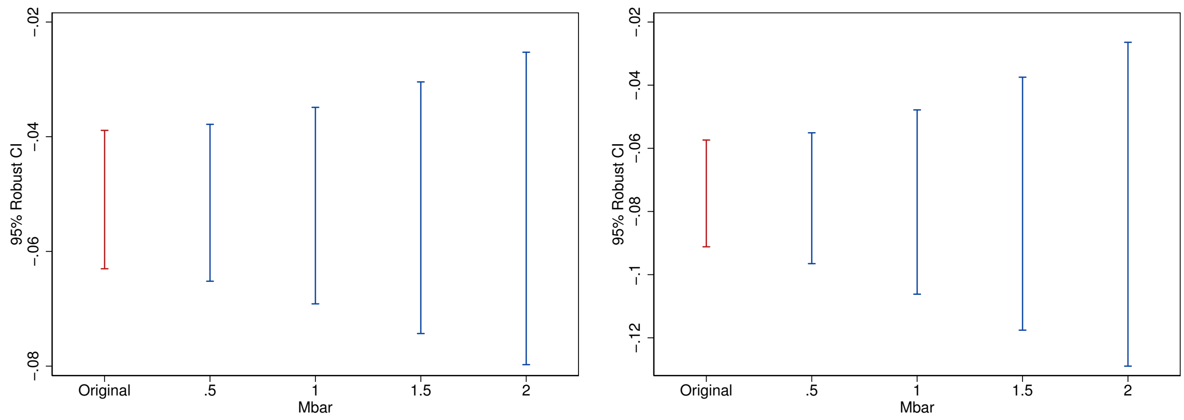
Tables and figures



(a) Beijing

(b) Hebei

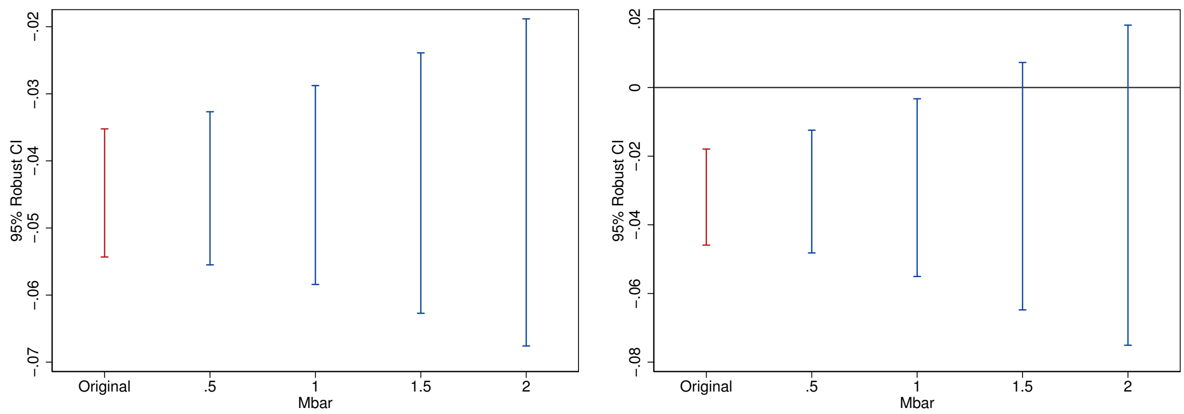
Figure 3.1: Map of Beijing and Hebei



(a) t=1 (b) t=2

Figure 3.2: Allowing for post-treatment violation of parallel trends (a)

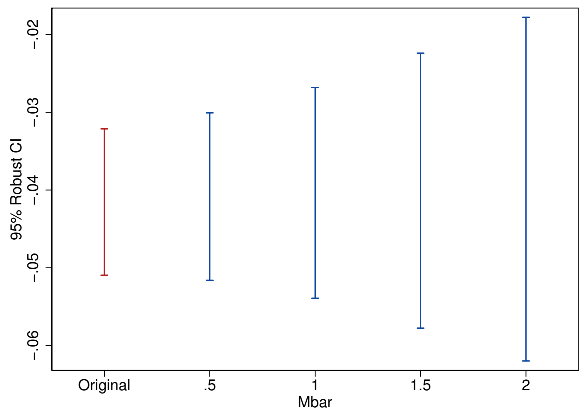
(Repeat Table 3.3 Column (1))



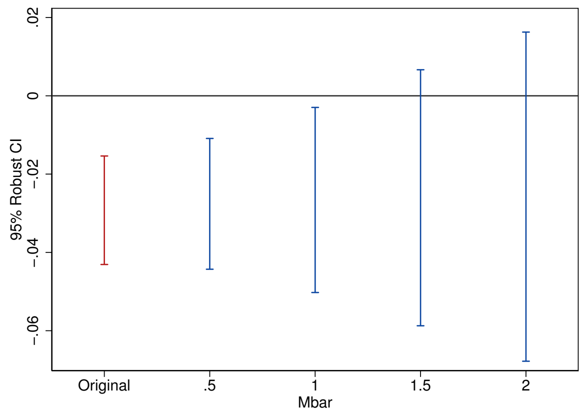
(a) t=1 (b) t=2

Figure 3.3: Allowing for post-treatment violation of parallel trends (b)

(Repeat Table 3.3 Column (2))



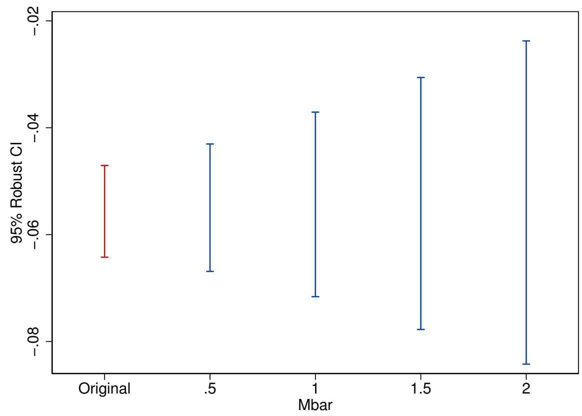
(a) t=1



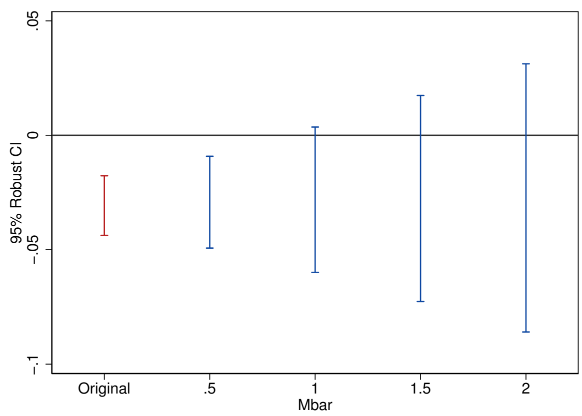
(b) t=2

Figure 3.4: Allowing for post-treatment violation of parallel trends (c)

(Repeat Table 3.3 Column (3))



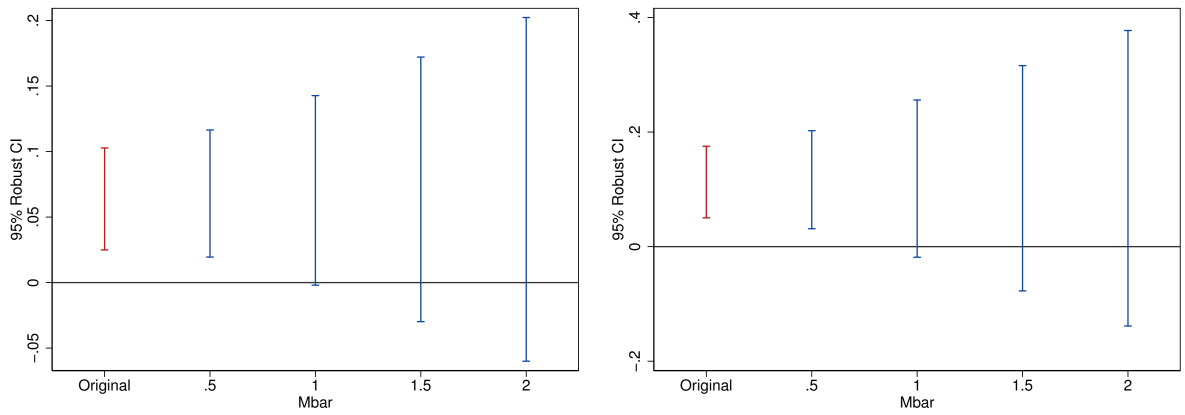
(a) t=1



(b) t=2

Figure 3.5: Allowing for post-treatment violation of parallel trends (d)

(Repeat Table 3.3 Column (4))



(a) t=1

(b) t=2

Figure 3.6: Allowing for post-treatment violation of parallel trends (e)

(Repeat Table 3.3 Column (2))

Note: When applying the Difference-in-Differences method, the implicit assumption is that the control group is parallel with the counterfactual treatment group. However, this assumption may be violated, specifically when spillover effects exist. We re-estimate Table 3.3 and Table 3.7 using the honestdid method proposed by Rambachan & Roth (2023) to allow for post-period violation of parallel trends. The results are plotted in this figure. We see that the "breakdown value" for a significant effect is $M \approx 1$, meaning that the significant result is robust to allowing for violations of parallel trends up to one time as big as the maximum violation in the pre-treatment period. As suggested by Rambachan & Roth (2023), $M = 1$ bounds the worst-case post-treatment difference in trends by the equivalent maximum in the pre-treatment period, which is suggested when equal numbers of pre- and post-years are available.

Table 3.1: Simple difference in mean property prices between the old border districts (treatment) and other non-border districts (control)

	Pre-annoucement (1)	Post announcement (2)	Difference (3)
Panel A: Price			
Treatment group	21,266	29,488	8,222***
	(123.015)	(231.976)	(271.539)
	[1,574]	[1,761]	[3,335]
Control group 1	20,566	30,904	10,338***
	(109.350)	(229.238)	(262.892)
	[2,314]	[2,569]	[4,883]
Difference	700***	-1,416***	-2,116***
	(166.951)	(336.973)	(389.319)
	[3,888]	[4,330]	[8,218]
Control group 2	37,636	55,249	17,613***
	(252.525)	(418.931)	(498.305)
	[4,455]	[4,801]	[9,246]
Difference	-16,369***	-25,761***	-9,392***
	(431.107)	(705.870)	(846.559)
	[6,029]	[6,562]	[12,591]
Control group 3	37,270	54,520	17,249***
	(237.348)	(395.610)	(469.939)
	[4,766]	[5,132]	[9,898]
Difference	-16,004***	-25,032***	-9,028***
	(419.036)	(668.916)	(825.354)
	[6,340]	[6,893]	[13,233]
Control group 4	38,725	56,365	17,640***
	(136.838)	(229.198)	(271.226)
	[10,715]	[11,422]	[22,137]
Difference	-17,458***	-26,877***	-9,419***
	(360.138)	(590.797)	(707.345)
	[12,289]	[13,183]	[25,472]
Panel B: Log(price)			
Treatment group	9.94	10.23	0.30***
	(0.006)	(0.008)	(0.010)
	[1,574]	[1,761]	[3,335]
Control group 1	9.89	10.26	0.37***
	(0.006)	(0.008)	(0.010)
	[2,314]	[2,569]	[4,883]
Difference	0.04 **	-0.03***	-0.07***
	(0.009)	(0.012)	(0.015)
	[3,888]	[4,330]	[8,218]
Control group 2	10.43	10.77	0.34***
	(0.007)	(0.008)	(0.011)
	[4,455]	[4,801]	[9,246]
Difference	-0.49***	-0.54***	-0.05***
	(0.013)	(0.015)	(0.020)
	[6,029]	[6,562]	[12,591]
Control group 3	10.42	10.76	0.34***
	(0.007)	(0.008)	(0.010)
	[4,766]	[5,132]	[9,898]

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	Pre-annoucement (1)	Post announcement (2)	Difference (3)
Difference	-0.48***	-0.53***	-0.04***
	(0.012)	(0.014)	(0.019)
	[6,340]	[6,893]	[13,233]
Control group 4	10.49	10.83	0.35***
	(0.004)	(0.005)	(0.006)
	[10,715]	[11,422]	[22,137]
Difference	-0.55***	-0.60***	-0.05***
	(0.011)	(0.012)	(0.016)
	[12,289]	[13,183]	[25,472]

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) The number of observations in square brackets. (3) Treatment group includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing. (4) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table 3.2: Simple difference in mean property prices between the new districts (treatment) and nearby counties surrounding Beijing (control)

	Pre-annoucement (1)	Post announcement (2)	Difference (3)
Panel A: Price			
Treatment group	15,513	20,915	5,402***
	(128.845)	(263.924)	(297.716)
	[317]	[331]	[648]
Control group 1	8,590	14,533	5,944***
	(138.639)	(329.655)	(418.796)
	[303]	[453]	[756]
Difference	6,924***	6,382***	-542
	(189.050)	(446.823)	(527.018)
	[620]	[784]	[1,404]
Control group 2	5,584	10,522	4,938***
	(86.100)	(264.975)	(355.641)
	[242]	[421]	[663]
Difference	9,929***	10,393***	463
	(165.564)	(379.570)	(463.294)
	[559]	[752]	[1,311]
Control group 3	5,832	9,643	3,811***
	(85.300)	(221.814)	(278.920)
	[258]	[382]	[640]
Difference	9,682***	11,272***	1,590***
	(162.245)	(342.252)	(408.536)
	[575]	[713]	[1,288]
Control group 4	4,364	5,740	1,376***
	(117.995)	(151.794)	(222.790)
	[134]	[244]	[378]
Difference	11,149***	15,175***	4,026***
	(212.581)	(333.931)	(434.651)
	[451]	[575]	[1,026]
Panel B: Log(price)			
Treatment group	9.64	9.92	0.28***
	(0.008)	(0.014)	(0.016)
	[317]	[331]	[648]
Control group 1	9.01	9.46	0.44***
	(0.018)	(0.025)	(0.034)
	[303]	[453]	[756]
Difference	0.63***	0.46***	-0.16***
	(0.020)	(0.031)	(0.039)
	[620]	[784]	[1,404]
Control group 2	8.60	9.12	0.52***
	(0.016)	(0.027)	(0.037)
	[242]	[421]	[663]
Difference	1.04***	0.80***	-0.24***
	(0.017)	(0.032)	(0.040)
	[559]	[752]	[1,311]
Control group 3	8.64	9.07	0.43***
	(0.016)	(0.024)	(0.032)
	[258]	[382]	[640]

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	Pre-annoucement (1)	Post announcement (2)	Difference (3)
Difference	1.00***	0.85***	-0.15***
	(0.017)	(0.029)	(0.036)
	[575]	[713]	[1,288]
Control group 4	8.33	8.58	0.24***
	(0.027)	(0.025)	(0.040)
	[134]	[244]	[378]
Difference	1.31***	1.34***	0.03***
	(0.022)	(0.027)	(0.002)
	[451]	[575]	[1,026]

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior to policy announcement and two years post policy announcement. (2) The number of observations in brackets. (3) Treatment group includes residential complexes in the new districts- Miyun and Yanqing in Beijing. (4) Control groups are residential complexes in the nearby counties surrounding Beijing; Control group 1, 2, 3, and 4 includes residential complexes for which the distance to the boundary of rezoned ares are in first quartile (within 33.9km), second quartile (between 33.9km-91.3km), third quartile (between 91.3km-110.8km), and fourth quartile (between 110.8km-177.6km), respectively.

Table 3.3: The effect of rezoning on property prices in the non-rezoned border districts (treatment) in Beijing

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Treatment group×1-2 year prior announcement	-0.004 (0.005)	0.004 (0.004)	0.003 (0.004)	0.008* (0.004)
Treatment group×0-1 year post announcement	-0.051*** (0.006)	-0.045*** (0.005)	-0.042*** (0.005)	-0.056*** (0.004)
Treatment group×1-2 year post announcement	-0.074*** (0.009)	-0.032*** (0.007)	-0.029*** (0.007)	-0.031*** (0.007)
adj. R^2	0.901	0.965	0.964	0.959
N	8,218	12,591	13,233	25,472

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table 3.4: The variation in the impacts of rezoning with the distance to the boundary of rezoned areas

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Panel A: within 5th percentile distance				
Treatment group×0-1 year post announcement	-0.114***	-0.109***	-0.105***	-0.119***
	(0.017)	(0.017)	(0.016)	(0.016)
Treatment group×1-2 year post announcement	-0.173***	-0.130***	-0.128***	-0.128***
	(0.025)	(0.024)	(0.024)	(0.024)
adj. R^2	0.904	0.965	0.964	0.956
N	3,847	7,145	7,634	16,890
Panel B: within 10th percentile distance				
Treatment group×0-1 year post announcement	-0.101***	-0.096***	-0.093***	-0.106***
	(0.011)	(0.010)	(0.010)	(0.010)
Treatment group×1-2 year post announcement	-0.147***	-0.104***	-0.102***	-0.102***
	(0.018)	(0.017)	(0.017)	(0.017)
adj. R^2	0.904	0.966	0.965	0.957
N	3,961	7,259	7,748	17,004
Panel C: within 15th percentile distance				
Treatment group×0-1 year post announcement	-0.088***	-0.083***	-0.080***	-0.093***
	(0.010)	(0.009)	(0.009)	(0.009)
Treatment group×1-2 year post announcement	-0.123***	-0.081***	-0.078***	-0.079***
	(0.017)	(0.016)	(0.016)	(0.016)
adj. R^2	0.904	0.966	0.965	0.957
N	4,076	7,374	7,863	17,119
Panel D: within 20th percentile distance				
Treatment group×0-1 year post announcement	-0.075***	-0.070***	-0.067***	-0.080***
	(0.009)	(0.008)	(0.008)	(0.008)
Treatment group×1-2 year post announcement	-0.087***	-0.045***	-0.042***	-0.043***
	(0.015)	(0.014)	(0.014)	(0.013)
adj. R^2	0.904	0.966	0.965	0.958
N	4,190	7,488	7,977	17,233

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group in Panel A includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing for which the distance to the boundary of rezoned areas are within 5th percentile (within 8.2km), while it is the 10th percentile (within 10.9km), 15th percentile (within 12.7km), and 20th percentile (within 13.8km) in Panel B, Panel C, and Panel D, respectively. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table 3.5: The effect of rezoning on property prices in the non-rezoned border districts in Beijing

(with five distance categories)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Treatment category 1 × Post announcement	-0.064*** (0.010)	-0.044*** (0.009)	-0.041*** (0.009)	-0.051*** (0.009)
Treatment category 2 × Post announcement	-0.040*** (0.009)	-0.020** (0.009)	-0.017* (0.009)	-0.027*** (0.008)
Treatment category 3 × Post announcement	-0.050*** (0.013)	-0.030** (0.012)	-0.026** (0.012)	-0.036*** (0.012)
Treatment category 4 × Post announcement	-0.071*** (0.010)	-0.051*** (0.009)	-0.047*** (0.009)	-0.057*** (0.009)
Treatment category 5 × Post announcement	-0.077*** (0.010)	-0.057*** (0.009)	-0.054*** (0.009)	-0.064*** (0.009)
adj. R^2	0.901	0.965	0.964	0.959
N	8,218	12,591	13,233	25,472

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Post announcement equals to 1 if after policy announcement (October 2015); equals to 0 otherwise. (3) Treatment category 1, 2, 3, 4, 5 represents for residential complexes in the old border districts (Huairou, Changping, Shunyi, and Pinggu) for which the distance to the boundary of rezoned areas (Miyun and Yanqing) are in first quintile (within 13.8km), second quintile (between 13.8km and 18.4km), third quintile (between 18.4km and 25.4km), fourth quintile (between 25.4km and 30.6km), fifth quintile (between 30.6km and 37.7km), respectively. (4) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table 3.6: The variation in the impacts of rezoning on the non-rezoned border districts (with high and low price properties)

	Control			
	group 1	group 2	group 3	group 4
	(1)	(2)	(3)	(4)
Treatment group (high)×0-1 year post announcement	0.032***	0.032***	0.032***	0.032***
	(0.008)	(0.008)	(0.008)	(0.008)
Treatment group (high)×1-2 year post announcement	0.011	0.011	0.011	0.011
	(0.012)	(0.012)	(0.012)	(0.012)
Treatment group×0-1 year post announcement	-0.061***	-0.066***	-0.057***	-0.071***
	(0.006)	(0.007)	(0.006)	(0.006)
Treatment group×1-2 year post announcement	-0.038***	-0.081***	-0.036***	-0.037***
	(0.009)	(0.011)	(0.009)	(0.009)
adj. R^2	0.964	0.903	0.964	0.959
N	6,312	9,610	10,099	19,355

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group (high) includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing for which the price are higher than the average property prices over the sample period. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table 3.7: The effect of rezoning on property prices in the rezoned areas (treatment) in Beijing

	Distance percentile to the boundary of rezoned areas			
	0-25	25-50	50-75	75-100
	(1)	(2)	(3)	(4)
Treatment group×1-2 year prior announcement	0.037*** (0.014)	0.049*** (0.019)	0.003 (0.014)	0.016 (0.028)
Treatment group×0-1 year post announcement	-0.202*** (0.017)	-0.124*** (0.021)	-0.065*** (0.015)	0.064*** (0.020)
Treatment group×1-2 year post announcement	-0.307*** (0.021)	-0.222*** (0.031)	-0.226*** (0.026)	0.113*** (0.032)
adj. R^2	0.946	0.939	0.954	0.973
N	1,404	1,311	1,288	1,026

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the rezoned areas - Miyun and Yanqing in Beijing. (3) Control groups are residential complexes in nearby counties surrounding Beijing; Control group 1 includes residential complexes in nearby counties for which the distance to the boundary of new districts are in the first quartile (within 33.9km), while Control group 2, 3, and 4 includes residential complexes in nearby counties for which the distance to the boundary of rezoned areas are in second quartile (between 33.9km and 91.3km), third quartile (between 91.3km and 110.8km), and fourth quartile (between 110.8km and 177.6km), respectively.

Table 3.8: The effect of rezoning on property prices in the non-rezoned counties (treatment) surrounding Beijing

	Distance percentile to the boundary of rezoned areas	
	0-25 vs 75-100	0-50 vs 50-100
	(1)	(2)
Treatment group×0-1 year post announcement	0.221***	0.156***
	(0.017)	(0.014)
Treatment group×1-2 year post announcement	0.349***	0.205***
	(0.022)	(0.022)
adj. R^2	0.962	0.931
N	1,418	2,511

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group in column 1 through column 2 includes residential complexes in nearby counties for which the distance to the boundary of rezoned ares are within the first quartile (within 33.9km), the first and second quartile (within 91.3km), respectively. (3) Control groups include residential complexes in nearby counties surrounding Beijing for which the distance to the boundary of new districts are in the fourth quartile (between 110.8km and 177.6km), the third and fourth quartile (between 91.3km and 177.6km), respectively.

Table 3.9: The effect of rezoning on property prices in the non-rezoned counties surrounding Beijing

(with five distance categories)

	(1)
Control category 1 × Post announcement	0.360*** (0.032)
Control category 2 × Post announcement	0.379*** (0.032)
Control category 3 × Post announcement	0.234*** (0.033)
Control category 4 × Post announcement	0.157*** (0.035)
Treatment group × Post announcement	0.091*** (0.027)
adj. R^2	0.938
N	3,085

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Post announcement equals to 1 if after policy announcement (October 2015); equals to 0 otherwise. (3) Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (4) Control category 1, 2, 3, and 4 represents for residential complexes in nearby counties for which the distance to the boundary of rezoned areas are between the first quintile (within 31.2km), the second quintile (between 31.2km and 51.6km), the third quintile (between 51.6km and 97.3km), and the fourth quintile (between 97.3km and 119.9km) respectively. Residential complexes for which the distance are between the fith quintile (between 119.9km and 177.6km) are dropped as the reference group.

Table 3.10: The variation in the impacts of rezoning on the rezoned areas with high and low price properties

	(1)
Treatment group (high) × 0-1 year post announcement	0.024 (0.016)
Treatment group (high) × 1-2 year post announcement	0.042* (0.022)
Treatment group × 0-1 year post announcement	0.045* (0.024)
Treatment group × 1-2 year post announcement	0.088** (0.036)
adj. R^2	0.975
N	804

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group (high) group includes residential complexes in new districts - Miyun and Yanqing for which the price are higher than the average property prices over the sample period. (3) Control group is residential complexes in nearby counties surrounding Beijing for which the distance to the boundary of rezoned ares are between the forth quartile (between 110.8km and 177.6km).

A3.A Robustness check A

Table A3.A.1: The effect of rezoning on property prices in the non-rezoned border districts (treatment) in Beijing
(Repeat Table 3 with method following Callaway & Sant'Anna (2021))

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Pre average	0.003 (0.004)	-0.008** (0.004)	-0.007* (0.004)	-0.009** (0.004)
Post average	-0.063*** (0.006)	-0.039*** (0.005)	-0.036*** (0.005)	-0.044*** (0.005)
Treatment group×1-2 year prior announcement	0.003 (0.004)	-0.008** (0.004)	-0.007* (0.004)	-0.009** (0.004)
Treatment group×0-1 year post announcement	-0.051*** (0.006)	-0.045*** (0.005)	-0.042*** (0.005)	-0.057*** (0.004)
Treatment group×1-2 year post announcement	-0.076*** (0.009)	-0.034*** (0.007)	-0.031*** (0.007)	-0.031*** (0.007)
N	7,747	11,954	12,565	24,244

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3. (4) The number of observations is different from Table 2 because panel is not balanced, observations with Pair balanced (observed at both pre and post announcement) are automatically used with CS-DiD. (4)The Stata command used was csdid.

Table A3.A.2: The effect of rezoning on property prices in the non-rezoned border districts (treatment) in Beijing
(Repeat Table 3 with method following Wooldridge (2021))

	Control			
	group 1	group 2	group 3	group 4
	(1)	(2)	(3)	(4)
_tr_×first_treat×year				
0-1 year 0-1 year	-0.054***	-0.051***	-0.048***	-0.064***
post announcement	(0.006)	(0.005)	(0.005)	(0.004)
0-1 year 1-2 year	-0.068***	-0.029***	-0.026***	-0.029***
post announcement	(0.008)	(0.007)	(0.007)	(0.006)
Constant	10.101***	10.478***	10.480***	10.597***
	(0.001)	(0.001)	(0.001)	(0.000)
adj. R^2	0.902	0.965	0.965	0.959
N	8,096	12,469	13,111	25,350

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3. (4)The Stata command used was jwdid.

Table A3.A.3: The variation in the impacts of rezoning with the distance to the boundary of rezoned areas (Repeat Table 3 with quarterly frequency)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Treatment group×7-8 quarter prior announcement	0.037 (0.039)	0.015 (0.035)	0.012 (0.034)	0.019 (0.029)
Treatment group×6-7 quarter prior announcement	0.017 (0.041)	0.018 (0.034)	0.015 (0.033)	0.029 (0.028)
Treatment group×5-6 quarter prior announcement	0.005 (0.033)	0.021 (0.028)	0.019 (0.027)	0.028 (0.020)
Treatment group×4-5 quarter prior announcement	-0.000 (0.032)	0.017 (0.023)	0.017 (0.022)	0.026 (0.018)
Treatment group×3-4 quarter prior announcement	0.015 (0.030)	0.019 (0.019)	0.018 (0.019)	0.028* (0.016)
Treatment group×2-3 quarter prior announcement	0.021 (0.027)	0.018 (0.017)	0.016 (0.017)	0.025 (0.016)
Treatment group×1-2 quarter prior announcement	0.020 (0.022)	0.015 (0.014)	0.014 (0.014)	0.018 (0.014)
Treatment group×0-1 quarter post announcement	-0.018 (0.014)	-0.016 (0.015)	-0.015 (0.014)	-0.015 (0.012)
Treatment group×1-2 quarter post announcement	-0.031 (0.033)	-0.025 (0.028)	-0.024 (0.027)	-0.033 (0.021)
Treatment group×2-3 quarter post announcement	-0.042 (0.046)	-0.041 (0.037)	-0.038 (0.035)	-0.057** (0.027)
Treatment group×3-4 quarter post announcement	-0.043 (0.051)	-0.033 (0.033)	-0.029 (0.032)	-0.046* (0.025)
Treatment group×4-5 quarter post announcement	-0.053 (0.047)	-0.018 (0.025)	-0.017 (0.024)	-0.032 (0.022)
Treatment group×5-6 quarter post announcement	-0.053*** (0.016)	-0.026* (0.016)	-0.025 (0.016)	-0.020 (0.015)
Treatment group×6-7 quarter post announcement	-0.061** (0.028)	-0.026 (0.021)	-0.025 (0.020)	-0.006 (0.016)
Treatment group×7-8 quarter post announcement	-0.060* (0.030)	0.011 (0.027)	0.013 (0.025)	0.027 (0.018)
Constant	10.102*** (0.008)	10.464*** (0.005)	10.466*** (0.005)	10.592*** (0.003)
adj. R^2	0.910	0.967	0.966	0.962
N	29,390	44,602	46,855	90,420

Note: (1) The dataset is unbalanced quarterly data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table A3.A.4: The variation in the impacts of rezoning with the distance to the boundary of rezoned areas
(Repeat Table 4 with covariates)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Panel A: within 5^{th} percentile distance				
Treatment group×0-1 year	-0.026	-0.150***	-0.133***	-0.110***
post announcement	(0.021)	(0.017)	(0.017)	(0.016)
Treatment group×1-2 year	-0.118***	-0.244***	-0.200***	-0.140***
post announcement	(0.035)	(0.027)	(0.026)	(0.024)
Covariates	Yes	Yes	Yes	Yes
adj. R^2	0.904	0.965	0.964	0.956
N	3,847	7,145	7,634	16,890
Panel B: within 10^{th} percentile distance				
Treatment group×0-1 year	-0.002	-0.118***	-0.108***	-0.096***
post announcement	(0.015)	(0.012)	(0.012)	(0.010)
Treatment group×1-2 year	-0.057**	-0.165***	-0.139***	-0.109***
post announcement	(0.029)	(0.028)	(0.024)	(0.018)
Covariates	Yes	Yes	Yes	Yes
adj. R^2	0.904	0.966	0.965	0.957
N	3,961	7,259	7,748	17,004
Panel C: within 15^{th} percentile distance				
Treatment group×0-1 year	0.006	-0.089***	-0.085***	-0.082***
post announcement	(0.011)	(0.010)	(0.010)	(0.009)
Treatment group×1-2 year	-0.041**	-0.091***	-0.081***	-0.077***
post announcement	(0.020)	(0.020)	(0.019)	(0.016)
Covariates	Yes	Yes	Yes	Yes
adj. R^2	0.904	0.966	0.965	0.957
N	4,076	7,374	7,863	17,119
Panel D: within 20^{th} percentile distance				
Treatment group×0-1 year	0.017*	-0.071***	-0.068***	-0.067***
post announcement	(0.009)	(0.009)	(0.009)	(0.008)
Treatment group×1-2 year	-0.007	-0.039**	-0.034**	-0.036***
post announcement	(0.015)	(0.015)	(0.015)	(0.014)
Covariates	Yes	Yes	Yes	Yes
adj. R^2	0.904	0.966	0.965	0.958
N	4,190	7,488	7,977	17,233

Note: (1) The dataset is unbalanced quarterly data, centered around October 2015 (policy announcement month). The sample period is 8 quarters pre- and post-policy announcement. (2) Treatment group in Panel A, B, C, and D includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing for which the distance to the boundary of new districts are within 5^{th}, 10^{th}, 15^{th}, 20^{th} percentile, respectively. (3) Control groups are residential complexes in other districts in Beijing, the same as in Table A1.

Table A3.A.5: The effect of rezoning on property prices in the non-rezoned border districts in Beijing (Repeat Table 5 with four distance categries)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Treatment category 1 × Post announcement	-0.060*** (0.009)	-0.039*** (0.009)	-0.036*** (0.008)	-0.046*** (0.008)
Treatment category 2 × Post announcement	-0.024*** (0.009)	-0.004 (0.008)	-0.000 (0.008)	-0.010 (0.008)
Treatment category 3 × Post announcement	-0.090*** (0.011)	-0.069*** (0.010)	-0.066*** (0.010)	-0.076*** (0.010)
Treatment category 4 × Post announcement	-0.072*** (0.009)	-0.052*** (0.008)	-0.049*** (0.008)	-0.059*** (0.008)
adj. R^2	0.901	0.965	0.964	0.959
N	8,218	12,591	13,233	25,472

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Post announcement equals to 1 if after policy announcement (October 2015); equals to 0 otherwise. (3) Treatment category 1, 2, 3, 4 represents for residential complexes in the old border districts (Huairou, Changping, Shunyi, and Pinggu) for which the distance to the boundary of new districts (Miyun and Yanqing) are within the first quartile (within 14.6km), the second quartile (between 14.6km and 20.8km), the third quartile (between 20.8km and 29.8km), the fourth quartile (between 29.8km and 37.7km), respectively. (4) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table A3.A.6: The effect of rezoning on property prices in the non-rezoned border districts in Beijing, with five distance categries
(Repeat Table 5 with separate post dummies)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Treatment category 1 × 0-1 year post announcement	-0.061*** (0.009)	-0.058*** (0.008)	-0.054*** (0.008)	-0.071*** (0.008)
Treatment category 1 × 1-2 year post announcement	-0.064*** (0.014)	-0.025* (0.014)	-0.022 (0.013)	-0.026* (0.013)
Treatment category 2 × 0-1 year post announcement	-0.030*** (0.009)	-0.028*** (0.008)	-0.024*** (0.008)	-0.041*** (0.008)
Treatment category 2 × 1-2 year post announcement	-0.043*** (0.012)	-0.004 (0.011)	-0.001 (0.011)	-0.005 (0.011)
Treatment category 3 × 0-1 year post announcement	-0.031*** (0.012)	-0.029** (0.011)	-0.025** (0.011)	-0.041*** (0.011)
Treatment category 3 × 1-2 year post announcement	-0.076*** (0.016)	-0.037** (0.016)	-0.034** (0.016)	-0.038** (0.015)
Treatment category 4 × 0-1 year post announcement	-0.068*** (0.010)	-0.066*** (0.009)	-0.062*** (0.009)	-0.078*** (0.009)
Treatment category 4 × 1-2 year post announcement	-0.086*** (0.014)	-0.048*** (0.013)	-0.044*** (0.013)	-0.048*** (0.013)
Treatment category 5 × 0-1 year post announcement	-0.055*** (0.010)	-0.052*** (0.009)	-0.049*** (0.009)	-0.065*** (0.009)
Treatment category 5 × 1-2 year post announcement	-0.093*** (0.014)	-0.054*** (0.013)	-0.051*** (0.013)	-0.055*** (0.013)
adj. R^2	0.901	0.965	0.964	0.959
N	8,218	12,591	13,233	25,472

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment category 1, 2, 3, 4, 5 represents for residential complexes in the old border districts (Huairou, Changping, Shunyi, and Pinggu) for which the distance to the boundary of new districts (Miyun and Yanqing) are within the first, second, third, and fourth quartile, respectively. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table A3.A.7: The variation in the impacts of rezoning on the non-rezoned border districts with high and low price properties
(Repeat Table 6 with covariates)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Treatment group (high) × 0-1 year post announcement	0.028***	-0.026***	-0.024***	-0.026***
	(0.008)	(0.006)	(0.006)	(0.006)
Treatment group (high) × 1-2 year post announcement	-0.023**	-0.017	-0.016	-0.014
	(0.011)	(0.011)	(0.010)	(0.009)
Treatment group (low) × 0-1 year post announcement	-0.005	-0.058***	-0.056***	-0.059***
	(0.007)	(0.006)	(0.006)	(0.006)
Treatment group (low) × 1-2 year post announcement	-0.034***	-0.028***	-0.027**	-0.025***
	(0.011)	(0.011)	(0.011)	(0.009)
Log(GRP per capita)	-0.152	0.399	0.249	-0.032
	(0.259)	(0.262)	(0.244)	(0.097)
Log(local government general public budget revenue)	1.468***	-0.063*	-0.097***	-0.157***
	(0.138)	(0.037)	(0.034)	(0.034)
Log(local government general public budget expenditure)	0.237***	-0.038	-0.045	0.132***
	(0.017)	(0.029)	(0.029)	(0.016)
Constant	-11.287***	7.232**	9.589***	11.304***
	(3.578)	(3.073)	(2.787)	(1.132)
adj. R^2	0.909	0.964	0.964	0.960
N	6,312	9,610	10,099	19,355

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group (high) includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing for which the price are higher than the average property prices over the sample period, while Treatment (low) includes those residential complexes for which the price are lower than average. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table A3.A.8: The effect of rezoning on the property prices in the rezoned areas (treatment) in Beijing
(Repeat Table 7 with method following Callaway & Sant'Anna (2021))

	Distance percentile to the boundary of rezoned areas			
	0-25 (1)	25-50 (2)	50-75 (3)	75-100 (4)
Pre average	-0.012 (0.009)	-0.039*** (0.012)	0.001 (0.011)	-0.038 (0.024)
Post average	-0.260*** (0.018)	-0.174*** (0.025)	-0.144*** (0.020)	0.086*** (0.025)
Treatment group×1-2 year prior announcement	-0.012 (0.009)	-0.039*** (0.012)	0.001 (0.011)	-0.038 (0.024)
Treatment group×0-1 year post announcement	-0.200*** (0.016)	-0.112*** (0.019)	-0.047*** (0.013)	0.056*** (0.018)
Treatment group×1-2 year post announcement	-0.320*** (0.021)	-0.236*** (0.033)	-0.241*** (0.028)	0.115*** (0.037)
N	1,257	1,163	1,160	896

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (3) Control groups are residential complexes in nearby counties surrounding Beijing; Control group 1 includes residential complexes in nearby counties for which the distance to the boundary of new districts are within the first quartile (within 33.9km), while Control group 2, 3, and 4 includes residential complexes in nearby counties for which the distance to the boundary of new districts are within the second quartile (between 33.9km and 91.3km), the third quartile (between 91.3km and 110.8km), and the fourth quartile (between 110.8km and 177.6km), respectively. (4) The number of observations is different from Table 7 because panel is not balanced, observations with Pair balanced (observed at both pre and post announcement) are automatically used with CS-DiD. (5)The Stata command used was csdid.

TABLE A3.A.9: The effect of rezoning on the property prices in the rezoned areas (treatment) in Beijing
(Repeat Table 7 with method following Wooldridge (2021))

	Distance percentile to the boundary of rezoned areas	
	50-75	75-100
	(1)	(2)
_tr_×first_treat×year		
0-1 year 0-1 year post announcement	-0.067***	0.057***
	(0.015)	(0.020)
0-1 year 1-2 year post announcement	-0.227***	0.107***
	(0.025)	(0.032)
Constant	9.365***	9.195***
	(0.004)	(0.007)
adj. R^2	0.954	0.973
N	1,409	1,208

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (3) Control groups are residential complexes in nearby counties surrounding Beijing; Control group 1 and 2 includes residential complexes in nearby counties for which the distance to the boundary of new districts are within the third quartile (between 91.3km and 110.8km), and the fourth quartile (between 110.8km and 177.6km), respectively. (4)The Stata command used was jwdid.

Table A3.A.10: The effect of rezoning on the property prices in the rezoned areas (treatment) in Beijing (Repeat Table 7 with quarterly data)

	Distance percentile to the boundary of rezoned areas			
	0-25	25-50	50-75	75-100
	(1)	(2)	(3)	(4)
Treatment group×7-8 quarter prior announcement	0.057	-0.049	-0.028	-0.021
	(0.050)	(0.088)	(0.066)	(0.076)
Treatment group×6-7 quarter prior announcement	0.086*	-0.034	0.011	-0.001
	(0.049)	(0.090)	(0.067)	(0.082)
Treatment group×5-6 quarter prior announcement	0.095**	0.022	0.013	0.008
	(0.043)	(0.087)	(0.062)	(0.066)
Treatment group×4-5 quarter prior announcement	0.094*	0.077	0.002	-0.012
	(0.053)	(0.093)	(0.084)	(0.064)
Treatment group×3-4 quarter prior announcement	0.098	0.006	0.030	-0.002
	(0.066)	(0.101)	(0.066)	(0.056)
Treatment group×2-3 quarter prior announcement	0.085	0.036	0.018	0.008
	(0.069)	(0.090)	(0.078)	(0.069)
Treatment group×1-2 quarter prior announcement	0.073	0.015	0.020	0.035
	(0.065)	(0.094)	(0.067)	(0.070)
Treatment group×0-1 quarter post announcement	-0.063	-0.057	0.007	0.003
	(0.052)	(0.087)	(0.072)	(0.062)
Treatment group×1-2 quarter post announcement	-0.111***	-0.059	0.045	0.032
	(0.040)	(0.076)	(0.063)	(0.059)
Treatment group×2-3 quarter post announcement	-0.156***	-0.112	-0.007	0.080
	(0.052)	(0.069)	(0.053)	(0.059)
Treatment group×3-4 quarter post announcement	-0.245***	-0.222**	-0.148**	0.126**
	(0.072)	(0.087)	(0.060)	(0.056)
Treatment group×4-5 quarter post announcement	-0.311***	-0.289***	-0.254***	0.102
	(0.063)	(0.101)	(0.085)	(0.065)
Treatment group×5-6 quarter post announcement	-0.308***	-0.281***	-0.254***	0.152**
	(0.060)	(0.095)	(0.090)	(0.063)
Treatment group×6-7 quarter post announcement	-0.271***	-0.256***	-0.254***	0.091
	(0.049)	(0.093)	(0.086)	(0.075)
Treatment group×7-8 quarter post announcement	-0.214***	-0.182**	-0.179**	0.092
	(0.044)	(0.075)	(0.069)	(0.066)
Constant	9.571***	9.468***	9.433***	9.376***
	(0.018)	(0.034)	(0.027)	(0.034)
adj. R^2	0.950	0.947	0.955	0.972
N	5,070	4,563	4,541	3,560

Note: (1) The dataset is unbalanced quarterly data, centered around October 2015 (policy announcement month). The sample period is eight quarters prior policy announcement and eight quarters post policy announcement. (2) Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (3) Control groups are residential complexes in nearby counties surrounding Beijing; Control group 1 and 2 includes residential complexes in nearby counties for which the distance to the boundary of new districts are within the third quartile (between 91.3km and 110.8km), and the fourth quartile (between 110.8km and 177.6km), respectively.

Table A3.A.11: How rezoning affects rezoned areas in Beijing
(Repeat Table 7 with covariates)

	Distance percentile to the boundary of rezoned areas			
	0-25	25-50	50-75	75-100
	(1)	(2)	(3)	(4)
Treatment group×1-2 year prior announcement	0.049	-0.065**	-0.155***	0.005
	(0.036)	(0.027)	(0.022)	(0.033)
Treatment group×0-1 year post announcement	-0.151***	-0.117***	0.082***	0.084***
	(0.025)	(0.020)	(0.019)	(0.021)
Treatment group×1-2 year post announcement	-0.143**	-0.294***	0.017	0.105***
	(0.059)	(0.027)	(0.031)	(0.032)
Log(GRP per capita)	-0.605**	0.859***	1.142***	0.206
	(0.251)	(0.117)	(0.172)	(0.321)
Log(local government general public budget revenue)	-0.005	0.401***	-0.169***	0.233**
	(0.097)	(0.075)	(0.055)	(0.115)
Log(local government general public budget expenditure)	-0.119*	0.008	0.639***	0.230
	(0.067)	(0.111)	(0.067)	(0.153)
Constant	17.820***	-4.852***	-9.254***	1.215
	(2.738)	(1.064)	(1.771)	(2.768)
adj. R^2	0.948	0.955	0.970	0.974
N	1,404	1,311	1,288	1,026

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (3) Control groups are residential complexes in nearby counties surrounding Beijing; Control group 1 includes residential complexes in non-rezoned surrounding counties for which the distance to the boundary of rezoned ares are within the first quartile, while Control group 2, 3, and 4 includes residential complexes in nearby counties for which the distance to the boundary of rezoned areas are between the second quartile, the third quartile, and the fourth quartile, respectively.

Table A3.A.12: The effect of rezoning on property prices in the non-rezoned counties surrounding Beijing
(Repeat Table 9 with four distance categories)

	(1)
Control category 1 × Post announcement	0.349*** (0.027)
Control category 2 × Post announcement	0.270*** (0.033)
Control category 3 × Post announcement	0.224*** (0.028)
Treatment group × Post announcement	0.077*** (0.024)
adj. R^2	0.937
N	3,085

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Post announcement equals to 1 if after policy announcement (October 2015); equals to 0 otherwise. (3) Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (4) Control category 1, 2, and 3 represents for residential complexes in nearby counties for which the distance to the boundary of new districts are between the first quartile (within 33.9km), the second quartile (between 33.9km and 91.3km), and the third quartile (between 91.3km and 110.8km), respectively. Residential complexes for which the distance are between the fourth quartile (between 110.8km and 177.6km) are dropped as the reference group.

Table A3.A.13: The effect of rezoning on property prices in the non-rezoned counties surrounding Beijing, with five distance categories
(Repeat Table 9 with separate post dummies)

	(1)
Control category 1 × 0-1 year post announcement	-0.061*** (0.009)
Control category 1 × 1-2 year post announcement	-0.064*** (0.014)
Control category 2 × 0-1 year post announcement	-0.030*** (0.009)
Control category 2 × 1-2 year post announcement	-0.043*** (0.012)
Control category 3 × 0-1 year post announcement	-0.031*** (0.012)
Control category 3 × 1-2 year post announcement	-0.076*** (0.016)
Control category 4 × 0-1 year post announcement	-0.068*** (0.010)
Control category 4 × 1-2 year post announcement	-0.086*** (0.014)
Treatment group × 0-1 year post announcement	-0.055*** (0.010)
Treatment group × 1-2 year post announcement	-0.093*** (0.014)
adj. R^2	0.901
N	8218

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2)Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (3) Control category 1, 2, and 3 represents for residential complexes in nearby counties for which the distance to the boundary of new districts are between the first quartile (within 33.9km), the second quartile (between 33.9km and 91.3km), and the third quartile (between 91.3km and 110.8km), respectively. Residential complexes for which the distance are between the fourth quartile (between 110.8km and 177.6km) are dropped as the reference group.

Table A3.A.14: The variation in the impacts of rezoning on the rezoned areas with high and low price properties
(Repeat Table 10 with covariates)

	(1)
Treatment group (high) × 0-1 year post announcement	0.086*** (0.022)
Treatment group (high) × 1-2 year post announcement	0.130*** (0.032)
Treatment group (low) × 0-1 year post announcement	0.063** (0.026)
Treatment group (low) × 1-2 year post announcement	0.090** (0.035)
Log(GRP per capita)	-0.047 (0.491)
Log(local government general public budget revenue)	0.266** (0.132)
Log(local government general public budget expenditure)	0.180 (0.186)
Constant	4.164 (5.163)
adj. R^2	0.975
N	804

Note: (1) The dataset is unbalanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group (high) group includes residential complexes in new districts - Miyun and Yanqing for which the price are higher than the average property prices over the sample period, while treatment (low) group includes residential complexes with low price. (3) Control group is residential complexes in nearby counties surrounding Beijing for which the distance to the boundary of new districts are between between the fourth quartile (between 110.8km and 177.6km).

A3.B Robustness Check B

Table A3.B.1: The effect of rezoning on property prices in the non-rezoned border districts (treatment) in Beijing
(Repeat Table 3 with balanced panel)

	Control			
	group 1	group 2	group 3	group 4
	(1)	(2)	(3)	(4)
Treatment group×1-2 year prior announcement	-0.005	0.007*	0.005	0.007**
	(0.004)	(0.004)	(0.004)	(0.003)
Treatment group×0-1 year post announcement	-0.049***	-0.042***	-0.039***	-0.057***
	(0.006)	(0.004)	(0.004)	(0.004)
Treatment group×1-2 year post announcement	-0.071***	-0.027***	-0.024***	-0.025***
	(0.008)	(0.007)	(0.007)	(0.006)
Constant	10.106***	10.473***	10.475***	10.600***
	(0.001)	(0.001)	(0.001)	(0.000)
adj. R^2	0.919	0.972	0.972	0.968
N	7,012	10,908	11,452	22,196

Note: (1) The dataset is balanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

TABLE A3.B.2: The variation in the impacts of rezoning with the distance to the boundary of rezoned areas
(Repeat Table 4 with balanced panel)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Panel A: within 5th percentile distance				
Treatment group×0-1 year post announcement	-0.102***	-0.095***	-0.092***	-0.110***
	(0.009)	(0.008)	(0.008)	(0.008)
Treatment group×1-2 year post announcement	-0.167***	-0.123***	-0.120***	-0.121***
	(0.018)	(0.018)	(0.018)	(0.017)
adj. R^2	0.920	0.973	0.973	0.966
N	3,252	6,174	6,582	14,640
Panel B: within 10th percentile distance				
Treatment group×0-1 year post announcement	-0.091***	-0.084***	-0.081***	-0.099***
	(0.009)	(0.008)	(0.008)	(0.008)
Treatment group×1-2 year post announcement	-0.162***	-0.118***	-0.116***	-0.117***
	(0.017)	(0.016)	(0.016)	(0.016)
adj. R^2	0.919	0.973	0.973	0.966
N	3,357	6,279	6,687	14,745
Panel C: within 15th percentile distance				
Treatment group×0-1 year post announcement	-0.089***	-0.082***	-0.078***	-0.096***
	(0.009)	(0.008)	(0.008)	(0.007)
Treatment group×1-2 year post announcement	-0.117***	-0.073***	-0.071***	-0.072***
	(0.016)	(0.015)	(0.015)	(0.015)
adj. R^2	0.919	0.973	0.973	0.967
N	3,462	6,384	6,792	14,850
Panel D: within 20th percentile distance				
Treatment group×0-1 year post announcement	-0.076***	-0.069***	-0.066***	-0.084***
	(0.008)	(0.007)	(0.007)	(0.007)
Treatment group×1-2 year post announcement	-0.095***	-0.051***	-0.049***	-0.050***
	(0.015)	(0.014)	(0.014)	(0.014)
adj. R^2	0.918	0.973	0.973	0.967
N	3,567	6,489	6,897	14,955

Note: (1) The dataset is balanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group in Panel A includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing for which the distance to the boundary of new districts are within 5th percentile (within 8.2km), while it is the 10th percentile (within 10.9km), 15th percentile (within 12.7km), and 20th percentile (within 13.8km) in Panel B, Panel C, and Panel D, respectively. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table A3.B.3: The effect of rezoning on property prices in the non-rezoned border districts in Beijing, with five distance categories
(Repeat Table 5 with balanced panel)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Treatment category 1 × Post announcement	-0.069*** (0.010)	-0.049*** (0.009)	-0.046*** (0.009)	-0.056*** (0.009)
Treatment category 2 × Post announcement	-0.045*** (0.009)	-0.025*** (0.008)	-0.022*** (0.008)	-0.032*** (0.008)
Treatment category 3 × Post announcement	-0.034*** (0.011)	-0.015 (0.010)	-0.011 (0.010)	-0.022** (0.010)
Treatment category 4 × Post announcement	-0.067*** (0.010)	-0.048*** (0.009)	-0.044*** (0.009)	-0.055*** (0.009)
Treatment category 5 × Post announcement	-0.072*** (0.010)	-0.052*** (0.009)	-0.049*** (0.009)	-0.059*** (0.009)
Constant	10.105*** (0.001)	10.474*** (0.001)	10.476*** (0.001)	10.600*** (0.000)
adj. R^2	0.920	0.972	0.972	0.968
N	7,012	10,908	11,452	22,196

Note: (1) The dataset is balanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Post announcement equals to 1 if after policy announcement (October 2015); equals to 0 otherwise. (3) Treatment category 1, 2, 3, 4, 5 represents for residential complexes in the old border districts (Huairou, Changping, Shunyi, and Pinggu) for which the distance to the boundary of new districts (Miyun and Yanqing) are between the first quintile (within 13.8km), the second quintile (between 13.8km and 18.4km), the third quintile (between 18.4km and 25.4km), the fourth quintile (between 25.4km and 30.6km), the fifth quintile (between 30.6km and 37.7km), respectively. (4) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table A3.B.4: The variation in the impacts of rezoning on the non-rezoned border districts with high and low price properties
(Repeat Table 6 with balanced panel)

	Control			
	group 1 (1)	group 2 (2)	group 3 (3)	group 4 (4)
Treatment group (high) × 0-1 year post announcement	-0.033***	-0.026***	-0.023***	-0.041***
	(0.007)	(0.005)	(0.005)	(0.005)
Treatment group (high) × 1-2 year post announcement	-0.064***	-0.020**	-0.017**	-0.018**
	(0.010)	(0.008)	(0.008)	(0.008)
Treatment group (low) × 0-1 year post announcement	-0.064***	-0.057***	-0.054***	-0.072***
	(0.007)	(0.005)	(0.005)	(0.005)
Treatment group (low) × 1-2 year post announcement	-0.078***	-0.034***	-0.031***	-0.032***
	(0.010)	(0.009)	(0.009)	(0.009)
Constant	10.160***	10.526***	10.527***	10.654***
	(0.002)	(0.001)	(0.001)	(0.000)
adj. R^2	0.923	0.973	0.972	0.968
N	5,259	8,181	8,589	16,647

Note: (1) The dataset is balanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group (high) includes residential complexes in the old border districts (including Huairou, Changping, Shunyi, and Pinggu) in Beijing for which the price are higher than the average property prices over the sample period, while Treatment (low) includes those residential complexes for which the price are lower than average. (3) Control groups are residential complexes in other districts in Beijing: Control group 1 includes Mentougou, Fangshan, Daxing, and Tongzhou districts; Control group 2 includes Fangshan, Daxing, Fengtai, Dongcheng, and Xicheng; Control group 3 includes all districts in Control group 2 and Shijingshan; Control group 4 includes all districts in Control group 1, 2, and 3.

Table A3.B.5 The effect of rezoning on property prices in the rezoned areas (treatment) in Beijing (Repeat Table 7 with balanced panel)

	Distance percentile to the boundary of rezoned areas			
	0-25	25-50	50-75	75-100
	(1)	(2)	(3)	(4)
Treatment group×1-2 year prior announcement	0.012	0.030***	0.007	0.017
	(0.012)	(0.010)	(0.011)	(0.016)
Treatment group×0-1 year post announcement	-0.235***	-0.208***	-0.061***	0.022
	(0.020)	(0.017)	(0.016)	(0.016)
Treatment group×1-2 year post announcement	-0.365***	-0.390***	-0.252***	0.009
	(0.025)	(0.028)	(0.034)	(0.040)
Constant	9.686***	9.654***	9.519***	9.337***
	(0.008)	(0.008)	(0.008)	(0.010)
adj. R^2	0.956	0.962	0.957	0.964
N	948	948	948	944

Note: (1) The dataset is balanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (3) Control groups are residential complexes in nearby counties surrounding Beijing; Control group 1 includes residential complexes in nearby counties for which the distance to the boundary of new ditricts are within the first quartile (within 33.9km), while Control group 2, 3, and 4 includes residential complexes in surround non-rezoned counties for which the distance to the boundary of rezoned areas are within the second quartile (between 33.9km and 91.3km), the third quartile (between 91.3km and 110.8km), and the fourth quartile (between 110.8km and 177.6km), respectively.

Table A3.B.6: The effect of rezoning on property prices in the non-rezoned counties (treatment) surrounding Beijing
(Repeat Table 8 with balanced panel)

	Distance percentile to the boundary of rezoned areas	
	0-25 vs 75-100	0-50 vs 50-100
	(1)	(2)
Treatment group×0-1 year post announcement	0.211***	0.243***
	(0.014)	(0.020)
Treatment group×1-2 year post announcement	0.312***	0.369***
	(0.024)	(0.028)
Constant	9.243***	9.331***
	(0.004)	(0.004)
adj. R^2	0.944	0.959
N	1,491	969

Note: (1) The dataset is balanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group in column 1 through column 2 includes residential complexes in nearby counties for which the distance to the boundary of new districts are within the first quartile (within 33.9km), the first and the second quartile (within 91.3km), respectively. (3) Control groups include residential complexes in nearby counties surrounding Beijing for which the distance to the boundary of new districts are between the fourth quartile (between 110.8km and 177.6km), the third and the fourth quartile (between 91.3km and 177.6km), respectively.

Table A3.B.7: The effect of rezoning on property prices in the non-rezoned counties surrounding Beijing, with five distance categories
(Repeat Table 9 with balanced panel)

	(1)
Control category 1 × Post announcement	0.342*** (0.036)
Control category 2 × Post announcement	0.407*** (0.030)
Control category 3 × Post announcement	0.200*** (0.042)
Control category 4 × Post announcement	0.258*** (0.031)
Treatment group × Post announcement	0.047* (0.027)
Constant	9.141*** (0.012)
adj. R^2	0.952
N	1,988

Note: (1) The dataset is balanced annual data, centered around October 2015 (policy announcement month). The sample period is two years prior policy announcement and two years post policy announcement. (2) Post announcement equals to 1 if after policy announcement (October 2015); equals to 0 otherwise. (3) Treatment group includes residential complexes in the new districts - Miyun and Yanqing in Beijing. (4) Control category 1, 2, 3, and 4 represents for residential complexes in nearby counties for which the distance to the boundary of new districts are within the first quintile (within 31.2km), the second quintile (between 31.2km and 51.6km), the third quintile (between 51.6km and 97.3km), and the fourth quintile (between 97.3km and 119.9km) respectively. Residential complexes for which the distance are between the fifth quintile (between 119.9km and 177.6km) are dropped as the reference group.

TABLE A3.B.8: The variation in the impacts of rezoning on the rezoned areas with high and low price properties
(Repeat Table 10 with balanced panel)

	(1)
Treatment group (high) × 0-1 year post announcement	0.038** (0.017)
Treatment group (high) × 1-2 year post announcement	0.029 (0.040)
Treatment group (low) × 0-1 year post announcement	0.000 (0.018)
Treatment group (low) × 1-2 year post announcement	-0.019 (0.043)
Constant	9.385*** (0.011)
adj. R^2	0.962
N	708

Note: (1) The dataset is balanced annual data, centered around October 2015 (policy announcement month). The sample period is one year prior policy announcement and two years post policy announcement. (2) Treatment group (high) group includes residential complexes in new districts - Miyun and Yanqing for which the price are higher than the average property prices over the sample period, while treatment (low) group includes residential complexes with low price. (3) Control group is residential complexes in nearby counties surrounding Beijing for which the distance to the boundary of new districts are within the fourth quartile (between 110.8km and 177.6km).

Table A3.B.9: Population in rezoned and non-rezoned border districts

	Population			
	2014	2015	2016	2017
	(1)	(2)	(3)	(4)
Shunyi	1,004,000	1,020,000	1,075,000	1,128,000
Changping	1,908,000	1,963,000	2,010,000	2,063,000
Huairou	381,000	384,000	393,000	405,000
Pinggu	423,000	423,000	437,000	448,000
Miyun	478,000	479,00	493,000	490,000
Yanqing	316,000	314,000	327,000	340,000

CHAPTER 4 Snowball effect of the US-China trade war: Evidence from China's commercial building markets

Abstract

We study the impact of the US-China tariff war on China's 34 large cities' commercial building rents. By utilizing Bartik-style tariff exposures, we find a 1 percentage point increase in the US tariff exposure proxy is associated with 1 percent decrease in commercial building rent growth after one quarter, ceteris paribus. We also identify heterogeneity in rent response, with cities having high US dependence amplifying the negative impact and better financial situations, social stability, innovation and location mitigating it. Moreover, US and China tariff exposures influence rents through distinct mechanisms. Specifically, US tariff exposure subtly impacts rent growth via US dependence, social stability, and geographical positioning, whereas China's tariff exposure primarily does so through financial health and innovation. Robustness checks confirm the stability of our findings. This research provides insights for policymakers, suggesting

maintaining a better financial situation, a stable society and encouraging innovation to keep the commercial real estate rental market stable under such unexpected shocks.

4.1 Introduction

The US-China trade war, initiated by the United States in 2018, has been a focal point of recent global policy shifts. By 2020, the United States had imposed tariffs on over $360 billion of Chinese imports, with China retaliating on $100 billion of US exports. This geopolitical event affected transactions equivalent to about 18 percent of China's total exports, 5.5 percent of China's GDP and 11 percent of US total exports, 3.6 percent of US GDP(Chang et al., 2021), respectively, changing the business landscape for many firms and cities. Researchers have examined the effects of tariff shocks on different aspects of the economy, such as GDP, employment, productivity, and investment(e.g., Waugh, 2019; Flaaen & Pierce, 2019).

However, the impact of the trade war on commercial real estate in China, an essential component of the economy, remains unexplored. The commercial real estate market is pivotal in facilitating economic activity, attracting domestic and foreign businesses, and contributing to employment and revenue generation. Understanding the impact of tariff shocks on the commercial building market is crucial for assessing the broader implications of the trade war on China's economy.

This study examines the influence of the US-China tariff war on China's commercial real estate market. Did elevated US tariffs depress the commercial real estate market in China? If so, to what extent? Similarly, in response to China's retaliatory tariffs, was there a discernible impact on China's domestic commercial real estate sector due to restricted access to US inputs? Despite the significance of these questions, to the best of our knowledge, no comprehensive study has yet addressed this issue. The lack of research is primarily attributed to delays in data reporting and challenges accessing Chinese data, especially at a sub-national level. Firm-level customs data for the tariff war period (2018 and after) have yet to become available to researchers, possibly due to the sensitive nature of the information(Chor & Li, 2021). There is, moreover, no prevailing literature focusing on how the tariff war affected the Chinese commercial real estate market, given that China was already experiencing a slowdown before the trade disputes, which could reflect other macroeconomic shocks with a timing coincident with the tariffs. Given that the rental sector often serves as a barometer for economic flux more promptly than sales, this study aims to quantify the effect of US-China tariffs on the commercial real estate market across Chinese cities utilizing rents.

To establish this link, we construct two Bartik (or shift-share) style variables for each city within China, serving as proxies of tariff exposures. Specifically, we construct a measure of exposure to the US tariffs for each city within China, based on the initial product composition of its exports. Intuitively, cities that primarily sold products to the US were subjected to heavy tariffs and likely faced a pronounced decrease in export demand. Simultaneously, the retaliatory tariffs imposed by China might have disrupted production for firms depending on US inputs. To explore this, we construct a second Bartik measure of input tariff shocks that combines the initial composition of a city's intermediate and capital goods imports with product-level retaliatory tariffs.

We perform our main analysis on a city-level panel dataset constructed at a quarterly frequency. Specifically, we examine the impact of the tariff shocks that unfolded over the period Q1/2018-Q3/2019 on the year-on-year growth in commercial building rents over Q2/2018-Q4/2019, using a reduced form two-way fixed effect regression.[1]

Our research employs detailed and novel panel data. This empirical strategy requires information on trade flow structure at the detailed city level as described. We assemble this using firm ID to geo-locate firms in the 2016 Chinese customs dataset. We then aggregate the firm-product-level trade flow at the city level and merge it with commercial building rents and various control variables, capturing socioeconomic and financial aspects.

Our baseline results demonstrate a significant negative relationship between US tariffs and commercial building rents at the city level: after accounting for city and time-fixed effects, as well as incorporating socioeconomic and financial attributes, a 1 percentage point increase in the US tariff exposure proxy is associated with 1 percent decrease in commercial building rents growth after one quarter, ceteris paribus. In contrast, China's retaliatory tariff exposure has a positive but insignificant impact on the growth of commercial building rents. This insignificance may result from Chinese importers' ability to easily shift from US to domestic or other international sources.[2] Such sourcing adaptation can bolster Chinese suppliers, amplify domestic product demand, and reinforce market trust, possibly driving up local product prices and profits, which might elucidate the observed positive coefficients, even if they are not statistically significant.

[1]By doing so, we can isolate the impact of COVID-19 and the Phase One agreement that both the US and China agree to reduce tariffs.

[2]Due to the lack of available data, we cannot examine the substitution channels. Regardless of the active channels, we anticipate only a minimal rise in input costs.

We next investigate the international substitution channel, emphasizing the role of trade diversification in potentially cushioning cities from tariff shock impacts. Our trade diversification measurements include dummies for export diversification, denoting a high count of destination markets and exported product types, which then interact with tariff exposures. Similarly, for import diversification, we employ dummies reflecting a high count of sourcing markets and imported product varieties. Our findings do not align with Benguria et al. (2022), revealing no support for the international substitution channel. Data constraints prevent an assessment of the domestic substitution channel. Nevertheless, given our observations, we expect a marginal increase in input costs.

The extent of a city's dependence on the US market could further shape the impact of tariffs on rent growth. While cities typically engaged in trade with multiple nations, the US was a primary partner for many. The fixed costs associated with penetrating new markets could have hindered cities heavily reliant on US sales from diversifying. To assess this, we created metrics reflecting each city's pre-trade war trade volume with the US. Using the city's share of exports and imports to the US as indicators, our results show that cities with a high dependency on US exports experience a 1.33 percent more pronounced effect of US tariff exposure on rents. In contrast, reliance on US imports shows negligible variation in rent impacts.

The literature (e.g., Benguria et al., 2022; Fajgelbaum et al., 2021; Jiao et al., 2022) identifies various cross-sectional drivers of rent responses, for example, financial status, social stability, innovation and location. Benguria et al. (2022) confirm that trade war tariffs increase trade policy uncertainty more for firms with higher revenue and capital. By interacting tariff exposure with the share of profit, tax, value-added tax (VAT), and total current asset of its GDP, respectively, we observe that a sound financial standing can cushion the blow of tariff shocks on commercial building rents.

Furthermore, Campante et al. (2023) emphasizes the importance of preserving social stability during the trade slowdown. Therefore, we use unemployment insurance coverage as a proxy for stability and interact it with tariff exposures. Our findings indicate that cities with extensive insurance coverage can marginally diminish the negative ramifications of the trade war on commercial real estate rents by 2.56 percent.

Then, we explore the role of innovation. Using the share of public financial budgetary expenditure on science as a proxy for innovation, we find that high innovation significantly increases the positive impact of China's tariff exposure by 2.85 percent, indicating that the US tariff exposure and China's tariff exposure affect rent growth through different channels.

Lastly, on the locational front, our research reveals that more developed cities, when compared to their counterparts in the less developed western region, face a reduced negative fallout from the trade war.

Thus, to conclude, our paper provides clear evidence that cities exposed to trade war tariffs experienced a negative commercial building rents shock, which operated particularly through high dependence on the US as an export partner, poor financial situations, social instability, low innovation, and undesired location.

Our paper contributes to the existing literature in assessing the impact of the trade war initiated by the Trump administration on China's commercial real estate rental market. Previous research has focused predominantly on impacts on the US, including pass-through to domestic prices(Fajgelbaum et al., 2020; Amiti et al., 2019; Cavallo et al., 2021), consumption (Waugh, 2019), employment (Flaaen & Pierce, 2019; Goswami, 2020; Chor & Li, 2021), investment (Amiti et al., 2020a), supply chains (Handley et al., 2020), and political economy(Blanchard et al., 2019; Thiemo & Schwarz, 2019).

Little attention has been paid attention to the impacts on China. Benguria et al. (2022) examine the impacts on Chinese listed firms' investments, R&D, and profits. Huang et al. (2020b) study the impacts on the stock price of listed firms in both China and the US Jiao et al. (2022) investigate the effects on export prices and adjustments of sales across different markets for Chinese exporters. Cui & Li (2021) find that the US tariffs curtailed new firm registrations in China while He et al. (2021) find that the US tariffs resulted in fewer online job postings and lower wage offers. Chor & Li (2021) exploit satellite coverage to observe the tariff war's impact on granular grid-level economic activities that span mainland China. Instead of focusing on a particular subset of firms, our study offers a complementary perspective by investigating the consequences for large and medium cities in China.

The remaining sections of this paper are organized as follows. Section 4.2 introduces the institutional background. Section 4.3 provides details on data collection and variable construction. Section 4.4 describes the methodology. Section 4.5 presents the baseline results, cities' heterogeneous responses, and robustness checks. Section 4.6 concludes.

4.2 Background

4.2.1 US-China tariff war

In February 2018, following the Section 201 investigation, the Trump administration declared that imports of solar panels and washing machines adversely affected domestic manufacturers.[3] Consequently, Trump levied safeguard tariffs of 28 percent on solar panels and 20 percent on washing machines. Subsequent to this, under Section 232, the administration sanctioned another series of tariffs based on national security justifications.[4] The US Department of Commerce then conducted comprehensive assessments and imposed a 25 percent tariff on imported steel and a 10 percent tariff on imported aluminum. These initial tariff bouts were not only aimed at China but affected specific products from various countries. According to Chor & Li (2021), only 0.3 percent of China's exports (by value in 2017) were directly impacted. In retaliation, China increased tariffs on US aluminum waste and numerous agricultural products in early April 2018, with the tariff rates varying from 15 to 25 percent.

In July 2018, the US initiated a series of tariff actions specifically targeting China under Section 301 in response to what it perceived as unfair trade practices. The first wave, List 1, affected $34 billion of US imports from China. A second wave, List 2, followed in late August 2018, imposing additional tariffs on another $16 billion of imports from China. These actions, taking place in July and August 2018, elevated tariff rates by 25 percent, encompassing around 3 percent of China's exports. In response, China retaliated by placing tariffs on an equivalent dollar value of Chinese imports from the US.

September 2018 saw the most extensive tariff action, with List 3 tariffs affecting $200 billion of imports from China at a tariff rate of 10 percent. This action broadened the range of HS 6-digit products subject to US tariffs and increased the proportion of China's exports affected to around 10% by value. By June 2019, the US had escalated the List 3 tariffs to 25 percent. In response to this tariff hike, China also raised its tariff rates on the product list that was already targeted in September 2018, covering $60 billion.

The trade war intensified in early September 2019 with the introduction of List 4 of Section 301 tariffs, which added a 15 percent tariff on various Chinese products valued at $112 billion.

[3] The provision of Section 201 in the Trade Act of 1974 empowers the president to invoke safeguard tariffs in response to harm inflicted on a domestic industry.

[4] Under Section 232 of the Trade Expansion Act of 1962, the president can impose protectionist measures on imports that constitute a national security threat.

As a result, the cumulative share of HS6 products and the cumulative share of China's dollar value exports now facing US tariff hikes rose to 93 percent and 14 percent, respectively. It is noteworthy that these US tariffs primarily impacted intermediate inputs and capital goods imported from China, rather than consumption goods, as the Trump administration's apparent desire was to avoid tariffs on consumption goods that would impact directly on American households (Grossman & Helpman, 2020; Bown & Kolb, 2022). In response, China imposed additional tariffs ranging between 5 to 10 percent on $75 billion of imports from the US.

Following signing of the Phase One agreement in January 2020, both sides agreed to a 50 percent reduction in tariffs from the September 2019. China also agreed to buy $200 billion of additional US exports before December 31, 2021. In the end, it bought none of that extra $200 billion, purchasing only 57 percent of its total commitments over 2020-21, not even enough to match its import levels from before the trade war. Meanwhile, the Trump administration extended the import protection under Section 201 for washing machines by two years. In February 2023, after three years of tariffs and a two-year extension, the import protection on washing machines initially announced by Trump in early 2018 expired. On the other hand, the Biden administration extended Section 201 tariffs on imported solar panels and cells by four years with certain modifications, indicating that the trade war is ongoing.

A last policy worth highlighting was China's remarkable unilateral reductions to its applied most favored nation (MFN) tariffs during the trade war. Tariff cuts were made for hundreds of products, accounting for about 35% of China's imports by July 2019(Chor & Li, 2021).

As shown in Figure 4.1, up to the first quarter of 2018, the average US tariff on imports from China remained essentially constant, at roughly 4.5%. In stark contrast, by the fourth quarter of 2019, the average US tariff hit 28.4%, more than six times larger than the original tariff. Over this period, China imposed similarly dramatic tariff increases on products imported from the US. Starting from a low level of 9.2% and without much change up to the third quarter of 2018, China's tariffs rose to 16.7% by the fourth quarter of 2018 and 26.9% by the fourth quarter of 2019.[5]

[Figure 4.1]

Figure 4.2 plots China's exports to the US and the rest of the world from 2017 to 2021. During the trade war period (from the start of 2018 to the end of 2019), the exports to the

[5] Figure 4.1 displays the evolution of tariffs imposed by the US and China, respectively, where each dot denotes the average tariff computed as the simple mean of tariffs across all HS 10-digit sectors.

US dramatically decreased in 2018, then recovered partially in 2019, following the waves of tariff shocks. At the same time, the export to the rest of the world (ROW) remains relatively constant.

[Figure 4.2]

4.2.2 Related literature

The US-China tariff war has had significant economic implications. Studies have consistently refuted the notion that China bore the burden of the US tariffs in 2018 (e.g., Amiti et al., 2019, 2020b). In fact, the aggregate welfare loss in China is limited, ranging from 0.04% to 0.29% of GDP (Fajgelbaum et al., 2020; Chang et al., 2021). Instead, there were considerable losses for US import buyers, which were almost entirely offset by gains to US producers (Fajgelbaum et al., 2020).[6]

Numerous studies have explored how tariffs impact export prices, firm profit, and welfare in the US.[7] They identify three primary channels: dampening foreign demand, increasing imported input costs, and reallocating expenditures to domestic goods (Fajgelbaum & Khandelwal, 2022). Focusing first on the retaliatory tariff channel, we find some conflicting results. Amiti et al. (2020b) show that US variety-level export prices to China did not fall in response to retaliatory tariffs, suggesting that US producers may flexibly adjust to other destinations (Fajgelbaum et al., 2020; Amiti et al., 2020b). In contrast, Cavallo et al. (2021) find relative price reductions in US products targeted by China. At more aggregate levels, Fajgelbaum et al. (2020) present evidence that US sector-level export price indices fell with retaliatory tariffs in the same sector.

Turning to the second channel, studies have found that US export prices generally rise with US tariffs due to the increased cost of imported inputs (Benguria & Saffie, 2019; Flaaen & Pierce, 2019). For instance, Flaaen & Pierce (2019) found that a shift in exposure to rising input costs was associated with a 4.5% relative increase in factory-gate prices.

Lastly, export prices also increased with import tariffs as domestic demand was reallocated away from imports into domestically produced goods, making them more scarce internationally

[6] However, many of these buyers are firms rather than final consumers, which has implications for the pass-through of tariffs on prices and other economic outcomes (Amiti et al., 2019, 2020b; Fajgelbaum et al., 2020).

[7] Relatedly, Jeanne & Son (2020) study the effects of the trade war on exchange rates and the extent to which the renminbi depreciation against the US dollar offsets tariffs. They find that tariffs explained at most one-fifth of the dollar's effective appreciation but around two-thirds of the renminbi effective depreciation observed in 2018-19.

and therefore more expensive (Fajgelbaum et al., 2020; Amiti et al., 2020b). Tariffs can further affect firm profit and commercial building rents through these channels.

In parallel, other researchers have performed model-based assessments of the impact of the tariffs on China's economy (e.g., Ferraro & Van Leemput, 2019; Ju et al., 2020; Zhou, 2023). For example, Caliendo & Parro (2015) built a general equilibrium trade model that accounts for global value chain linkages. Their results highlight the importance of sectoral heterogeneity, intermediate goods, and sectoral linkages for quantifying welfare gains from tariff reductions.

Despite the extensive research on the economic impact of the US-China tariff war, there is a noticeable gap in the literature regarding its specific effect on the commercial real estate market. The trade war may increase the price of products from China and therefore decrease profitability for Chinese exporters through dampening foreign demand (particularly US demand). It may also decrease profitability for Chinese importers through increasing costs due to the Chinese government's retaliatory tariffs on inputs sourced from the US (Benguria & Saffie, 2019; Flaaen & Pierce, 2019). Such negative impacts on firms can lead to a deterioration of their financial positions, making it harder for them to survive during the trade war and thus reducing the demand for commercial real estate (Titman, 1985). Similar phenomena have been observed during economic downturns when companies face financial hardships, and the demand for commercial real estate decreases (Wheaton, 1999; Ling & Naranjo, 2015).

Benguria et al. (2022) find that trade war tariffs can increase a firm's trade policy uncertainty and, furthermore, reduce firm-level profits and investment. These findings align with prior research indicating that heightened policy uncertainty negatively impacts investment and demand for commercial property (Baker et al., 2016; Bernanke, 1983). Potential tenants can hesitate to commit to long-term leases due to concerns about future economic conditions, leading to lower rents (Grenadier, 1995).

Some literature highlights the heterogeneity of the tariff war's impact. The negative effects were highly skewed across locations, with areas experiencing the largest US tariff shocks seeing significant decreases in income per capita and manufacturing employment (Benguria et al., 2022). Large heterogeneity in impacts across locations was also observed in China (Chor & Li, 2021). Similarly, the impact of the trade war on commercial building rents in China is likely to vary across different regions and types of commercial real estate. Areas or types of properties that are more heavily dependent on sectors of the economy that have been hit hard by the trade war could see larger rent decreases. This aligns with previous research indicating that economic shocks can have varied effects across different regions and property types (Glaeser et al., 2008).

4.3 Data

4.3.1 City level data

We obtain monthly average commercial building rents from CREI for 34 large Chinese cities, measured in yuan per square meter.[8] We then aggregate it into quarterly frequency for each corresponding city. We extract a set of macroeconomic variables from China city statistical yearbooks. This includes city-level new commercial building construction, exports and imports, end-of-the-year population, public budgetary revenue and expenses, GDP, end-of-year deposits, and loans of the national banking system.

4.3.2 Tariff data

The detailed tariff data includes city-level tariff shocks exposed during the US trade war, as derived from Chor & Li (2021).[9] The tariff data is a combination of product-level tariff data from Bown (2021) and firm-level 2016 Chinese customs data. We obtain the Bartik (or shift-share) proxy for key tariff shock explanatory variables from Chor & Li (2021). The export exposure of city i in China to the US tariffs is given by:

$$\Delta Tariff_{i,t}^{US} = \sum_{k} \frac{X_{ik0}^{US}}{X_{i0}} \Delta Tariff_{kt}^{US}, \tag{4.1}$$

where X_{ik0}^{US}/X_{i0} is the value of product-k exports from city i to the US as a share of city i's total exports in 2016 prior to the tariff war. This Bartik-style tariff exposure with time-invariant share (weights) can help circumvent potential reverse causality in a city's exports with respect to US tariffs. The variation in $\Delta Tariff_{i,t}^{US}$ arises from: (i) differences across cities in initial export product composition and in the significance of the US as a destination for these exports;[10] and (ii) differences across products (at the HS 6-digit level) in the US tariff increases

[8] The cities available on CREI includes Tianjin, Shijiazhuang, Taiyuan, Hohhot, Dalian, Shenyang, Changchun, Harbin, Shanghai, Nanjing, Hangzhou, Ningbo, Hefei, Fuzhou, Xiamen, Nanchang, Jinan, Qingdao, Zhengzhou, Wuhan, Changsha, Guangzhou, Shenzhen, Nanning, Haikou, Chongqing, Chengdu, Guiyang, Kunming, Laksa, Xi'an, Lanzhou, Xinning, Yinchuan, and Urumqi. Beijing is excluded due to data unavailability.

[9] We also incorporate Chinese MFN tariffs. Please refer to the Appendix for more details.

[10] The share is calculated using 2016 Chinese customs data. For more details, please refer to Chor & Li (2021).

over time, $\Delta Tariff_{kt}^{US}$.[11] Hence, a city specializing in exporting products to the US market, which subsequently faces larger US tariff increases, is exposed to a more significant decline in external demand.[12]

The Chinese retaliatory tariff may raise production costs for Chinese manufacturers, leading to a deterioration in their financial situation and ultimately impacting the commercial building rental market. The import retaliatory tariff exposure is also extracted from Chor & Li (2021), measured by:

$$\Delta Tariff_{i,t}^{CHN} = \sum_{k} \frac{M_{ik0}^{US}}{M_{i\mathcal{K}0}} \Delta Tariff_{kt}^{CHN}, \tag{4.2}$$

where $\mathcal{K}$ represents the set of products k classified as inputs.[13] $M_{ik0}^{US}/M_{i\kappa_0}$ denotes the base-year value of imports from the US for product k, expressed as a share of total imports of inputs at the city level. Chor & Li (2021) focus on tariffs on inputs, as these could potentially raise production costs for Chinese manufacturers. As constructed, $\Delta Tariff_{i,t}^{CHN}$ leverages: (i) cross-location differences in initial import composition and in the importance of the US as a source country for these inputs; and (ii) variation across products and over time in China's retaliatory tariffs, $\Delta Tariff_{kt}^{CHN}$.

We used a reduced form fixed effect model to examine the impact of the tariff shocks that unfolded over Q1/2018-Q3/2019 on the year-on-year growth in commercial building rents over Q2/2018-Q4/2019. The analysis encompasses the Section 201 tariffs on solar panels and washing machines (which were implemented in February 2018), the Section 232 tariffs on aluminum and steel products (March 2018), as well as the four rounds of Section 301 tariffs (from July 2018 to September 2019). The primary focus of the analysis is on these tariff actions up until September 2019, prior to the Phase One Trade Agreement between the US and China. Our sample included 229 observations constructed by 34 large cities available from CREI.

4.3.3 Tariff exposure and Trade Policy Uncertainty

Recent studies have confirmed a positive relationship between trade war tariff exposure and Trade Policy Uncertainty (TPU). For instance, Benguria et al. (2022) found that firm-level

[11] The product-level tariff is from Bown (2021). For more details, please refer to Bown (2021).

[12] The tariff exemptions granted by US authorities are accounted for in Chor & Li (2021)'s tariff exposure construction. While aggregating the data to either the monthly or quarterly level, they scale the tariffs by the number of days they were in effect.

[13] More specifically, this is the set of HS 6-digit products classified as either an intermediate input or a capital good by the United Nations' Broad Economic Categories (BEC), Revision 5, coding system.

tariff exposure positively impacts firm-level TPU. Existing TPU indices in China are largely limited in geographic location and temporal scope and thus cannot be used at the city level. However, national-level validation is still available. Davis et al. (2019) constructed a TPU index for mainland China using articles from two leading newspapers. They quantified the index by counting the number of specific words related to uncertainty, economics, and trade policy.

Figure 4.3 illustrates the national TPU alongside a national version of our tariff exposures. They exhibit similar fluctuations. Both measures spike in 2018Q3, then experience a slight drop in 2018Q4 and rebound until a dramatic fall in the last quarter of 2019 when the Phase One Agreement is signed. The only discrepancy occurs in 2019Q3 when TPU decreases, contrasting with the tariff measure. This is plausible as there may have been disagreement in point of view between newspapers then. Overall, the parallel trends suggest that trade exposure potentially captures part of the TPU, supporting Benguria et al. (2022).

[Figure 4.3]

4.4 Empirical Strategy

Our primary interest is studying the effects of the tariff war on city-level commercial rents in Chinese cities of the following trade barriers: (i) the US tariffs imposed on goods from China and (ii) the retaliatory tariffs imposed by China on goods from the US. We therefore apply below reduced form baseline regression following Chor & Li (2021) and Benguria et al. (2022):

$$\begin{aligned} \Delta log(Rent_{it}) =& \alpha_i + \beta_1 \Delta Tariff_{i,t-1}^{US} + \beta_2 \Delta Tariff_{i,t-1}^{CHN} \\ &+ \gamma log(W_{it}) + \delta D_t + \eta D_i + \epsilon_{it} \end{aligned} \tag{4.3}$$

The variable $Rent_{it}$ represents the average commercial building rents in city i during time t, measured in yuan per square meter. The operator Δ denotes the difference between the values at time t and $t-4$ for a specific variable. Here, we refer to the unit of time as the year-quarter (e.g., Q2/2019).[14]

The variable $Tariff_{i,t-1}^{US}$ represents the degree to which city i's exports are exposed to US tariff shock in the period $t-1$. $Tariff_{i,t-1}^{CHN}$ signifies the extent of exposure for city i's imports in

[14] To check stationarity, we conduct a Fisher-type unit root test (Choi, 2001), which examines the unit root for each cross-section separately and provides an overall test result. Tariff exposures are found to accept the null hypothesis of unit roots, indicating it is non-stationary. Consequently, we estimate the model in first differences, following the approach of Campante et al. (2023), Benguria et al. (2022) and Chor & Li (2021).

response to China's retaliatory tariffs levied against inputs sourced from the US. It is important to note that we lag the tariff shocks on the right-hand side by one period to account for the delayed response of commercial rents to tariff increases, same as Chor & Li (2021) and Benguria et al. (2022). The two-way fixed effects structure includes a comprehensive set of year-quarter dummies (D_t) and city dummies (D_i).

In our estimation, we regress stacked year-on-year changes in log commercial building rents over the period Q2/2018 to Q4/2019 on stacked year-on-year changes in city-level tariffs over Q1/2018 to Q3/2019, which covers all the significant tariff increases by the Trump administration and the responses by the Chinese government. To address potential issues of serial correlation over time and heteroskedasticity, we clustered standard errors at the city and year-quarter level.[15]

Recent literature presents two separate arguments suggesting that, conditional upon the inclusion of relevant controls, the ϵ_{it} should be uncorrelated with either: (i) the initial export structure of the city level, referred to exogenous "share", as considered in Goldsmith-Pinkham et al. (2020)); or (ii) the product-specific tariff shocks experienced at the national level, referred to exogenous "shift", as considered in Borusyak et al. (2022). Defending the second condition proves more challenging in our case, given the fact that tariffs were targeted at narrow sets of products, such as solar panels and washing machines, under Section 201 and Section 232. Recent studies pointed out Trump administration avoided tariffs on consumption goods that would bear directly on American households at the early rounds of Section 301. The Section 301 tariffs were moreover ostensibly directed at products from high-tech manufacturing industries emphasized by China's "Made in China 2025" industrial policy plan (Chor & Li, 2021).

We thus defence the exogeneity from the perspective of "share", that is, the exogeneous of the initial city-level trade shares that capture a location's exposure to subsequent tariff changes (i.e., following Goldsmith-Pinkham et al. (2020)). To express this more formally, we require: $E\left[\left(X_{ik0}^{US}/X_{i0}\right) u_{it} \mid \mathcal{W}\right] = 0$, where $\mathcal{W} = \{D_t, D_i, W_{it}\}$ is a set of controls on the right hand-side of the regression. Nonetheless, one might still challenge these orthogonality conditions under two circumstances: (i) if local specific trends in commercial rents, influenced by forces other than the tariff shocks yet correlated with the initial city-level trade shares (i.e., X_{ik0}^{US}/X_{i0}), exist;

[15] To check the robustness, we also clustered standard errors at the provincial level. Unless specified, the standard errors in this paper are clustered at the city and year-quarter levels.

or (ii) if other contemporaneous product-level shocks affect local commercial rents through the same assortment of exposure shares.

In order to address concern (i), we employ several strategies. First, we will confirm that the tariff shocks in exposures is uncorrelated with pre-trends in commercial building rents growth. Second, it bears repeating that the specification in the regression includes city fixed effects D_i, which account for differences in average year-on-year growth in commercial rents across cities. Moreover, we take into account additional city-level variables W_{it} that might also influence local commercial building rents, namely:

new commercial building construction (to capture supply of commercial building), exports and imports(to capture overall openness), population, public budgetary revenue, public budgetary expense and GDP (to capture local economic development), year-end deposit in banks and year-end loans in banks (to capture financial activities). All in logarithm.

In further checks, we also consider the possible role of initial city-level trade shares in particular subsets of products, as well as of the importance of state-owned enterprises in city-level exports. These are each interacted with a full set of year-quarter dummies ($W_{i0} \times D_t$), to control for trends in local economic activity that might stem from these initial city-level features. In relation to the second concern identified in (ii), we ensure that our findings hold when controlling for Bartik-style variables that seek to directly pick up city-level exposure to another candidate shock: product-level adjustments in China's MFN tariffs.[16]

4.5 Results

4.5.1 Baseline Results

Table 4.1 presents our baseline results in equation 4.3. In columns (1) and (2), we report the impact of US tariff exposure and China's retaliatory tariff exposure on city commercial building rents in the absence of control variables, respectively. A 1 percentage point increase in a city's exposure to US tariffs reduces growth in commercial building rents by 0.73 percent, statistically significant at 10% level. In contrast, China's retaliatory tariff exposure has a positive but insignificant impact on the growth of commercial building rents (0.09).[17]

[16] See robustness check for more details

[17] We also examined the impact of the US-China tariff war on office building rents using the same empirical model. Not surprisingly, the results did not exhibit statistical significance. This outcome aligns with expectations, given that stores and factories closely tied to exported or imported products are directly impacted by the tariff

[Table 4.1]

In column (3), we incorporate both the US tariff and China's retaliatory tariff exposures in the regression. The coefficient of US tariff exposure remains negative and statistically significant at 10%. A 1 percentage point increase in the US tariff exposure measure is associated with a 0.79 percent decrease in commercial building rents after one quarter. China's retaliatory tariff exposure remains positive and statistically insignificant, with a coefficient of 0.36.

In column (4), our preferred result, we control for a set of socioeconomic variables that could concurrently affect commercial building rents. This specification addresses the potential concern that tariff shocks could instead have been picking up underlying trends in commercial rents related to local openness, financial activities, and local development. We observe a large negative impact of US tariff exposure (-0.84), statistically significant at 5% level. Quantitatively, a 1 percentage point increase in the US tariff exposure is associated with a 1.03 percent decrease in commercial building rents after one quarter (holding all else constant). In other words, a one standard deviation increase in the US tariff exposure will result in a 0.40 standard deviation decrease in rent growth. In contrast, China's retaliatory tariff remains positive and statistically insignificant (0.84).

In column (5), we clustered the standard errors at the provincial level to account for serial correlation over time and spatial correlation across cities within a province. This does not affect our conclusions on the negative and significant effect of the US tariffs on commercial building rents, while China's retaliatory tariff remains positive and statistically insignificant.

In all columns, we observe insignificant China's retaliatory tariff exposures. This can be attributed to (i) China's cuts to its MFN tariffs, which helped to dampen the rise in the cost of imports, (ii) the ease with which Chinese importers can redirect their sourcing of inputs from American suppliers to domestic or alternative international sources. Subsequently, we examine the first channel in the latter section: impacts of the inclusion of MFN tariff exposure into our baseline model (see column (1) of Table A2 in Section 4.5.3). Due to the lack of relevant data, we cannot assess the influence of substitutes. Nonetheless, existing channels are unlikely to cause a significant increase in input costs according to literature (see Chor & Li (2021)). Furthermore, leveraging domestic substitutes can nurture Chinese suppliers, increase domestic product demand, and boost market confidence. This increase in demand could elevate

war. In contrast, office buildings primarily serve administrative purposes and are less intertwined with import-export activities. Therefore, while the tariff war exerts a significant negative influence on commercial building rents, its effects on office building rents appear to be negligible.

product prices and profits for local suppliers, potentially explaining the observed positive, albeit insignificant, coefficients. In addition, it is harder and will take more time to open new markets for exporter comparing with importers to redirect their source of inputs, which could also explain the non-effect of China's retaliatory tariff, comparing with the significant negative impact of US tariff exposure.

In addition, it is worth to note that commercial real estate rents tend to be fixed for at least one year for most contracts. When aggregating rents change at city level, only a fraction of rents change at any given point of time is captured, the estimated effect should be interpreted as the lower bound of the US-China tariff war on China's commercial real estate rent growth.

4.5.2 Heterogeneity in rent response

US dependence Drawing on the findings of Benguria et al. (2022), the effect of tariffs on firm-level trade policy uncertainty varies with the extent of reliance on US sales. This heterogeneity might also exist in the commercial building rental market. While many cities engage in sales with diverse countries, the US remains a predominant partner for numerous cities. Given the fixed costs associated with identifying and accessing new markets, cities with pronounced dependence on US sales may find it challenging to hedge their US export challenges by quickly expanding elsewhere, thereby experiencing substantial profit reductions.[18] This situation might push numerous firms towards insolvency, while the surviving entities may resort to cost-cutting measures, culminating in diminished demand in the commercial building market.

We construct a US reliance proxy to test this channel by utilizing the 2016 Chinese customs trade data.[19] Specifically, $High_i$ is a dummy variable that divides the cities into two equal groups according to the national median value in the year 2016. Specifically, $High_i^{US}$ and $High_i^{CHN}$ equal one if the city's US exports or imports as a share of its total exports or imports exceed the corresponding national median value in 2016, respectively. Then, we incorporate them into our baseline equation as follows:

[18]Such reductions can precipitate a decline in the financial health of firms, an aspect we intend to examine subsequently.

[19]To operationalize the idea of dependence, we exploit the transaction data from Chinese customs at the city-product country level. We compute the US exports(imports) as a share of the city's total exports(imports) in 2016.

$$
\begin{aligned}
\Delta log(Rent_{it}) =& \alpha_i + \beta_1 \Delta Tariff^{US}_{i,t-1} + \beta_2 \Delta Tariff^{CHN}_{i,t-1} \\
& + \beta_3 \Delta Tariff^{US}_{i,t-1} \times High^{US}_i + \beta_4 \Delta Tariff^{CHN}_{i,t-1} \times High^{CHN}_i \\
& + \gamma High_i + log(W_{it}) + \delta D_t + \epsilon_{it}
\end{aligned} \tag{4.4}
$$

The coefficient β_3 captures the differential impact of city-level exposure to US tariffs on commercial building rents as mediated by the city's differences in the dependence on the US, while β_4 captures the differential impact of China's retaliatory tariff exposure with response to US dependence.

As reported in column (1) of Table 4.2, β_3 is negative and statistically significant at 10%. Compared to non-US-dependent cities, high US dependence results in a 1.33 percent stronger negative impact from US tariff exposure on commercial building rents, which is 0.25 standard deviations stronger negative impact. This result is in line with the finding of Benguria et al. (2022) at the firm level- the ability of Chinese firms to hedge in export markets diminishes when they have a high level of dependence on US sales due to the fixed costs of locating and entering new markets.

[Table 4.2]

Product and market diversification Fajgelbaum et al. (2021) and Jiao et al. (2022) both confirm that US tariffs led China to reduce exports to the United States but also increase exports to ROW (or, at least, they cannot reject that Chinese product-level exports to the ROW remained constant). These results indicate an easy reallocation of products across destinations for Chinese producers.[20] Such flexible substitution makes it easier for highly diversified cities to shift their exports to other destinations or produce other types of products when the US-China trade war increases tariffs on specific products to the US.

Similarly, more diversified Chinese cities can shift their imported inputs from the US to cheaper domestic or other international markets, resulting in a smaller profit loss (or even an increase in profit). The negative impact of export tariff exposure on the commercial real estate rental market should be smaller for those diversified cities. In comparison, the positive impact of import tariff exposure should be more significant for those more diversified cities.

[20]Similarly, Amiti et al. (2020b) show that US variety-level export prices to China did not fall in response to retaliatory tariffs, suggesting that US producers may adjust flexibly across destinations (Fajgelbaum et al., 2020; Amiti et al., 2020b).

Following the same strategy as we construct the US dependence proxy, we construct two measures of trade diversification utilizing the 2016 Chinese customs trade data. The first refers to the diversification concerning export (import) destinations, whereas the second pertains to the diversification concerning types of exported (imported) products. Now, both $High_i^{US}$ and $High_i^{CHN}$ attain a value of one if either the number of a city's export (import) destinations or the variety of products exported (imported), as a proportion of the city's total export (import) destinations or the overall variety of exported (imported) products, surpasses the respective national median value for 2016.

Columns (2) and (3) of Table 4.2 report the results of how trade diversification changes the impact of tariff shocks on commercial building rents. Both β_3 and β_4 are statistically insignificant. We find no evidence that high trade diversification precipitates a more pronounced positive effect of the trade war on commercial building rents. As previous literature has concluded on the impact of tariff war on trade policy uncertainty, we can link export prices to the commercial real estate market.

Financial situation Fajgelbaum & Khandelwal (2022) concludes that the US-China trade war has lowered aggregate real income in both the United States and China. At the firm level, Benguria et al. (2022) confirms that trade war tariffs increase trade policy uncertainty more for firms with higher revenue and capital. In the commercial real estate rental market, cities with high profits and better financial performance will have a higher probability to survive when the trade war hits. Thus, commercial real estate demand increase is marginally larger in those cities.

We test whether financial situations will heterogeneously affect commercial building rent growth. To do this, we replace the interaction terms with several proxies that reflect a city's financial situation, including dummies that are equal to one if the city's profit, tax, value-added tax (VAT), or total current assets, as a share of its GDP, are higher than the corresponding national median value in 2017, respectively. We then repeat the analysis using equation 4.4.

The results are reported in Table 4.3. In column (1), β_4 is positive and statistically significant at 1% level. This implies that the positive impact of China's tariff exposure on commercial building rents is stronger for cities with a higher share of initial profits. Specifically, the positive impact is 3.35 percent more for high-profit cities compared to low-profit cities, which is 0.43 standard deviations more. On the other hand, the results in column (2) with the city's tax are positive but statistically insignificant (1.59). The results in column (3) with value-added tax remain positive and statistically significant at 1% level(3.29), supporting that for cities with more firms in a good financial position (the share of value-added tax will be higher too), the positive impact from China's tariff exposure will be stronger. In the last column, we use the total current assets to interact with tariff exposures, and the results remain consistent, though insignificant (with an elasticity of 7.49). β_3 is statistically insignificant through all results, indicating that China's tariff exposure and US tariff exposure affect commercial real estate through different channels. In all cases (i.e., profit, tax, VAT, and total current asset), the results show that a larger China tariff exposure increases commercial building rents for better financially situated cities, in line with our assumption that higher profit and better financial situation can protect the city from negative impacts of the trade war.

[Table 4.3]

Social stability Campante et al. (2023) highlight the role of social stability during China's export slowdown in the mid-2010s. We augment our empirical model to control for social stability, measured by the dummy variable $High_i$, representing the share of unemployment insurance coverage to the city population in 2017. All other variables remain as in (4.4). Our coefficient of interest is β_3. If higher unemployment insurance coverage leads to easier redundancy without worrying about social stability, firms can reduce costs and survive during the trade war. The decrease in demand for commercial building rents will be smaller, implying a positive β_3.

The result in column (1) of Table 4.4 confirms our hypothesis. The coefficient of 2.56 indicates that a 1 percentage point increase in tariff exposure in cities with a high percentage of unemployment insurance coverage will result in a 2.56 percent smaller decrease in the growth rate of commercial building rents compared to the benchmark city with low unemployment insurance coverage.

[Table 4.4]

Innovation In column (2), we further examine how the impact of tariff exposure on commercial building rents differs based on innovation, proxied by the share of public financial budgetary expenditure on science to total expenditure in cities. β_4 is positive and statistically significant at 1% level. The results show that compared to cities with low science expenditure, the cities with high shares of science expenditure increase the commercial real estate rents by

2.85 percent. In other words, a one standard deviation increase in tariff exposure in cities with a high innovation will result in a 0.38 standard deviation larger increase in the growth rate of commercial building rents compared to the benchmark city with low innovation, mitigating the negative impact from the tariff war. $\beta_3 4$ is statistically insignificant. Again, this result indicates that China's tariff exposure and US tariff exposure affect commercial building rents through different channels.

Location China's economic growth and the policies that facilitated it have progressed in waves from the East to the West of China. We thus explore whether the trade war tariffs impacted commercial building rents differently depending on the city's location. We divide all cities into three regions: East, Central, and West, according to the classification of the China National Bureau of Statistics. We drop the Western region as the reference group and run a regression with interactions involving the Eastern and Central regions.

The results are reported in column (3) of Table 4.4. The coefficient of US tariff exposure remains statistically significant at the 1% level and is large in magnitude (-1.72). The interaction term β_3 reflects that, compared to the relatively undeveloped Western region, the negative impact of the trade war is smaller for the Eastern and Central regions (1.59 percent and 1.03 percent less). A plausible explanation for this result could be that cities in Western China have low trade diversification, high reliance on the US, fewer firms, and low capability to redirect imports to alternative sources of imports.

4.5.3 Robustness

Pre-Trend Checks Table A1 presents the pre-trends test. Columns (1) through (3) in this table report the results using $\Delta \log(Rent_{i,t-1})$, $\Delta \log(Rent_{i,t-2})$, and $\Delta \log(Rent_{i,t-3})$ as the respective dependent variables. The findings reveal statistically insignificant coefficients on the explanatory variables related to tariff shocks. We thus confirm that these tariff changes are exogenous and uncorrelated with pre-trends in commercial building rent growth.

[Table A1]

Most Favored Nation tariff Shocks During the same time as the US-China tariff war, there were other policy changes that could have similarly influenced commercial rental markets. Therefore, our regression analysis may not solely capture the effects of tariff war-related factors. To address this concern, we aim to control for a proxy that captures such shocks directly.

During the tariff war, China reduced Most Favored Nation (MFN) tariffs on a range of products. These tariff cuts would have partially offset the negative impact of export tariff shocks. To capture the change in input costs resulting from the MFN tariff reductions, we again utilize the Bartik style MFN input tariff shock from Chor & Li (2021):

$$\Delta Tariff_{i,t}^{MFN} = \sum_k \frac{M_{ik0}^{US}}{M_{i\mathcal{K}0}} \Delta Tariff_{kt}^{MFN}, \tag{4.5}$$

where $M_{ik0}/M_{i\mathcal{K}0}$ is the imports of HS 6-digit product k attributable to firms in city i in the base year (2016), expressed as a share of total imports of inputs in that year. $\Delta Tariff_{kt}^{MFN}$ denotes the change in the MFN tariff rate for product k at time t relative to a year ago. As input tariffs, we limit the construction of these shares to products $k \in \mathcal{K}$ that are deemed to be intermediates or capital goods under the UN BEC, Revision 5.

We include the additional variable $\Delta Tariff_{i,t}^{MFN}$ in the baseline regression 4.3. The result is reported in columns (1) and (2) of Table A2. However, we acknowledge a potential caveat with this specification. The changes in MFN tariffs for certain products could have been endogenous responses by the Chinese government, strategically reducing MFN tariffs to mitigate the adverse effects of the tariff war on economic activities. Moreover, some MFN tariff cuts for high-technology goods were already scheduled before the tariff war, leading to potential anticipation effects. Due to these considerations, we treat the MFN tariff shock as a control variable in a robustness check. This analysis finds a statistically insignificant coefficient for $\Delta Tariff_{i,t}^{MFN}$. According to our preferred result in column (2), where we include a set of controls, the inclusion of MFN tariff shock reduces the magnitude of China's retaliatory tariffs on US-sourced inputs slightly from 0.84 in baseline results (see column (4) of Table 4.1) to 0.81, indicating that China's cuts to its MFN tariffs did help to dampen the rise in the cost of imports, though the impact is small. MFN tariff reduction may boost market confidence. This increase in confidence and reduction in costs could elevate profits for local suppliers, potentially explaining the observed positive, yet insignificant, coefficient of China's retaliatory tariff exposure. The estimated effect of the US tariff shock remains large in magnitude and statistically significant at 5% level (-1.04).

[Table A2]

Weighted Least Squares regression We additionally estimate a Weighted Least Squares regression to address heteroscedasticity (Wooldridge, 2015) employing population weights. Our

Weighted Least Squares results remain significant (the results are reported in column (3) of Table A2.

Alternative controls There remains the concern that the initial export structure could be correlated with other location characteristics, which in turn might be the basis for systematic shifts over time in commercial building rents (reverse causality). Toward this end, we construct additional city base-year variables W_{i0}, including log new commercial building construction, the share of industrial enterprises, log population, log GDP per capita, log manufacturing industry employment, log year-end deposit in banks and log year-end loans in banks. These are each interacted with a full set of year-quarter dummies ($W_{i0} \times D_t$), to control for trends in local economic activity that might stem from these initial city features. The results reported in column (4) of Table A2 remain unchanged.

4.6 Conclusion

We examine the indirect and unintended effects of trade policy changes on commercial building rents in China. Our baseline results reveal a significant negative impact of US tariffs on commercial building rents at the city level. In contrast, China's retaliatory tariff has no significant impact on commercial rent growth. A 1 percentage point increase in the US tariff exposure measure is associated with a 1.03 percent decrease in commercial building rent growth after one quarter, ceteris paribus. The negative impact remains significant after controlling for socioeconomic factors, suggesting that pre-existing commercial rent trends do not drive the effect.

We further explore heterogeneity in rent responses to tariff shocks. Our findings demonstrate that cities with a high dependence on US sales experience a stronger negative impact on commercial building rents. This aligns with previous studies that highlight the challenges faced by firms with a high level of dependence on US sales in hedging export markets, and suggests that the ability to expand into new markets quickly is limited for these cities dominated by this kind of firms (e.g., Benguria et al., 2022).

We additionally explore the role of trade diversification as a potential mitigator of the detrimental effects of the trade war. We distinguish between import and export diversification in our examination. Initially, cities demonstrating greater export diversification would be able to reorient these exports to alternate markets or adapt to different products. Similarly, cities with enhanced import diversification would exhibit corresponding flexibility and import inputs from the US to domestic or alternative markets. However, our empirical findings do not statistically validate these hypotheses. The results derived from prior literature, indicating an effect of trade diversification on trade policy uncertainty and export prices, are not extendable to the domain of commercial real estate rental markets.

Moreover, we examine the influence of the city's financial situation on rent response to tariff exposure. Cities with higher initial profit, tax, value-added tax, and total current assets experience a smaller negative impact on commercial building rents. This suggests that better financial positioning enables firms to survive the trade war, reducing the overall demand decrease in the commercial rental market, in line with previous literature (Fajgelbaum & Khandelwal, 2022).

We also confirm that high social stability leads to a lower negative impact of tariffs on rents, supporting Campante et al. (2023) 's finding of the negative impact of trade slowdown on social stability. Better innovation and location can also marginally reduce the negative impact of the tariff war.

We finish with a battery of further robustness checks. Pre-trend checks confirm that the tariff changes do not correlate with pre-existing commercial building rent growth trends. Including Most Favored Nation (MFN) tariff shocks as a control variable does not substantially change the impact of export or import tariff shocks on commercial building rents. Controlling for additional city characteristics and implying Weighted Least Square regressions yield consistent results.

Overall, our study provides valuable insights into the snowball effects of trade shocks on commercial building rents in China. The findings highlight the significance of export tariff shocks in influencing the commercial rental market, with variations based on US dependence and firms' financial situations. Such observations indicate that policymakers in developing nations should strategize proactively. There is an imperative to bolster the resilience of their property markets against exogenous disturbances. This can be achieved by diversifying international economic ties and formulating preemptive risk-mitigation strategies. Of paramount importance is the realization that trade confrontations have harmful consequences for both the US and Chinese economic landscapes. Hence, a comprehensive understanding of these multifaceted repercussions is crucial when policymakers contemplate initiating or escalating tariff conflicts.

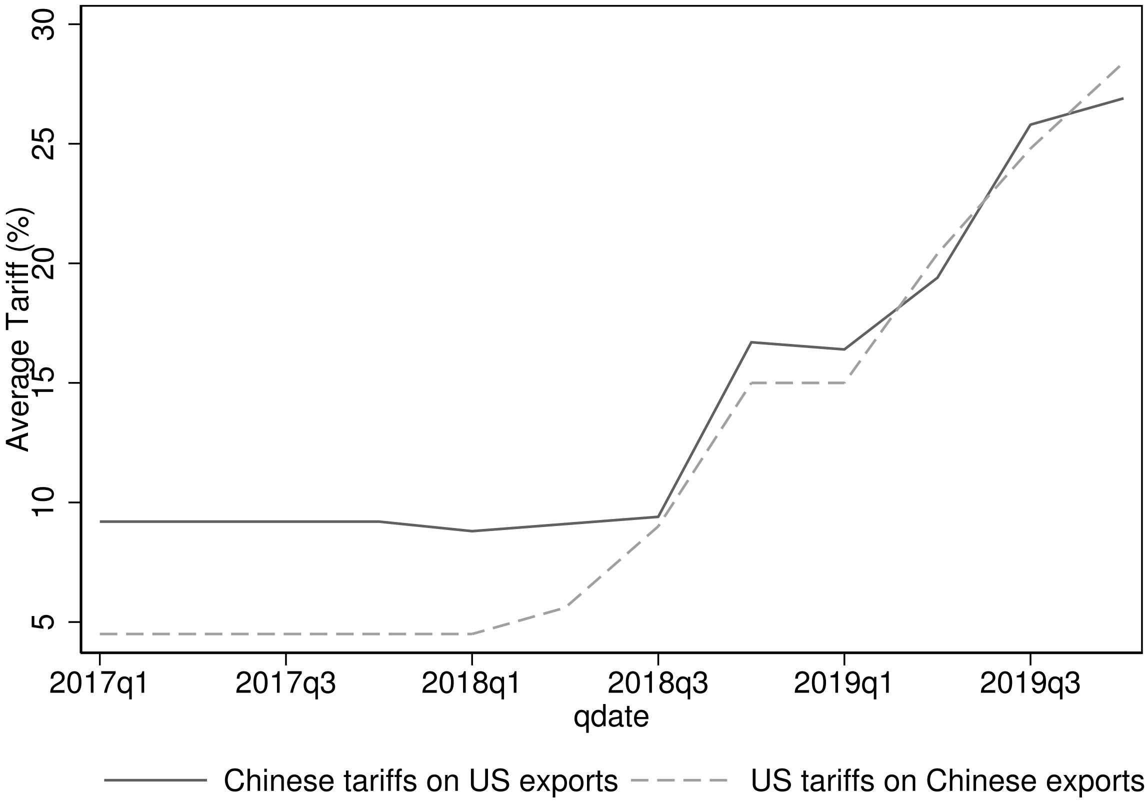

Figure 4.1: US-China tariff rates toward each other and rest of world (ROW)

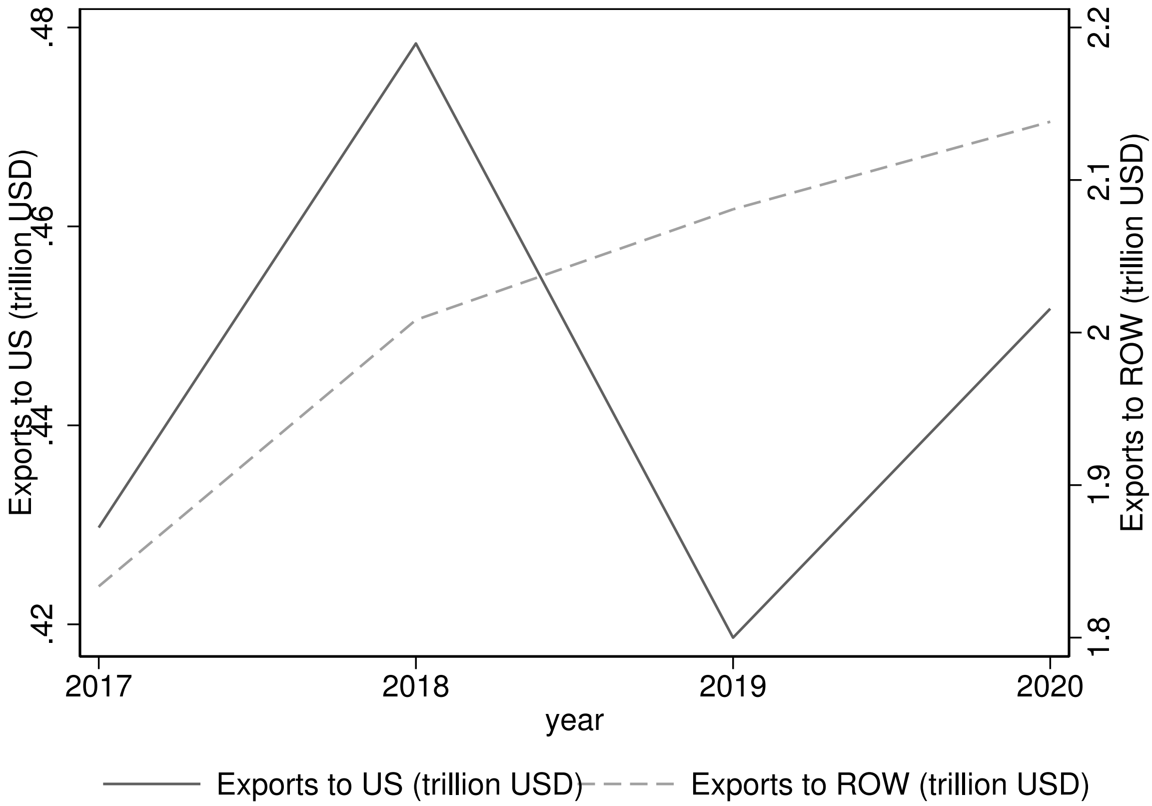

Figure 4.2: China's export to US and rest of world (ROW)

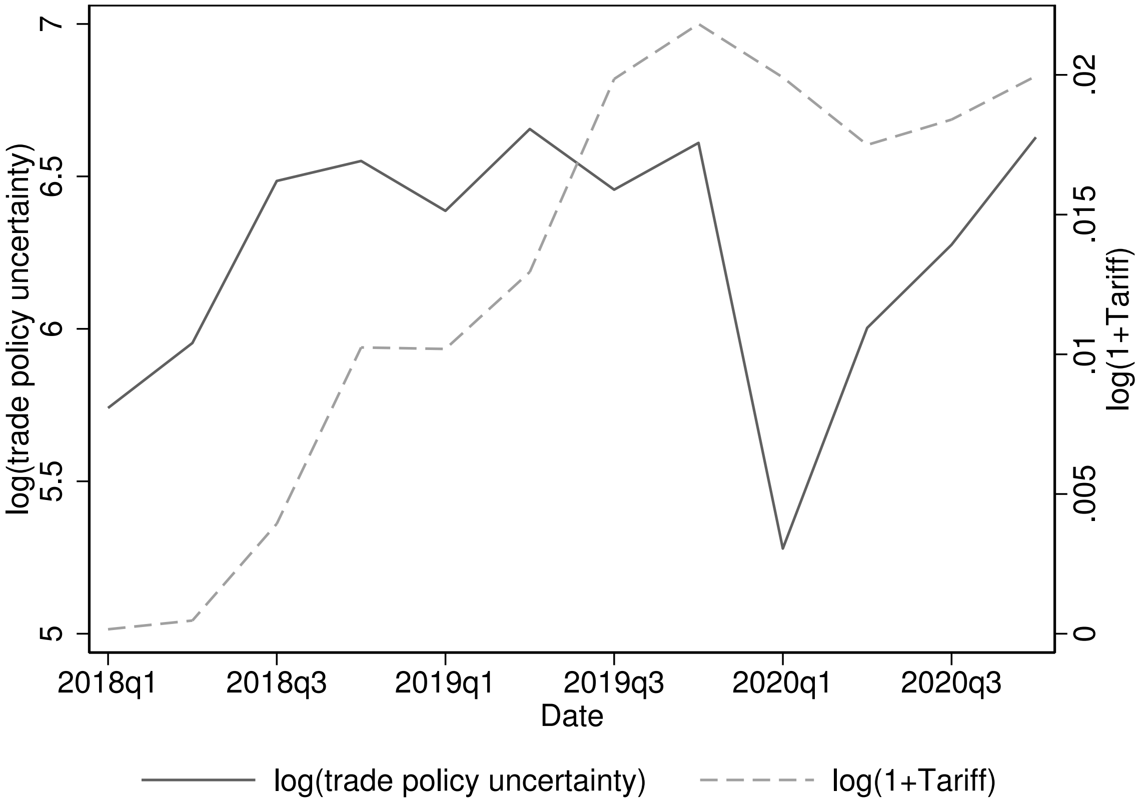

Figure 4.3: National tariff exposure and trade policy uncertainty

Table 4.1: Tariff shocks and commercial building rents

	Dependent variable: $\Delta log(Rent_{it})$				
	(1)	(2)	(3)	(4)	(5)
$\Delta Tariff_{i,t-1}^{US}$	-0.73*		-0.79*	-1.03**	-1.03**
	(0.39)		(0.42)	(0.48)	(0.41)
$\Delta Tariff_{i,t-1}^{CHN}$		0.09	0.36	0.84	0.84
		(0.61)	(0.63)	(0.73)	(0.85)
Year-Quarter FE	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y
Cluster by (it)	Y	Y	Y	Y	N
Cluster by (province)	N	N	N	N	Y
City W_{it}	N	N	N	Y	Y
adj. R^2	0.218	0.213	0.215	0.277	0.277
N	229	229	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff_{i,t-1}^{US}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff_{i,t-1}^{CHN}$ denotes city i's exposure to China's retaliatory tariff. (3) City W_{it} includes various factors such as log new commercial building construction, log exports, log imports, log population, log public budgetary revenue, log public budgetary expense, log GDP, log year-end deposit in banks, and log year-end loans in banks, to capture commercial building supply, overall openness, local economic development, and financial activities. (4)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (5) Standard errors in parentheses.

Table 4.2: Tariff shocks, commercial building rents, US dependence and trade diversification

	Dependent variable: $\Delta log(Rent_{it})$		
	(1)	(2)	(3)
$\Delta Tariff^{US}_{i,t-1}$	0.29	-0.32	-0.27
	(0.50)	(0.53)	(0.51)
$\times High^{exp,USdependence}_i$	-1.33*		
	(0.69)		
$\times High^{exp,ctry}_i$		0.45	
		(0.67)	
$\times High^{exp,prod}_i$			-0.25
			(0.73)
$\Delta Tariff^{CHN}_{i,t-1}$	0.39	0.96	0.99
	(0.72)	(0.75)	(0.73)
$\times High^{imp,USdependence}_i$	1.62		
	(10.37)		
$\times High^{imp,ctry}_i$		4.32	
		(11.12)	
$\times High^{imp,prod}_i$			4.40
			(12.93)
$High^{exp,USdependence}_i$	0.02		
	(0.06)		
$High^{exp,ctry}_i$		-0.11	
		(0.07)	
$High^{exp,prod}_i$			0.02
			(0.07)
$High^{imp,USdependence}_i$	0.01		
	(0.05)		
$High^{imp,ctry}_i$		0.04	
		(0.08)	
$High^{imp,prod}_i$			0.01
			(0.07)
Year-Quarter FE	Y	Y	Y
adj. R^2	0.049	0.035	0.024
N	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) City W_{it} includes various factors such as log new commercial building construction, log exports, log imports, log population, log public budgetary revenue, log public budgetary expense, log GDP, log year-end deposit in banks, and log year-end loans in banks, to capture commercial building supply, overall openness, local economic development, and financial activities. (4)$High_i$ is a dummy variable that divides the cities into two equal groups according to the national median value in the year 2016. Specifically, $High_i$ equals one if the city's US exports as a share of its total exports (to capture the dependence on the US), the number of destination markets, and the number of exported products (to capture trade diversification) is larger than the corresponding national median level. (5)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (6) Standard errors in parentheses.

Table 4.3: Tariff shocks, commercial building rents, and firm financial situation

	Dependent variable: $\Delta log(Rent_{it})$			
	(1)	(2)	(3)	(4)
$\Delta Tariff^{US}_{i,t-1}$	-0.13	-0.51	-0.19	-0.06
	(0.52)	(0.57)	(0.54)	(0.44)
$\times High^{profit}_i$	0.36			
	(0.69)			
$\times High^{tax}_i$		0.71		
		(0.71)		
$\times High^{VAT}_i$			0.15	
			(0.65)	
$\times High^{totalasset}_i$				7.03
				(6.28)
$\Delta Tariff^{CHN}_{i,t-1}$	0.43	0.29	0.36	1.19^{*}
	(0.75)	(0.86)	(0.73)	(0.61)
$\times High^{profit}_i$	3.35^{***}			
	(1.11)			
$\times High^{tax}_i$		1.59		
		(1.23)		
$\times High^{VAT}_i$			3.29^{***}	
			(1.14)	
$\times High^{totalasset}_i$				7.49
				(11.87)
$High^{profit}_i$	-0.16^{**}			
	(0.06)			
$High^{tax}_i$		-0.09		
		(0.06)		
$High^{VAT}_i$			-0.16^{**}	
			(0.07)	
$High^{totalasset}_i$				0.16^{**}
				(0.06)
Year-Quarter FE	Y	Y	Y	Y
City W_{it}	Y	Y	Y	Y
adj. R^2	0.112	0.066	0.103	0.140
N	229	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) City W_{it} includes various factors such as log new commercial building construction, log exports, log imports, log population, log public budgetary revenue, log public budgetary expense, log GDP, log year-end deposit in banks, and log year-end loans in banks, to capture commercial building supply, overall openness, local economic development, and financial activities. (4)$High_i$ is a dummy variable that divides the cities into two equal groups according to the national median value in the year 2016. Specifically, $High_i$ equals one if the city's profit, tax, value-added tax, and total current asset are larger than the corresponding national median level. (5)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (6) Standard errors in parentheses.

Table 4.4: Tariff shocks, commercial building rents, social stability and location

	Dependent variable: $\Delta log(Rent_{it})$		
	(1)	(2)	(3)
$\Delta Tariff^{US}_{i,t-1}$	-1.37**	0.39	-1.72***
	(0.54)	(0.45)	(0.61)
$\times High^{unemply,insurance}_i$	2.56***		
	(0.55)		
$\times High^{science,expenditure}_i$		-0.99	
		(0.73)	
$\times High^{East}_i$			1.59***
			(0.60)
$\times High^{Central}_i$			1.03
			(1.26)
$\Delta Tariff^{CHN}_{i,t-1}$	1.77**	0.20	1.03
	(0.79)	(0.69)	(0.88)
$\times High^{unemply,insurance}_i$	-1.19		
	(1.19)		
$\times High^{science,expenditure}_i$		2.85***	
		(0.98)	
$\times High^{East}_i$			0.32
			(1.50)
$\times High^{Central}_i$			0.58
			(1.05)
$High^{unemply,insurance}_i$	-0.11		
	(0.08)		
$High^{internet,subscription}_i$		-0.06	
		(0.07)	
Year-Quarter FE	Y	Y	Y
City W_{it}	Y	Y	Y
adj. R^2	0.075	0.063	0.281
N	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) City W_{it} includes various factors such as log new commercial building construction, log exports, log imports, log population, log public budgetary revenue, log public budgetary expense, log GDP, log year-end deposit in banks, and log year-end loans in banks, to capture commercial building supply, overall openness, local economic development, and financial activities. (4)$High_i$ is a dummy variable that divides the cities into two equal groups according to the national median value in the year 2016. Specifically, in columns (1) and (2), $High_i$ equals one if the city's unemployment insurance coverage as a share of its population, and public financial budgetary expenditure on science as a share of total expenditure is larger than the corresponding national median level. In column (3), $High_i = 1$, 2, or 3 if the city is located in East, Central, or Northeast region, respectively. (5)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (6) Standard errors in parentheses.

A4.A Robustness check

Table A4.A.1: Basic pre-trend checks

Dependent variable:	$\Delta log(Rent_{i,t-1})$ (1)	$\Delta log(Rent_{i,t-2})$ (2)	$\Delta log(Rent_{i,t-3})$ (3)
$\Delta Tariff^{US}_{i,t-1}$	-0.73	-0.47	-0.03
	(0.52)	(0.70)	(0.90)
$\Delta Tariff^{CHN}_{i,t-1}$	0.75	0.65	0.13
	(0.95)	(0.97)	(1.40)
Year-Quarter FE	Y	Y	Y
City FE	Y	Y	Y
City W_{it}	Y	Y	Y
adj. R^2	0.237	0.233	0.230
N	262	262	262

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) City W_{it} includes various factors such as log new commercial building construction, log exports, log imports, log population, log public budgetary revenue, log public budgetary expense, log GDP, log year-end deposit in banks, and log year-end loans in banks, to capture commercial building supply, overall openness, local economic development, and financial activities. (4)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (5) Standard errors in parentheses.

Table A4.A.2: Additional MFN shock, and alternative controls

	Dependent variable: $\Delta log(Rent_{it})$			
	(1)	(2)	(3)	(4)
$\Delta Tariff^{US}_{i,t-1}$	-0.91**	-1.04**	-1.10*	-1.04**
	(0.43)	(0.47)	(0.56)	(0.47)
$\Delta Tariff^{CHN}_{i,t-1}$	0.29	0.81	1.52	-1.55
	(0.61)	(0.75)	(1.56)	(0.97)
$\Delta MFNTariff^{CHN}_{i,t-1}$	-6.73	-1.11		
	(6.09)	(6.87)		
Year-Quarter FE	Y	Y	Y	Y
City FE	Y	Y	Y	Y
City W_{it}	N	Y	Y	N
City $W_{i0}\times$ Year-Quarter FE	N	N	N	Y
adj. R^2	0.213	0.273	0.497	0.078
N	229	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) City W_{it} includes various factors such as log new commercial building construction, log exports, log imports, log population, log public budgetary revenue, log public budgetary expense, log GDP, log year-end deposit in banks, and log year-end loans in banks, to capture commercial building supply, overall openness, local economic development, and financial activities. (4)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (5) Standard errors in parentheses.

A4.B Full results

Table A4.B.1: Tariff shocks and commercial building rents

	Dependent variable: $\Delta log(Rent_{it})$				
	(1)	(2)	(3)	(4)	(5)
$\Delta Tariff^{US}_{i,t-1}$	-0.73*		-0.79*	-1.03**	-1.03**
	(0.39)		(0.42)	(0.48)	(0.41)
$\Delta Tariff^{CHN}_{i,t-1}$		0.09	0.36	0.84	0.84
		(0.61)	(0.63)	(0.73)	(0.85)
$log(newconstruction)_{i,t}$				0.41	0.41*
				(0.28)	(0.24)
$log(exports)_{i,t}$				0.18***	0.18*
				(0.07)	(0.10)
$log(imports)_{i,t}$				-0.05	-0.05
				(0.06)	(0.10)
$log(yearendpopulation)_{i,t}$				-0.76***	-0.76**
				(0.29)	(0.37)
$log(GDP)_{i,t}$				-0.58***	-0.58***
				(0.21)	(0.18)
$log(publicbudgetaryrevenue)_{i,t}$				0.42	0.42
				(0.29)	(0.40)
$log(publicbudgetaryexpenses)_{i,t}$				-0.36	-0.36
				(0.30)	(0.37)
$log(yearenddeposits)_{i,t}$				0.88*	0.88
				(0.48)	(0.64)
$log(yearendloans)_{i,t}$				0.43	0.43
				(0.43)	(0.73)
Constant	0.06**	0.01	0.05	-33.85*	-33.85
	(0.03)	(0.03)	(0.03)	(20.47)	(20.80)
Year-Quarter FE	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y
Clustered by (it)	Y	Y	Y	Y	N
Clustered by (province)	N	N	N	N	Y
adj. R^2	0.218	0.213	0.215	0.277	0.277
N	229	229	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (4) Standard errors in parentheses.

TABLE A4.B.3: Tariff shocks, commercial building rents, US dependence and trade diversification

	Dependent variable: $\Delta log(Rent_{it})$		
	(1)	(2)	(3)
$\Delta Tariff^{US}_{i,t-1}$	0.29	-0.32	-0.27
	(0.50)	(0.53)	(0.51)
$\times High^{exp,USdependence}_i$	-1.33*		
	(0.69)		
$\times High^{exp,ctry}_i$		0.45	
		(0.67)	
$\times High^{exp,prod}_i$			-0.25
			(0.73)
$\Delta Tariff^{CHN}_{i,t-1}$	0.39	0.96	0.99
	(0.72)	(0.75)	(0.73)
$\times High^{imp,USdependence}_i$	1.62		
	(10.37)		
$\times High^{imp,ctry}_i$		4.32	
		(11.12)	
$\times High^{imp,prod}_i$			4.40
			(12.93)
$High^{exp,USdependence}_i$	0.02		
	(0.06)		
$High^{exp,ctry}_i$		-0.11	
		(0.07)	
$High^{exp,prod}_i$			0.02
			(0.07)
$High^{imp,USdependence}_i$	0.01		
	(0.05)		
$High^{imp,ctry}_i$		0.04	
		(0.08)	
$High^{imp,prod}_i$			0.01
			(0.07)
$log(newconstruction)_{i,t}$	-0.04	-0.02	-0.01
	(0.04)	(0.03)	(0.04)
$log(exports)_{i,t}$	0.02	0.01	0.01
	(0.02)	(0.02)	(0.02)
$log(imports)_{i,t}$	-0.03*	-0.04**	-0.03*
	(0.02)	(0.02)	(0.02)
$log(yearendpopulation)_{i,t}$	0.01	0.00	-0.00
	(0.03)	(0.03)	(0.03)
$log(GDP)_{i,t}$	0.05	0.07	0.06
	(0.07)	(0.08)	(0.07)
$log(publicbudgetaryrevenue)_{i,t}$	-0.04	-0.04	-0.06
	(0.06)	(0.05)	(0.05)
$log(publicbudgetaryexpenses)_{i,t}$	0.02	0.06	0.07
	(0.06)	(0.06)	(0.06)
$log(yearenddeposits)_{i,t}$	0.22***	0.21***	0.20***
	(0.07)	(0.08)	(0.08)
$log(yearendloans)_{i,t}$	-0.24***	-0.26***	-0.24***
	(0.07)	(0.08)	(0.08)
Constant	0.62	0.35	0.31
	(1.07)	(1.04)	(1.06)
Year-Quarter FE	Y	Y	Y
adj. R^2	0.049	0.035	0.024
N	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) $High_i$ is a dummy variable dividing the cities into two equal groups based on the national median value of 2016, indicating higher dependence on the US, greater trade diversification, and more destination markets. (4)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Table A4.B.4: Tariff shocks, commercial building rents, and firm financial situation

	Dependent variable: $\Delta log(Rent_{it})$			
	(1)	(2)	(3)	(4)
$\Delta Tariff^{US}_{i,t-1}$	-0.13	-0.51	-0.19	-0.06
	(0.52)	(0.57)	(0.54)	(0.44)
$\times High^{profit}_i$	0.36			
	(0.68)			
$\times High^{tax}_i$		0.71		
		(0.71)		
$\times High^{VAT}_i$			0.15	
			(0.65)	
$\times High^{totalasset}_i$				7.03
				(6.28)
$\Delta Tariff^{CHN}_{i,t-1}$	0.43	0.29	0.364	1.19*
	(0.75)	(0.86)	(0.73)	(0.61)
$\times High^{profit}_i$	3.35***			
	(1.11)			
$\times High^{tax}_i$		1.59		
		(1.23)		
$\times High^{VAT}_i$			3.29***	
			(1.14)	
$\times High^{totalasset}_i$				7.49
				(11.87)
$High^{profit}_i$	-0.16**			
	(0.06)			
$High^{tax}_i$		-0.09		
		(0.06)		
$High^{VAT}_i$			-0.16**	
			(0.07)	
$High^{totalasset}_i$				0.16**
				(0.06)
$log(newconstruction)_{i,t}$	-0.01	-0.03	-0.03	-0.03
	(0.04)	(0.04)	(0.03)	(0.04)
$log(exports)_{i,t}$	0.01	0.02	0.01	0.04***
	(0.02)	(0.02)	(0.02)	(0.01)
$log(imports)_{i,t}$	-0.03*	-0.04**	-0.03**	-0.05***
	(0.02)	(0.02)	(0.01)	(0.02)
$log(yearendpopulation)_{i,t}$	-0.00	-0.00	-0.00	0.01
	(0.03)	(0.03)	(0.03)	(0.03)
$log(GDP)_{i,t}$	0.05	0.07	0.08	-0.01
	(0.07)	(0.06)	(0.07)	(0.07)
$log(publicbudgetaryrevenue)_{i,t}$	-0.06	-0.06	-0.06	-0.10*
	(0.05)	(0.05)	(0.05)	(0.05)
$log(publicbudgetaryexpenses)_{i,t}$	0.10	0.07	0.09	0.06
	(0.06)	(0.06)	(0.06)	(0.06)
$log(yearenddeposits)_{i,t}$	0.16**	0.19***	0.17***	0.23***
	(0.07)	(0.07)	(0.06)	(0.07)
$log(yearendloans)_{i,t}$	-0.22***	-0.24***	-0.24***	-0.22***
	(0.07)	(0.07)	(0.07)	(0.06)
Constant	0.29	0.52	0.54	1.13
	(1.01)	(1.07)	(1.02)	(0.95)
Year-Quarter FE	Y	Y	Y	Y
adj. R^2	0.112	0.066	0.103	0.140
N	229	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) $High_i$ is a dummy variable that divides the cities into two equal groups based on the national median value of 2016, indicating higher profit, tax, value-added tax, and total current assets compared to the national median level. (4)* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (6) Standard errors in parentheses.

Table A4.B.5: Tariff shocks, commercial building rents, social stability and location

	Dependent variable: $\Delta log(Rent_{it})$		
	(1)	(2)	(3)
$\Delta Tariff^{US}_{i,t-1}$	-1.37**	0.39	-1.72***
	(0.54)	(0.45)	(0.61)
$\times High^{unemply,insurance}_i$	2.56***		
	(0.55)		
$\times High^{science,expenditure}_i$		-0.99	
		(0.73)	
$\times High^{East}_i$			1.59***
			(0.60)
$\times High^{Central}_i$			1.03
			(1.26)
$\Delta Tariff^{CHN}_{i,t-1}$	1.77**	0.20	1.03
	(0.79)	(0.69)	(0.88)
$\times High^{unemply,insurance}_i$	-1.19		
	(1.19)		
$\times High^{science,expenditure}_i$		2.85***	
		(0.98)	
$\times High^{East}_i$			0.32
			(1.50)
$\times High^{Central}_i$			0.58
			(1.05)
$High^{unemply,insurance}_i$	-0.11		
	(0.08)		
$High^{internet,subscription}_i$		-0.06	
		(0.07)	
$log(newconstruction)_{i,t}$	-0.03	-0.03	0.44*
	(0.04)	(0.04)	(0.26)
$log(exports)_{i,t}$	0.02	0.02	0.13
	(0.02)	(0.02)	(0.09)
$log(imports)_{i,t}$	-0.04**	-0.04**	-0.00
	(0.02)	(0.02)	(0.07)
$log(yearendpopulation)_{i,t}$	-0.01	0.01	-0.90***
	(0.03)	(0.03)	(0.28)
$log(GDP)_{i,t}$	0.09	0.05	-0.47
	(0.07)	(0.06)	(0.30)
$log(publicbudgetaryrevenue)_{i,t}$	-0.11**	-0.06	0.20
	(0.06)	(0.05)	(0.27)
$log(publicbudgetaryexpenses)_{i,t}$	0.08	0.07	-0.26
	(0.06)	(0.06)	(0.26)
$log(yearenddeposits)_{i,t}$	0.23***	0.15**	0.67
	(0.08)	(0.07)	(0.63)
$log(yearendloans)_{i,t}$	-0.24***	-0.19***	0.24
	(0.07)	(0.07)	(0.53)
Constant	-0.05	0.03	-16.67
	(1.08)	(1.22)	(31.51)
Year-Quarter FE	Y	Y	Y
adj. R^2	0.075	0.063	0.281
N	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) $High_i$ is a dummy variable dividing the cities into two equal groups based on the national median value of 2016. In columns (1) and (2), $High_i = 1$ if the city's unemployment insurance coverage as a share of its population and public financial budgetary expenditure on education as a share of total expenditure exceed the corresponding national median level. In column (3), $High_i = 1$, 2, or 3 if the city is located in East, Central, or Northeast region, respectively.

Table A4.B.6: Basic pre-trend checks

Dependent variable:	$\Delta log(Rent_{i,t-1})$ (1)	$\Delta log(Rent_{i,t-2})$ (2)	$\Delta log(Rent_{i,t-3})$ (3)
$\Delta Tariff_{i,t-1}^{US}$	-0.73	-0.47	-0.03
	(0.52)	(0.70)	(0.90)
$\Delta Tariff_{i,t-1}^{CHN}$	0.75	0.65	0.13
	(0.95)	(0.97)	(1.40)
$log(newconstruction)_{i,t}$	0.37	0.36	0.38
	(0.220)	(0.23)	(0.26)
$log(exports)_{i,t}$	0.20*	0.20*	0.20*
	(0.09)	(0.09)	(0.09)
$log(imports)_{i,t}$	-0.09	-0.09	-0.09
	(0.09)	(0.09)	(0.09)
$log(yearendpopulation)_{i,t}$	-0.71**	-0.70*	-0.71**
	(0.28)	(0.30)	(0.29)
$log(GDP)_{i,t}$	-0.53**	-0.52**	-0.53**
	(0.21)	(0.210)	(0.22)
$log(publicbudgetaryrevenue)_{i,t}$	0.31	0.32	0.37
	(0.35)	(0.33)	(0.35)
$log(publicbudgetaryexpenses)_{i,t}$	-0.22	-0.22	-0.24
	(0.35)	(0.35)	(0.36)
$log(yearenddeposits)_{i,t}$	0.96	0.95	0.88
	(0.58)	(0.62)	(0.63)
$log(yearendloans)_{i,t}$	0.47	0.48	0.49
	(0.65)	(0.71)	(0.72)
Constant	-40.22	-40.62	-39.53
	(21.85)	(22.33)	(22.03)
Year-Quarter FE	Y	Y	Y
City FE	Y	Y	Y
adj. R^2	0.237	0.233	0.230
N	262	262	262

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff_{i,t-1}^{US}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff_{i,t-1}^{CHN}$ denotes city i's exposure to China's retaliatory tariff. (3) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (4) Standard errors in parentheses.

Table A4.B.7: Additional MFN shock

	Dependent variable: $\Delta log(Rent_{it})$		
	(1)	(2)	(3)
$\Delta Tariff^{US}_{i,t-1}$	-0.91**	-1.04**	-1.10*
	(0.43)	(0.47)	(0.56)
$\Delta Tariff^{CHN}_{i,t-1}$	0.29	0.81	1.52
	(0.61)	(0.75)	(1.56)
$\Delta MFNTariff^{CHN}_{i,t-1}$	-6.73	-1.11	
	(6.09)	(6.87)	
$log(newconstruction)_{i,t}$		0.41	0.69***
		(0.28)	(0.22)
$log(exports)_{i,t}$		0.18***	0.12
		(0.07)	(0.12)
$log(imports)_{i,t}$		-0.05	-0.03
		(0.06)	(0.10)
$log(yearendpopulation)_{i,t}$		-0.76***	-0.73**
		(0.29)	(0.33)
$log(GDP)_{i,t}$		-0.57**	-0.40***
		(0.22)	(0.15)
$log(publicbudgetaryrevenue)_{i,t}$		0.42	0.40
		(0.30)	(0.33)
$log(publicbudgetaryexpenses)_{i,t}$		-0.36	-0.60
		(0.29)	(0.52)
$log(yearenddeposits)_{i,t}$		0.87*	1.30
		(0.491)	(1.08)
$log(yearendloans)_{i,t}$		0.42	0.14
		(0.45)	(0.71)
Constant	0.04	-33.70	-36.59
	(0.03)	(20.81)	(45.95)
Year-Quarter FE	Y	Y	
City FE	Y	Y	
adj. R^2	0.213	0.273	0.497
N	229	229	229

Note: (1) The dependent variable is the change in log commercial rents yuan per square meter in city i between time $t-4$ and t. (2) $Tariff^{US}_{i,t-1}$ represents exposure to US tariffs, calculated as a weighted average of each city's exported products to the US; $Tariff^{CHN}_{i,t-1}$ denotes city i's exposure to China's retaliatory tariff. (3) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. (4) Standard errors in parentheses.

CHAPTER 5 Conclusion

5.1 Main findings

This thesis presents an empirical examination of the impacts of social media sentiment, place-based policy, and tariff shocks on China's city-level real estate market. The analytical results not only contribute to existing literature but also furnish novel insights beneficial to policymakers, especially in developing nations.

Three principal discoveries emerge from this research in the context of development economics and its relation to the real estate market: First, social media sentiment can forecast China's city-level house price growth for up to six quarters. This phenomenon is not attributable to unseen fundamentals or shifts in policy but is rooted in social learning and "animal spirits".

Second, taking the rezoning in Beijing as an example, we find that rezoning elevates house prices within the newly rezoned districts. Conversely, prices decline in the old neighboring districts. In both cases, the impacts are localized. Furthermore, rezoning hurts economically disadvantaged groups and culminates in an aggregate loss in housing market wealth.

Finally, the US-China tariff war reduces commercial real estate rent growth in 34 large Chinese cities, likely through diminished profits and high reliance on the US market. The subsequent sections detail the contributions and offer policy recommendations for each empirical chapter.

5.2 Social media sentiment and house prices: Evidence from 35 Chinese cities

Chapter 2 introduces a novel housing market social media sentiment index derived from over 800,000 daily posts between 2010 and 2020 on Weibo, a dominant Chinese social media platform. This sentiment index significantly predicts city-specific house price fluctuations up to 6 quarters ahead across 35 major cities, even after accounting for fundamental economic drivers of these price changes. Specifically, a 1 percent increase in the accumulated lagged sentiment leads to an approximate 0.81 percent increase in house prices, ceteris paribus.

Following Soo (2018), we initially demonstrate that our result is more than merely due to unmeasured fundamentals. Next, we employ a difference-in-differences analysis on a sequence of home purchase restriction policies enacted by the Chinese government. This study indicates that neither these policies' enforcement, nor subsequent removal can account for our baseline results' magnitude or size.

Furthermore, we identify several theoretically grounded drivers of cross-sectional effects. Predictably, cities with better information accessibility exhibit a stronger predictive power of sentiment. We identify limited support for the proposition that more speculative markets amplify sentiment's impact. Notably, our findings corroborate the assertions in Tetlock (2007), Loughran & McDonald (2011) that a negative sentiment tone wields a more potent predictive capability on price shifts.

We finish with a battery of further robustness checks. We assign weights to posts based on their popularity when formulating the sentiment index and adopt diverse aggregation methods for city-level posts. Our analysis also integrates population weights in Weighted Least Squares regression and an alternative measure of tone using adverbs.

In summation, our findings endorse theories suggesting social media sentiment mirrors an animal spirit metric, significant across models from Keynes (1936) to Acharya et al. (2021), or serves as an indicator of social learning.

5.3 Rezoning and house prices: Evidence from Beijing, China

The city-county merger is China's most important national spatial policy since the county-to-city upgrade program. It is intended to address the ineffective and inefficient county system. The county systems' low levels of economic development, poor infrastructure, and a lack of skilled workers lead to huge efficiency and welfare losses. This program aims to generate neighborhood revival. Yet, we find conflicting evidence on this view in Chapter 3.

Chapter 3 examines the merger of the Miyun and Yanqing counties' into Beijing districts on both local and adjacent housing markets. Adopting a difference-in-differences methodology, the analysis in this chapter indicates that house prices in the newly rezoned districts increases following the formal establishment of the merger in 2015. However, concurrently, there is a decline in house prices in the neighboring districts.

The evidence also highlights that the impact of rezoning is stronger as the proximity to the shared border increases. This effect remains concentrated within the first quartile distance and does not permeate wider regions. Furthermore, this study reveals a heterogeneity in the market response based on housing affordability. Specifically, the rezoning lead to a surge in the prices of high-priced properties in the new districts and a decrease in the prices of the cheaper homes in the older border areas. A plausible explanation for this differential is the varying financial capabilities of the buyers. Those relocating from older border areas to the new districts may operate on constrained budgets. Conversely, those transitioning into the new districts from other parts of Beijing might possess higher budgets, especially given the relatively lower property prices in these areas, thereby exhibiting a preference for higher-end properties.

In conclusion, this study provides valuable insights into the impact of the merger on house prices. The results show that rezoning has significantly impacted the distribution of housing.

5.4 Snowball effect of the US-China trade war: Evidence from China's commercial building markets

Chapter 4 delves into the snowball effect of the US-China trade war on China's commercial real estate rents. Primary results manifest a significant negative impact of US tariffs on city-level commercial rents growth, while China's countering tariff displayed an insubstantial influence. Specifically, a 1 percentage point increase in US tariff exposure results in a 1.03 percent contraction in commercial building rent growth after one quarter, holding other factors constant.

We proceed to dissect heterogeneity in rent reactions to these tariff shocks, discovering that cities primarily reliant on US markets endure a more pronounced negative rent impact. Further investigations illuminate that cities with robust initial financial metrics such as the share of profit, tax, value-added tax, and total assets to their GDP experience a smaller negative rent influence. Better innovation, social stability, and location also help reduce the trade war's negative impact, while trade diversification has no impact on rent responses.

In conclusion, this study furnishes invaluable perspectives on trade policy shifts' indirect effects on China's commercial building rents, highlighting the pivotal role of export tariff shocks modulated by US reliance and corporate financial standings. Policymakers from both parts should consider such negative direct and indirect impacts before implementing such trade conflicts.

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