Essays on the Role of Durables and Financial Frictions in Business Cycles and International Trade

By

Dong Cheng

Dissertation

Submitted to the Faculty of the

Graduate School of Vanderbilt University

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

in

Economics

August 10, 2018

Nashville, Tennessee

Approved:

Mario J. Crucini, Ph.D.

Gregory W. Huffman, Ph.D.

Atsushi Inoue, Ph.D.

Hyunseung Oh, Ph.D.

David C. Parsley, Ph.D.

To my parents, who supported me all the things great and small.

ACKNOWLEDGMENTS

I would like to extend sincere thanks to the many people, in many places, who generously contributed to the work presented in this dissertation.

Special mention goes to my caring and enthusiastic supervisor, Mario J. Crucini. He provided invaluable encouragement and guidance in my writing of this dissertation. My Ph.D. has been a wonderful experience with his help. I thank Mario wholeheartedly, not just for the tremendous academic guidance, but also for giving me a vast of opportunities. He supported me many times using his research funds, which financed my trips for presenting papers, attending conferences, interacting with great economists, and exploring the other parts of the world. I also learnt so much by working with him on our joint projects. He is always ready to help and keeps optimistic in face of all challenges. His kindness and positive attitude have greatly encouraged me to overcome difficulties that emerged in the process of writing this dissertation.

I am also appreciative to Gregory W. Huffman, Atsushi Inoue, Hyunseung Oh and David C. Parsley, who served as my dissertation committee members. They provided great comments and feedbacks during my presentations and academic talks between us. I even got very useful advice from then on how to behave well in job market interviews and flyouts.

Profound gratitude goes to Zhongzhong Hu, Joel Rodrigue, Yong Tan and Jian Yu, who are my coauthors in multiple papers. I learnt a lot of innovative ideas, cutting-edge skills, positive attitudes and the spirits of teamwork from them. Special mention goes to Joel Rodrigue, who helped me tremendously by providing comments on my dissertation and exchanging ideas for our joint work. He showed genuine interests to work with Ph.D. students like me and always encouraged us to explore the frontier.

I would also want to thank many other professors at Renmin University of China and Vanderbilt University for encouraging and supporting me. My M.A. thesis advisor, Yanbin Chen, inspired me to pursue a Ph.D. Tong Li and Mattias Polborn supported me a lot in my searching for jobs. Federico Gutierrez and Pedro Sant'anna provided great comments during my job practice talk.

Sincere thanks also go to a great number of friends, including Hayri Alper Arslan, Hui (Gracie) Hao and many others, for their help and encouragement. I learnt a lot by interacting with them. Moreover, gratitude goes to the Walter M. Noel Dissertation Fellowship and a Kirk Dornbush Summer Research Grant for financial assistance.

Finally, but by no means least, massive thanks go to my family, for their almost unbelievable support. Bean and our lovely cat Dill have always been standing with me and giving me enormous support. My mom and dad are the greatest parents for me and give my endless love. They are the most important people in my world and I dedicate this dissertation to them.

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INTRODUCTION

Financial frictions are important features of the economy and yet their role in shaping business cycle dynamics and the pattern of international trade is incompletely understood. The role of financial friction is even more crucial when durables are explicitly built into the macroeconomic models because they are big-ticket items. Durables also amplify business cycles through the stock-flow relation because a given percentage change in the desired stock requires a much larger percentage change in the flow. In the globalized world, trade flows, especially durables, are substantially more volatile than domestic economic aggregates like GDP. Therefore, investigating the role of durables and financial frictions in closed and open economy settings is very important for understanding economic fluctuations and trade dynamics. Using rich microeconomic and historical data, this dissertation proposes new empirical and theoretical approaches to quantify the importance of durables and financial frictions in three different settings. The first chapter explores the response of non-shelter consumption to shocks from the housing sector, where homes are durable goods that have the unique feature that they also serve a collateral for loans. We develop a novel instrumental variable for local housing price movements to achieve identification. We pay close attention to the role of collateral constraints and show that their presence increases the consumption response. In the second chapter, we investigate the diffusion of a durable good, the automobile, from the United States to the rest of world through international trade. We collect archival data to document the diffusion pattern, and then rely on international price frictions, including markups, tariffs, trade costs, and the Penn effect, to explain the much lower adoption rates of automobiles abroad, compared to the U.S. The third chapter examines the heterogeneous and time-varying effects of finance on firms' exporting performance. We apply panel-data DID and DDD methods to comprehensive microeconomic data from China's manufacturing sector. We show that both internal and external finance have positive impacts on the intensive margin of firms' exports when firms switch from indirect to direct exporting, and even larger positive impacts when the switch occurs in the post-WTO accession period during which government restrictions on direct exporting was removed.

Chapter 1 studies the response of non-housing consumption to housing price movements in urban China, which has been enjoying a real estate boom ever since 2003. Using Urban Household Survey data over the period 2002-2009, we estimate an elasticity of consumption with respect to housing price of 0.06 to 0.07 for homeowners. Moreover, we find that the average marginal propensity to consume out of housing wealth is 0.025 to 0.03. The estimates are economically significant because they imply that the increase in consumption induced by housing appreciation during 2002-2009 amounts to 14%-17% of current consumption in 2009 for a representative homeowner. We employ a novel instrumental variable associated with China's higher-education expansion between 1998 and 2005 to ensure that these estimates are causal effects. As for renters, we show that their consumption response to housing shocks is insignificant. We further reveal that the marginal propensity to consume is larger for homeowners residing in poorer and more collateral constrained cities. Greater durability or a higher income elasticity of a consumption category amplifies homeowners' consumption response to housing shocks.

The first half of the twentieth century provides an unparalleled opportunity to explore the impact of technological innovation in the worldwide diffusion of a new and highly traded good, automobile, because the United States was dominant in both production and trade of passenger automobiles. In Chapter 2, we scrape historical data on quantity and value of passenger vehicles exported from the United States to approximately 80 destinations, annually from 1913 to 1940. We model the rise of the automobile from global obscurity to a fixed point which depends on per capita national wealth, and with the transition path depending on the evolution of the relative price of the automobile and its pass-through to destination markets. We then conduct wedge accounting for international price frictions, including markups, tariffs, trade costs, and the Penn effect, to explain the gaps of adoption levels between the U.S. and other countries.

Chapter 3 investigates the heterogeneous and time-varying effects of financial credits on firm level export performance. China's WTO accession leads to trade deregulation, which allows domestic private firms with low registered capital to switch their export mode from indirect (through intermediaries) to direct exporting. Using a comprehensive data set of Chinese manufacturing firms and employing a difference-in-differences approach (DID), we find that financial credits improve firm-level exports and productivity more for firms that switch from indirect to direct exporting than continuous indirect exporting firms. Further, we employ a difference-in-difference-in-differences (DDD) approach and find that improvements in firm-level internal and external finance have larger positive impacts on firm export values in the post-WTO accession period, conditioning on the firm switching from indirect to direct exporting. The time-varying impact may suggest an export distortion in China before its WTO accession, which prevents more productive but financially constrained private domestic firms from direct exporting.

CHAPTER 1

Housing Boom and Non-housing Consumption: Evidence from Urban Households in China

1.1 Introduction

When homeowners experience an increase in the market value of their homes due to macroeconomic or local sources of variation, how does their non-housing consumption respond? This question is important for understanding how wealth shocks translate into business cycle fluctuations, and has strong policy implications with regard to large swings in asset prices. Existing studies (e.g. Mian et al., 2013; Kaplan et al., 2016) show that there is a large consumption response to housing price movements in developed economies, especially during economic crises.[1] However, less is known about the consumption response in a developing economy like China that has been enjoying a decade-long boom in housing markets.[2]

In this paper, we estimate the causal impact of housing shocks on consumption in China. Two features make China a quite different setting from developed economies for evaluating the consumption response. First, less developed financial markets force households to save a large share of current income against future uncertainties, hence limiting the extent of consumption response to perceived appreciation in home values. Second, following a decade of consistent price appreciation in housing markets, households may expect a continuation and this amplifies consumption response to the current price rise. These competing forces (among others) may lead to either a larger or smaller consumption response than those found in developed economies. Hence, our work avoids the extrapolation of existing estimates to economic environments where they may be inappropriate while providing additional lessons on the general boom-bust cycle of housing.

[1]Mian et al. (2013) refer to housing price movements as housing shocks to reflect the fact that they explore how exogenous variation in housing price movements affects household consumption using the instrumental variable method. We follow their terminology in this study because we also construct an exogenous source of variation in housing price movements.

[2]Since 2003, Chinese housing markets have been growing rapidly and steadily. Glaeser et al. (2017) document that, during 2003-2014, real housing prices in China rose by over 10% per year, and Chinese real estate developers added around 100 billion square feet of residential space. Even the U.S. housing boom between 1996 and 2006 pales in comparison to the great Chinese housing boom, with real housing prices growing by 5% per year.

Exploring the casual effect of housing shocks on consumption faces the challenge of isolating multiple channels that could explain any observed relationship between the two variables. One branch of papers have employed a calibrated model of consumption and housing to evaluate the consumption response, and find that the elasticity of consumption with respect to housing price is positive and significant. Early studies like Campbell and Cocco (2007) and Attanasio et al. (2011) employed variants of the partial equilibrium life cycle model to explore consumption responses, while recent papers (e.g. Berger et al., 2017; Kaplan et al., 2017) have switched to introducing housing into (general equilibrium) incomplete market models with heterogeneous agents. Another branch of papers are more reduced-form, and while differences in estimation strategies and data leads to a broad range of estimates for the consumption response, the median response across these studies is large, positive, and statistically significant. Early reduced-form studies using aggregate data (e.g. Carroll et al., 2011) found it hard to construct a reliable source of exogenous variation in housing price movements to isolate the impact of housing shocks on consumption. Recently, relying on the housing supply elasticity constructed in Saiz (2010) as an instrumental variable for local housing price movements, Mian et al. (2013) improved the situation when exploiting the consumption response with data at county and zip-code levels.[3]

The challenge of identification also plagues our empirical work when we extend the studies on consumption effects of housing shocks to a developing economy like China that has been enjoying a decade-long housing boom. In **Figure A.1**, we plot correlation patterns between real growth in county-level consumption, housing prices, and disposable income during 2002-2009. Panel (A) clearly displays that real consumption and real housing prices rose dramatically in urban China between 2002 and 2009. The two growth rates also show strong positive correlation across locations, suggesting a positive effect of housing appreciation on consumption. However, the ob-

[3]Though recent reduced-form studies explore the consumption response to both housing price and home value (housing wealth) movements, they focus on changes in home value driven by housing price movements (e.g. Mian et al., 2013; Kaplan et al., 2016). Hence, the changes in quantity of housing are not considered. It makes sense to isolate changes in home value driven by housing price movements because housing prices tend to be more exogenous than housing quantity for households once they buy homes. This treatment also implies that it is appropriate to instrument home value changes with the instrumental variable for housing price movements, as Mian et al. (2013) have done in their study.

served correlation may be ascribed to a common outside factor that moves both the growth rates of consumption and housing prices in the same direction. For instance, Panel (B) indicates that the spectacular income growth could boost consumption and housing price simultaneously through increased demand for non-housing consumption and housing services. As a consequence, any unobserved permanent income shock might lead to the positive correlation between consumption and housing price growth. To avoid misattribution like this, we need to find an exogenous source of variation in housing price movements to clearly identify the causal effect of housing shocks on consumption. It is beyond the scope of this paper to construct geography-based long-run housing supply elasticities as Saiz (2010) using geographic information system (GIS) techniques. Instead, we develop an alternative source of exogenous variation in housing price changes that takes advantage of an arguably natural experiment in China and originates from the demand side of housing markets.

We exploit China's higher-education expansion between 1998 and 2005 to construct an instrumental variable for housing price movements during 2002-2009.[4] The higher-education expansion is conceivably a natural experiment and exogenous to concurrent economic growth trend in China (Che and Zhang, 2017). We construct college enrollment expansion shock as an instrumental variable for city-level housing price movements between 2002 and 2009 through the multiplication of initial number of city-level higher-education institutions in 1998 (prior to 1999 when higher-education expansion was introduced) and province-level college enrollment expansion during 1998-2005.[5] The choice of initial number of higher-education institutions prior to college enrollment expansion mitigates the endogeneity concern that more colleges are built for expansion in areas that are expected to grow faster, while the province-level college enrollment expansion further helps us to avoid endogenous expansions at the city level.

Our instrumental variable is predictive of local (city-level) housing price increases over the

[4]Our approach to constructing an instrumental variable for housing price movements is inspired by the recent work of Chen and Zhang (2016) who utilize the same higher-education expansion to explain housing price increases in China. They find that the college enrollment expansion leads to an increase in local demand for housing that can account for around 12%-20% of housing price changes in China during 2002-2009.

[5]The timeline of events is plotted in Section F of this paper's **Online Appendix**. It clearly defines our sample period, the period of higher-education expansion, and the period we employ to construct our instrumental variable.

period 2002-2009 mainly through local accumulation of college graduates who enjoyed substantially high wage premiums as skilled workers. When a city experiences a larger college enrollment expansion shock, it accumulates more local human capital in the form of skilled workers once a larger number of college students graduate and stay locally to work after four years of college study.[6] This further translates into higher demand for local housing and hence (*ceteris paribus*) strongly appreciates local housing prices because Chinese college graduates enjoy high wage premium and have strong demand for housing.[7] By checking correlation patterns, we show that the college enrollment expansion shock is significantly positively correlated with housing shocks, and orthogonal to major endogeneity concerns on housing price movements that we could expect in the context of China, like permanent income shocks and trade liberalization shocks to the domestic private sector.[8]

We employ microeconomic data from Urban Household Survey (UHS) to estimate the causal response of household consumption to housing price movements in China. The UHS is a nationwide survey conducted by China's National Bureau of Statistics (NBS) on an annual basis, and we have access to a subset of data covering 6 provinces over the period 2002-2009. We construct consumption (excluding consumption of housing services), housing, and other socioeconomic variables at both household and city (county/prefecture) levels. The comparison of summary statistics at the household level with existing studies cross validates the representativeness of our sample. Rich cross-sectional variation across households is further used to show the strong positive correlation between consumption and housing price or housing wealth. In contrast, we rely on city-level

[6]According to data from the National Bureau of Statistics, the annual four-year college graduation rate during 2002-2009 was between 95% and 97% in China. This is much higher than the case in the United States. Aggregate data from the National Center for Education Statistics (NCES) show that U.S. annual four-year college graduation rate was 35%-40% during 2004-2014.

[7]For instance, Han et al. (2012) document that wage premium for college graduates (in comparison to high school dropouts) increased from 60% to 80% in urban China during 2002-2008. Though there exists a concern that a sharp expansion in college enrollment (through the lowering of admission scores) could lead to a remarkable drop in the quality of college graduates and hence a decline in wage premium for skilled workers (mainly college graduates), evidence from Han et al. (2012) strongly falsifies this narrative. As Han et al. (2012) suggest, this puzzle can be reconciled by the fact that China's accession to the World Trade Organization (WTO) created massive demand for skilled workers. Consequently, the increased demand overcome the decline in quality and resulted in a rise in wage premium for college graduates.

[8]More intuitively, for the relevance of our instrumental variable, we find a simple elasticity showing that local housing prices on average go up by 1.4% when local college enrollment expands by 10%.

regressions to estimate the causal effect because the instrumental variable is available only at the city level.

With the help of the novel instrumental variable (college enrollment expansion shock), we estimate an elasticity of consumption with respect to housing price of 0.06-0.07 for homeowners, or equivalently an elasticity of 0.12-0.13 with respect to housing wealth.[9] We also find that the average marginal propensity to consume (MPC) out of housing wealth is 0.025 to 0.03. The MPC estimate is quantitatively consistent with the elasticity estimate, given that the average ratio between housing wealth and consumption was 6.8 for homeowners during our sample period.[10] The estimated consumption response is economically significant. Specifically, the MPC estimate indicates that the increase in consumption induced by housing appreciation during 2002-2009 equates to 14%-17% of current consumption in 2009 for a representative urban homeowner. In addition, we show that our baseline results are robust to error in variables that emerges when we proxy population means with sample averages to implement city-level analysis. They also survive robustness checks on hedonic housing prices and habit formation in consumption.

In addition to the economically significant average consumption response to housing shocks, we reveal that there exists considerable heterogeneity in consumption response along several dimensions, including homeownership, income/wealth status, degree of collateral constraint, and durability or income elasticity of consumption goods. As for renters, the estimation results suggest that their consumption response to housing shocks is insignificant. Furthermore, we find that the MPC out of housing wealth is higher for households residing in poorer and more collateral constrained cities. Greater durability or a higher income elasticity of consumption goods also amplifies the consumption response.

Our paper mainly contributes to the empirical studies on household consumption response to

[9]The equivalence is implied by the fact that the average share of housing wealth in total net worth was around 56% in 2002 for homeowners (multiplying 0.12-0.13 by 0.56 produces 0.06-0.07).

[10]Note that the elasticity with respect to housing wealth equals to the product of the estimated MPC out of housing wealth and the ratio of housing wealth to consumption. Therefore, the MPC estimate implies an elasticity with respect to housing wealth of 0.17-0.21 (multiplying 0.025-0.03 by 6.8). The magnitude difference between 0.17-0.21 and 0.12-0.13 is mainly ascribed to the fact that we add quite different controls when estimating elasticity and MPC.

housing price changes.[11] It is most closely related to the recent papers by Mian et al. (2013) and Kaplan et al. (2016) that empirically investigate the causal impact of housing price bust on consumption collapse during the 2006-2009 financial crisis. Employing U.S. data at the county or Core-Based Statistical Area (CBSA) level, they find that the elasticity of consumption with respect to housing price is around 0.2. Compared with their estimates, we discover an elasticity of 0.06 to 0.07 for homeowners in a large developing economy that has been experiencing a decade-long housing boom. Besides the asymmetry induced by the concavity of consumption function in boom and bust of asset prices, we conjecture that the smaller consumption response of Chinese urban households might also be ascribed to several macroeconomic features unique to China.[12] It includes but is not necessarily limited to the underdevelopment of financial markets and strong precautionary saving motives due to uncertainties stemming from China's transition to a market economy. Moreover, unlike Mian et al. (2013) and Kaplan et al. (2016), we separate homeowners from renters by taking advantage of microeconomic data. This enables us to straightforwardly assess the role of homeownership, which implicitly reflects the importance of wealth effect and collateral constraint channel.

The rest of the paper is structured as follows. We start with the introduction of background information on Chinese housing markets and higher-education expansion in Section 2. We then present data, variable construction, and summary statistics in Section 3. Section 4 describes baseline regression specifications and deals with endogeneity in housing price movements. Main em-

[11] Our work is also related to a large literature that employ more structural techniques to study the response of consumption to housing price movements. Good examples include Carroll and Dunn (1998), Campbell and Cocco (2007), Attanasio et al. (2011), Berger et al. (2017), and Gorea and Midrigan (2017). In a broader sense, this paper echoes the empirical studies that investigate the macroeconomic implications of housing wealth from the firm side as well. See, among many others, Chaney et al. (2012), Bahaj et al. (2016), and Catherine et al. (2017).

[12] Unlike existing studies that attempt to understand the sharp decrease in household consumption following a financial crisis (Jensen and Johannesen, 2016) or a bust in asset prices (Mian et al., 2013; Kaplan et al., 2016), our study explores the contribution of a asset market boom to consumption growth. As suggested by the work of Carroll and Kimball (1996), households with precautionary saving motives have an optimal consumption function that is concave in wealth, when there is uncertainty in labor productivity and asset prices. Hence, we expect an asymmetry in consumption response to positive and negative housing wealth shocks. Starting with the same initial housing wealth, an increase in housing wealth will have a smaller impact on consumption than a decline in housing wealth of the same magnitude. The expected less responsive consumption in the boom case also helps us to explain the finding that our elasticity of consumption with respect to housing price is smaller than the elasticity Mian et al. (2013) obtain using U.S. data during the crash period of 2006-2009.

pirical results of this study are reported in Section 5. Robustness checks for the baseline regression results are presented in Section 6. Section 7 concludes.

1.2 Background

We present in this section the background information on the urban housing market and higher-education expansion in China. We first show how institutional changes contributed to the emergence of a decade-long housing boom in the early 2000s. Then we introduce the higher-education expansion that started from 1999, against the backdrop of several adverse economic conditions. The higher-education expansion generated a college enrollment boom, which is employed by this study as a novel instrumental variable for housing price movements.

1.2.1 Housing Markets in China

A great housing boom has been built up ever since China abolished the welfare-oriented public housing allocation system and established formal housing markets. The decade-long housing boom then provides us a unique setting to explore how consumption responds to substantially positive housing shocks.

Housing markets in China are nascent, not formally established until the late 1990s. Before 1978, housing was exclusively provided by the public sector and distributed to households via the *working unit-employee* linkage. Any institution or organization where people work could be counted as a *working unit*, including but not limited to enterprises, educational establishments, and government agencies. The working units provided free housing (which was allocated by the public sector) to reward their employees as a form of in-kind compensation. Owing to insufficient funds, an expanding population, and an inefficient allocation system, per capita residential space was very low for Chinese urban households during 1949-1978. In 1978, it was only 3.6 square meters, even lower than that in 1950 (4.5 square meters) when the People's Republic of China was newly founded.

Starting from 1978, the reform and opening-up policy triggered a series of institutional changes in the provision of housing. In September 1978, Deng Xiaoping (the paramount leader of China between 1978 and 1989) first proposed that the central government should incentivize private sector to provide housing. His guidance facilitated the State Council to formulate a commercialization policy of housing in June 1980, which granted individuals the rights to purchase homes. In 1982, the central government started a pilot program in four cities to subsidize the purchase of housing from the public sector by households. It primarily aimed to delink housing provision from employment (*working units*). A milestone was reached in 1988 when Chinese constitution was amended to allow for trading use rights of land, which also laid legal foundations for the marketization of housing in China.[13]. Broader and more profound housing reform was proposed in July 1994, including subsidizing private purchase of housing, establishing the commercialization and marketization of housing provision, and fostering housing credit markets.

In July 1998, the central government abolished the public housing allocation system and guided individuals to acquire housing, "commodity houses", from housing markets. This marked the establishment of housing markets in China. Hence, Chinese housing markets are very nascent when compared with mature markets in most developed economies.[14] Also in 1998, to mitigate negative economic shocks from the 1997 Asian Financial Crisis, the central government established housing

[13]The situation continues to this day. Only the use rights of land is allowed for transactions in China. The land, by the Chinese constitution, is state owned. Local governments sell leases of land for residential use to real estate developers with a maximum length of 70 years. The leases of land use rights can be traded freely, as long as they do not expire. As pointed out by Glaeser et al. (2017), it is still unclear what the government will do when those use rights expire. Even though there is massive uncertainty in the protection of private property rights pertaining to housing which is built on land that is state owned and has an expiration date for use rights, the central government is taking measures to reduce this risk. Li Keqiang, China's current premier minister, responded to a reporter question in a recent congress news conference in March 2017 that the State Council is formulating a new policy that allows households to renew leases.

[14]Though housing prices are market equilibrium prices after the establishment of housing markets, Chinese local governments play an important role in determining housing prices. Since houses and land are closely related, the institutional background on land also affects housing prices. In urban China, land are publicly owned by governments, only use rights of land is allowed for transactions. The typical procedure of land use rights trading goes like this: a local government prepares a piece of land that is leased for residential use with a maximum length of 70 years; next the local government sells the lease to real estate developers mainly through auctions; then developers build houses on the leased land and transfer the lease to households when selling houses to them, notice that land costs prepaid by developers have been factored into selling prices of houses; then households transfer the lease to other households if they decide to sell their houses, note that the depreciated value of lease has been factored into selling prices of houses by households. Considering that land prices accounted for over 40% of housing prices during 1998-2015, it is thus natural for us to expect that local governments can substantially affect equilibrium housing prices in housing markets, simply by controlling the supply of land and thus land prices. Section A of this paper's **Online Appendix** provides more detailed discussion on housing markets and land supply in urban China.

sector as a new engine of economic growth and a pillar industry for the economy, which substantively raised economic status of housing sector. Supporting measures, including subsidizing residential mortgages and broadening construction loans by real estate developers, were implemented to fuel the growth of housing markets. These measures, combined with other institutional changes, were quite effective and contributed to the formation of a housing boom in the early 2000s.[15] During 2002-2013, home sales have maintained an annual growth of 15%, and the construction of residential housing has grown by 18% annually. Another indicator confirms the housing boom in China during this period, real housing price has risen by over 10 percent between 2003 and 2014.

1.2.2 Higher-Education Expansion in China

To overcome a series of adverse conditions like economic downturns and soaring unemployment, the Chinese central government massively expanded college enrollment during 1999-2005. The sharp expansion then translated into strong demand for local housing, and hence serving as an effective instrumental variable for local housing price movements.

Like housing, higher education was exclusively provided by the public sector before the economic reform and market opening-up. The central government adopted the Soviet model to nationalize higher-education institutions in the early 1950s, and made admission plans for universities or colleges based on the needs of economic development. Since 1952 (even to this day), almost all college students in China have been admitted through a unified national college entrance exam (CEE). The higher-education sector experienced steady growth in the 1950s, yet disrupted and impeded in the 1960s by the "Great Leap Forward" and in the 1970s by the Cultural Revolution.[16]

[15] Many factors have been contributing to the decade-long (from 2003 until now) housing boom in China. To name a few, the strong incentive of local governments to acquire fiscal revenues through the selling of land use rights, the excessive injection of money into housing markets to maintain stably high economic growth, the limited investment vehicles for households due to underdeveloped stock and bond markets, the massively rigid demand for housing by young adults with funding support from parents and extended relatives, and the cultural tradition to own rather than rent homes intensified by the competition among males in the marriage market. Examining relative contributions of those factors requires more rigorous work, which is beyond the scope of this paper.

[16] Essentially, these two nationwide campaigns severely curtailed the admission of college students and disturbed the normal educational activities in comprehensive universities and specialized colleges. A landmark event was the suspension of college entrance exam in 1966.

It returned to the normal historical track in December 1977 when the college entrance exam was resumed. Between 1978 and 1998, the number of colleges increased from around 600 to more than 1,000, and the college enrollment increased from 0.4 million to 1.08 million (see Li et al., 2014, for more facts).

Although higher education in China experienced steady growth starting from 1978, an unexpected takeoff occurred in 1999. The sharp expansion was triggered by several unfavorable factors. First, the 1997 Asian Financial Crisis led to an economic downturn and increase in unemployment. Second, the South Tour Speeches by Deng Xiaoping catalyzed the reform of state-owned enterprises (SOE) into modern corporate entities, which laid off a large number of workers who were inefficient and redundant (Berkowitz et al., 2016). Third, the former prime minister of China, Zhu Rongji, took stringent monetary and fiscal polices in the early 1990s to guide the overheated Chinese economy to a soft landing, which resulted in weak internal demand. To overcome these adverse conditions, the central government adopted the proposal of an economist in Asian Development Bank (see Che and Zhang, 2017, for more archival details about this proposal) and formulated a higher-education expansion plan that aimed to pull up internal demand, stimulate consumption, promote economic growth, and alleviate employment pressure. The sudden expansion was mainly achieved by lowering CEE admission scores.

The higher-education expansion was conceivably a natural experiment. We plot levels and growth rates of college enrollment and the four-year lead college graduates in **Figure A.2** to show the unexpected expansion. Note, in particular, the sharp increase in college enrollment of 0.51 million in 1999. The total number of newly enrolled college students reached around 1.6 million, which was a 47.3% rise when compared to that in 1998. The massive expansion continued into the early 2000s, with college enrollment increasing by 38.1% in 2000, 21.6% in 2001, and 19.5% in 2002. On average, the growth rate of college enrollment was as high as 25.1% between 1999 and 2005. It is an extraordinary expansion when we compare it with the average enrollment growth during 1996-1998, that is, 25.1% versus 5.4%. The expansion substantially slowed down starting from 2006, with the annual growth rate dropping from 12.8% to 8.2%. The boom in college enrollment since 1999 translated into a boom in college graduates since 2003, where the four-year gap reflects the typical length of college study in China. College graduates rose by 40.4% in 2003 and kept a growth rate of more than 20% for four years during 2003-2006. Average growth in college graduates between 2003 and 2009 was as high as 22.3%. In this study, we employ the arguably exogenous college enrollment expansion (see Che and Zhang, 2017, and Knight et al., 2017, for more arguments on exogeneity of the higher-education expansion) between 1998 and 2005 to characterize increased local demand for housing over the period 2002-2009, and hence utilize it as an instrument for local housing price movements between 2002 and 2009.

There are two channels through which the central government managed to deal with such a massive expansion in college enrollment. At the intensive margin, the existing higher-education institutions were quite spacious to accommodate an abrupt rise in enrollment. Aggregate data show that the average student-faculty ratio was around 7 to 1 in 1998. At the extensive margin, many more higher-education institutions were established. Between 1998 and 2005, the number of higher-education institutions increased from 1,022 to 1,792. An explosive increase occurred in 2001 when around 200 universities and colleges were built in that single year. Meanwhile, more faculty members were recruited. The number of faculty was more than doubled during 1998-2005, from 407,000 to 966,000. Expansion in higher-education institutions and faculty members further increased the demand for local housing, hence fueling the growth of local housing prices. Construction of new institutions generated fierce competition for land against residential developers, and the newly recruited faculty members had strong purchasing power for housing with their relatively high income. More higher-education institutions within a city also attracted more in-migrants via the externality of local human capital accumulation, which then created additional demand for local housing.

1.3 **Data and Measurement**

We employ Chinese microeconomic data from the Urban Household Survey to explore consumption response to housing price/home value movements. Non-housing consumption, housing, and other socioeconomic variables are constructed at both household and county levels. We then present summary statistics of primary variables. The comparison of summary statistics with existing studies helps to cross validate the representativeness of our sample.

1.3.1 **Urban Household Survey**

The dataset we use is from the Urban Household Survey, which is conducted annually by China's National Bureau of Statistics. The UHS contains extensive information on urban households, such as household size, employment status, detailed income and expenditure, and housing wealth. All surveys are uniformly designed and conducted, and of high quality that is guaranteed by province-level scrutiny and timely reporting starting from base-level survey units. The UHS employs a stratified sampling, which is advantageous to sample each stratum independently. It puts massive emphasis on representativeness of the survey at all stratums, including community, county, prefecture, and province. Besides these features embedded in survey design to seek for randomization and representativeness, the NBS also devises cross-validation procedures at the aggregate level to further insure the quality of UHS data. It routinely checks the sample variances of disposable income, total consumption expenditure, and household size to track the change in sampling errors. We put more details on the design and implementation of UHS in Section B of this paper's **Online Appendix**.

The UHS data are proprietary and hard to acquire. We have access to a subset that covers six provinces over the period 2002-2009. The six provinces are: Beijing, Liaoning, Zhejiang, Sichuan, Guangdong, and Shaanxi. **Figure A.3** presents the geographic span of our sample. It reveals that these six provinces are well divided and geographically dispersed. **Figure A.3** also displays that the counties surveyed within each province are also geographically dispersed. The sample size tends to increase with population and, as such, the number of microeconomic observations at the county level tends to be representative of that county's aggregate size in population and output. However, it needs to point out that the UHS samples a disproportionately larger share of population for counties with less population and economic importance to guarantee representativeness. The representativeness of our dataset is also ensured by the fact that the six provinces in our sample took up a reasonable share of national population (25%), consumption (32%), gross domestic product (33%), and completed residential investment (25%) during the sample period. Our data also has a representative number of cities from all tiers of cities which are categorized according to their importance in population and economic volume, hence generating substantial cross-sectional variation in local housing markets. We cross validate several variables in our sample with the aggregated national counterparts published by the NBS to further test the representativeness of our data. For instance, average household per capita residential space grew from 25.4 square meters to 32.5 square meters during our sample period, while the national average increased from 22.8 square meters to around 30 square meters. More discussion on data availability and representativeness can be found in Section B of this paper's **Online Appendix**.

1.3.2 Variable Construction

Variables on consumption, housing, and other socioeconomic variables like employment, income, and wealth are constructed at both household and city levels. In the baseline results, county-level cities which match the boundaries of counties are considered. We employ prefecture-level cities which likewise match the boundaries of prefectures in robustness checks.[17] Since the construction of variables is similar for the two layers of cities, we only introduce county-level variables in this subsection.

1.3.2.1 Household-level Variables

At the household level, consumption variables can be obtained directly from the UHS data. Consumption expenditures for over 180 items are recorded. We sum up items to define total consumption (C_t^h), nondurable consumption ($C_{t,nd}^h$), durable consumption ($C_{t,d}^h$), and service consumption ($C_{t,s}^h$) using the methodology proposed by the Bureau of Economics Analysis (2015), U.S.

[17]In China, communities constitute counties, counties constitute prefectures, prefectures constitute provinces, and provinces constitute the nation. Both counties and prefectures can be treated as cities. Therefore, we name counties and prefectures as county-level and prefecture-level cities, respectively.

Department of Commerce. Note t denotes a year, h denotes a household, $\{nd, d, s\}$ denote non-durables, durables, and services, respectively. Housing consumption is carefully taken care of in this study. We include housing consumption in household-level summary statistics to make sure that our results are comparable to existing studies that employ other sources of microeconomic data. However, to avoid the endogeneity involved in housing consumption, we exclude it from consumption in all household-level regressions.[18] We exclude housing consumption when constructing city-level consumption variables, thus city-level regressions are automatically free of any direct endogeneity caused by the inclusion of housing consumption. More detailed information on the construction of consumption variables can be found in Section C of this paper's **Online Appendix**.

Most of the housing variables need to be calculated using recorded variables. In line with Mian et al. (2013) and Kaplan et al. (2016), we are particularly interested in two housing variables: home value and housing price. Home value is needed for us to assess MPC out of housing wealth, while housing price is the main driving force of variation in home value and indispensable for us to assess elasticity of consumption with respect to housing price. Home value (HV_t^h) is well recorded in the data. Housing price (HP_t^h) can be recovered using home value and construction area (in square meters) of the house (CA_t^h), i.e. $HP_t^h = \frac{HV_t^h}{CA_t^h}$. Unlike Mian et al. (2013) and Kaplan et al. (2016) who focus on county or zip-code level analysis using U.S. data, we cannot construct housing net worth shock or housing price shock as they do at the household level, for the reason that our UHS data do not provide an identifier to track the same households over time. It means that our data are pooled cross-sections, rather than a panel. Nevertheless, we construct housing net worth shock and housing price shock as they do for county- or prefecture-level cities when we aggregate households into geographic cohorts (county/prefecture) to conduct pseudo panel analysis.

Besides home value and housing price serving as primary explanatory variables in regressions, we also construct other housing variables that are indicative of the development of housing markets.

[18] When housing consumption is included in consumption, an endogeneity problem easily arises in the regression of consumption on housing shocks. There exist lots of unobserved factors that could affect both consumption of housing and housing prices. For instance, a negative permanent income shock tends to depress both consumption of housing and housing prices within a location.

Specifically, we calculate per capita residential space in square meters as the ratio of construction area and household size to show the change in housing conditions for Chinese urban households. We define monthly housing rent rate in *yuan* per square meter as the ratio of total monthly rent to the area of the owned house, where total monthly rent is the self-estimated amount that should be paid for the housing service delivered by owned homes. We check the validity of the housing rent rate using the rent rate paid by renters for rental houses. It produces quite similar summary statistics in each year of our sample. We prefer the housing rent rate constructed using owned homes because we can further utilize it to define price-to-rent ratio (i.e. the ratio of housing price to annual rent) at the household level, which may be informative of the likelihood of a housing bubble.

The other household variables we construct mainly serve as controls in our empirical analysis, including household characteristics on employment, income, and wealth. Household size is the number of people in the household, recorded in the original data, among which the number of wage earners is the sum of all wage earners in different sectors. The fraction of workers in state-owned enterprises (SOE) measures the share of labor in the public sector. Household income characteristics contain total income, disposable income, and the fraction of income from working. Disposable income is the total income net of operation outlays (mostly related to home-owned small business), individual income tax, and social security fees. With annual income information, we can construct a proxy variable for the affordability of homes, the price-to-income ratio, which is defined as the ratio of home value to annual household disposable income.

Measuring household wealth characteristics poses greater challenges because the UHS provides a very limited record of specific household asset information other than home values. We employ detailed information on asset income to recover net worth (NW_t^h). Net worth is defined conventionally as $NW_t^h = F_t^h + HV_t^h - D_t^h$, where F_t^h are financial assets including cash holdings and bank deposits, D_t^h is the outstanding debt. Cash holdings are well recorded, and bank deposits can be estimated using the formula $\frac{Total\ Banking\ Interest\ Income}{Weighted\ Bank\ Deposit\ Interest\ Rate}$, where the weighted deposit interest rate is published by the NBS. Similarly, debt can be estimated using annual interest payments on outstanding debt and weighted bank loan interest rate from the NBS. Bonds and stocks cannot be recovered because the UHS has no income information for them. As a consequence, we ignore them in this study. We argue in Section C of this paper's **Online Appendix** that dealing with bonds and stocks in alternative ways does not alter our main results.

1.3.2.2 County-level Variables

County-level variables are constructed by aggregating corresponding variables at the household level. We employ sample averages within each county for aggregation, hence abstracting from local demographic transitions and focus on economics behaviors. We keep counties or prefectures that have observations in both 2002 and 2009, for the reason that we need to construct changes and growth rates of variables over the sample period. Of the original 155 counties and 66 prefectures in our sample, 42 counties and 59 prefectures satisfy this criterion.[19]

Housing variables in our study consist of a housing net worth shock, a housing price shock, the change in home value, and the housing leverage ratio. We define housing net worth shock ($HNW^c_{2002-2009}$) as the product of initial housing wealth share in 2002 ($\frac{HV^c_{2002}}{NW^c_{2002}}$) and the growth rate of housing price between 2002 and 2009 (defined as $\Delta log(HP^c_{2002-2009}) = log(HP^c_{2009}) - log(HP^c_{2002})$), i.e. $HNW^c_{2002-2009} = \frac{HV^c_{2002}}{NW^c_{2002}} \times \Delta log(HP^c_{2002-2009})$.[20] Note c denotes a county. Mian et al. (2013) focus on this composite term because they want to investigate the strength of a particular transmission mechanism for the effects of housing price movements on consumption, the so-called household balance sheet channel, which echoes the line of work by Mishkin et al. (1977) and Carroll and Dunn (1997). If a county has a higher initial housing wealth share in 2002, we

[19] For these 42 counties and 59 prefectures, we have observations for them over all sample years. Hence, it means that we obtain a completely balanced panel data for these 42 counties and 59 prefectures during 2002-2009. In robustness checks, we employ this full balanced panel over eight successive years to deal with habit formation in consumption.

[20] Due to a lack of information on financial assets (including quantity and price), we are unable to construct a financial net worth shock as Mian et al. (2013). It is arguable that the overlook of financial net worth shock should only generate a minor issue. Since Chinese stock market is highly risky (see Fang et al., 2015, for more information), we expect that a transitory increase in financial wealth may have only a minor impact on consumption (Lettau and Ludvigson, 2004). More discussion on the issue of financial net worth shock is presented in Section C of this paper's **Online Appendix**.

expect the propagation channel to generate larger impacts on consumption for this county, given housing price movements of the same size in other counties. Kaplan et al. (2016) suggest interpreting the composite term as an interaction between housing price shock and the initial housing wealth share. Using this specification, they find that the household balance sheet channel is not an economically significant mechanism to explain the co-movement between housing prices and consumption. Housing price shock does matter for the movement in consumption, yet the interacted term loses its statistical significance. Lessons from these researches inspire us to check the relevance of household balance sheet channel with Chinese county-level data. For this purpose, we separate off the growth rate of housing price ($\Delta log(HP^c_{2002-2009})$) from the housing net worth shock as housing price shock. Change in home value is defined as the difference between the two years, i.e. $\Delta HV^c_{2002-2009} = HV^c_{2009} - HV^c_{2002}$.

Following Mian et al. (2013), we define housing leverage ratio as the ratio of mortgage plus home equity line of credit (HELOC) to home value, and employ it to proxy the degree of collateral constraint (or leverage) at the local level. We discuss the legitimacy of using leverage ratio specific to housing as a proxy for credit constraints in C of this paper's **Online Appendix**. It needs to be mentioned that our housing leverage ratio is underestimated due to missing information in the data. First, we only have new mortgages rather than all existing mortgages. Second, we have no information on home equity line of credit that allows households to borrow money using homes' equity as collateral. If the scale of existing mortgages/HELOC is positively correlated with the scale of new mortgages, we expect that even the underestimated housing leverage ratio would be still informative about the degree of leverage at the local level. The condition is probably true because the supply of standard loans by commercial banks (including mortgages and HELOC) tend to be positively correlated with local financial development.

County-level consumption variables include total consumption growth and changes in different categories of consumption. Total consumption growth ($\Delta log(C^c_{2002-2009})$) is defined as the growth rate of total consumption expenditures, i.e. $\Delta log(C^c_{2002-2009}) = log(C^c_{2009}) - log(C^c_{2002})$. Change in total consumption ($\Delta C^c_{2002-2009} = C^c_{2009} - C^c_{2002}$), nondurable consumption ($\Delta C^c_{2002-2009,nd} =$

$C^c_{2009,nd} - C^c_{2002,nd}$), durable consumption ($\Delta C^c_{2002-2009,d} = C^c_{2009,d} - C^c_{2002,d}$), and service consumption ($\Delta C^c_{2002-2009,s} = C^c_{2009,s} - C^c_{2002,s}$) are straightforwardly constructed in a similar way as change in home value. Notice that we exclude housing consumption when constructing these county-level consumption variables. We also construct socioeconomic variables at the county level to serve as controls in regression analysis. Employment share in SOE is the share of SOE wage earners in the pool of all wage earners within a county. Similarly, employment share in domestic private sector (DPS) is the share of wage earners from domestic private firms, most of which are legal person enterprises. Total disposable income per household and net worth per household are simply sample averages of corresponding variables at the household level within a county.

It is worth pointing out that we focus on the responses of real consumption expenditure to housing price/home value movements. As revealed by Stroebel and Vavra (2016), retails prices significantly respond to changes in local house prices, mainly through the change in homeowner demand elasticity and firm markup decision.[21] Hence, it is necessary for us to exclude general price changes. To this end, we collect annual county- or prefecture-level CPI from the China City Statistical Yearbook to deflate nominal variables in our sample. We find massive geographic variation in price index changes that might be ascribed to geographic heterogeneity in consumer or pricing behaviors, as well as idiosyncratic local supply or demand shocks. See Section C of this paper's **Online Appendix** for more information and discussion on deflating nominal variables.

1.3.3 Summary Statistics

We document in this subsection summary statistics of primary variables. Household-level summary statistics exhibit strong growth in consumption, housing prices and housing wealth over our sample period. They are also in accordance with existing studies, thus providing cross validation for the representativeness of our sample. County-level summary statistics reinforce household-level facts, and show substantial cross-sectional variation that contributes to the identification of

[21] In particular, Stroebel and Vavra (2016) argue that markups rise with housing prices, particularly in high home-ownership locations, because greater housing wealth reduces homeowners' demand elasticity, and firms raise markups in response.

consumption response to housing shocks. Notice that all nominal variables have been adjusted using annual CPI (base year 2002=100).

1.3.3.1 **Household-level Descriptive Statistics**

We briefly report household-level summary statistics and discuss important stylized facts revealed by them. More detailed information on the household-level summary statistics can be found in Section D of this paper's **Online Appendix**.

Household housing wealth increased dramatically during 2002-2009, mainly driven by housing prices. From 2002 to 2009, housing wealth per household was more than tripled, and housing prices increased by over 300%. In contrast, household per capita residential space rose very mildly, from 25.4 squares to 32.5 square meters. Since household size was stable around 3 (due to the one-child policy) over the sample period, it suggests that the fast-growing housing wealth mainly came from soaring housing prices rather than more spacious homes. We also document substantial dispersion in housing wealth and housing prices across households throughout all sample years. Again, it shows that dispersed housing prices contributed to most of the cross-sectional difference in housing wealth.

The homeownership rate was high and rising, accompanied by a remarkable increase in multiple-homeownership rate. Urban households in China maintained a very high homeownership rate, which rose from 70% to 85% during 2002-2009. Meanwhile, among homeowners, the share of households possessing two or more homes ascended from 10% to over 16%. The observed high and increasing homeownership and multiple-homeownership rates find support from rising housing price premium, which is defined as the ratio of current home value to value paid when households purchased homes. Average housing premium increased from 4.4 in 2002 to 6.6 in 2009. The implied increasingly high expected returns in housing markets could have incentivized households to own homes, even multiple homes if funds were sufficient.

Household income showed a continuous and stable growth over the sample period, while price-to-income ratio was rising. Disposable income per household was more than doubled, from 26,000 *yuan* (approximately 3,137 USD when we use exchange rate in 2002 to exclude the effect of Renminbi appreciation against USD during 2002-2009) to 57,000 *yuan* (approximately 6,877 USD).[22] However, average price-to-income ratio ballooned from 3.75 to 7.24 during 2002-2009. Despite strong and steady income growth, the high price-to-income ratio means heavy financial burdens for a typical urban household. It might have contributed to China's high household saving rates because low- and middle-income households need to save a large amount of current income to cover housing down payment, which lies between 30%-40% of home value. The rising price-to-income ratio may also suggest the building up of a housing bubble, which is supported by another fact that average price-to-rent ratio was as high as 40-50 over the sample period.

On the expenditure side, household total spending was also more than doubled between 2002 and 2009, hit 60,000 *yuan* in 2009 from about 25,000 *yuan* in 2002. Out of the total spending, consumption expenditure took up the majority, almost 75% on average across years.[23] Nondurable and service consumption were nearly of equal importance and accounted for more than 80% of total consumption. Durable spending seized a smaller share, yet showed a stronger growth momentum. It also exhibited more responsiveness to business cycle fluctuations, stalling in the Great Recession despite that the other two categories of consumption kept rising. Similar to housing price/home value, we find massive cross-sectional variation in consumption, which helps to assess the correlation between housing and consumption.

Household net worth was almost quadrupled during 2002-2009, showing a spectacular creation of wealth at the microeconomic level. Net worth per household was over 460,000 *yuan* in 2009, building up from 130,000 *yuan* in 2002. Due to stringent capital controls, households in China are not allowed to invest in international capital markets. This forces them to invest in domestic markets where only a few investment vehicles exist. Since the underdeveloped financial markets exhibit volatile and low expected returns (see, e.g. Fang et al., 2015), households in urban China primarily

[22] The *renminbi* (RMB) is the of official currency of the People's Republic of China. The *yuan* is the basic unit of the *renminbi*. In Chinese currency, 1 *yuan* is equal to 10 *jiao* or 100 *fen*.

[23] In the UHS, total spending includes consumption expenditure and several other types of expenditure, such as operation outlays (mostly related to home-owned small business), asset expenditures (like interest rate payments), transfer expenditures (like income taxes and donations), and social security fees (like retirement pensions). In this study, we mean consumption expenditure (rather than total spending) whenever we refer to household spending.

choose to hold assets as bank deposits and housing. Though bank deposits generate nearly zero or even negative real returns, the risk-free feature and strong precautionary saving motives of Chinese urban households (see Meng, 2003, and Chamon and Prasad, 2010, for the precautionary saving motives) make it a suitable choice. However, as emphasized by Fang et al. (2015), a better choice is housing due to its high returns and relatively mild risk when compared with stocks. Average share of housing wealth in household net worth is consistent with these broader market features. It rose from 56% in 2002 to 71% in 2009.

Household size was fairly stable around 3 over the sample period, and the share of household wage earners in SOEs was declining steadily. As a result of one-child policy, the typical urban household size was close to 3 in China. Mean household size mildly shrank from 3 to 2.8 over the sample years, indicating a decline in fertility rate, probably triggered by higher cost in raising children and postponed childbearing from couples with more education (Morgan et al., 2009). The average number of wage earners was 1.3-1.6, and kept considerably stable. With only one noticeable drop in 2008, partly reflecting the economic downturn in the Great Recession. The fraction of wage earners in state-owned enterprises dropped from 59% in 2002 to 44% in 2009. It dovetails nicely with the simultaneous reform of state-owned enterprises, which reduced the redundancy of inefficient labor as narrated by Berkowitz et al. (2016) and hence forced previous SOE employees to seek jobs in the domestic private sector.

In addition to revealing stylized facts, household-level summary statistics also provide us opportunities to cross-validate the representativeness of our sample. We compare our summary statistics with existing studies that use other sources of household-level data. Summary statistics on consumption, housing, income, and wealth have been compared. All show strong comparability between descriptive statistics from our sample and those from existing studies. For instance, our sample shows that average per capital residential space was 32.5 square meters in 2009, while Li and Wu (2017) document that it was 31.2 square meters in 2010 using microeconomic data from the 2010 China Family Panel Studies (CFPS). See Section D of this paper's **Online Appendix** for more comparisons.

1.3.3.2 County-level Descriptive Statistics

We tabulate county-level summary statistics in **Table A.1**. Since we utilize the growth rates or changes in levels from 2002 to 2009, we need to keep counties that have valid information in both years. 42 counties satisfy this criterion. In describing county-level variables, we also correct for the variation in county size by weighting variables with population share. The weights we utilize are set to be time invariant, from the initial year 2002 only. We focus on housing, consumption, and several initial socioeconomic conditions.

Housing and consumption variables show strong growth and considerable geographic variation across counties. The mean value for housing net worth shock was as high as 52.6%, and the median was 51.4%. What matters more for estimation using the cross-section is the remarkable geographic variation. The standard deviation of housing net worth shock was more than 53%, even higher than the mean. And the interquartile range reached 56.9%. The housing price shock, defined as the growth rate of housing price between 2002 and 2009 further indicates a spectacular housing boom. Average growth rate over the sample period was 95.4%, almost doubling. There also exists massive geographic variation in housing price shock. The standard deviation and interquartile range are both around 60%. Change in home value exhibited an enormous increase in housing wealth, around 196,500 *yuan* (approximately 23,675 USD) per household. The large standard deviation and interquartile range both suggest extensive variation across counties. Consumption also exhibited strong growth during 2002-2009. Total consumption growth was around 62%, with a median even higher than 64%. In level values, total consumption increases by 14,500 *yuan* (approximately 1747 USD) between 2002 and 2009. As for the composition, all the three categories of consumption were growing remarkably fast. We find massive variation in consumption growth and changes across counties as well.

Besides growth rates and changes in levels, we also summarize variables in the initial year, which are employed as controls in county-level regressions. Housing leverage ratio (a proxy of collateral constraint) in 2002 has a mean of 14.3%, substantially lower than that in the United States (more than 60%). An important caveat is that due to missing information on existing mortgages and

HELOC, our housing leverage ratio is significantly underestimated. And yet the true value is likely to be lower than the U.S. counterpart because the Chinese households save much more and only borrow externally when outlays surpass savings. Young adults in China are also more likely to get financial supports from parents or extended relatives, probably due to differences in culture and financial development.[24] More interestingly, it shows that the housing leverage ratio was highly dispersed. The standard deviation was 37.1%, more than double of the mean. The interquartile range is smaller than standard deviation, yet still close to 22%. This generates rich variation for us to assess the role collateral constraint plays in affecting the consumption response to China's housing boom. The socioeconomic variables, including employment shares, income and net worth per household in 2002 was slightly different from household-level averages because county-level means are population weighted.

1.4 Regression Specification and Identification

This section first presents baseline regression specifications employed to evaluate the consumption response to housing shocks, in the spirit of Mian et al. (2013) and Kaplan et al. (2016). Then we address the identification issue that might be caused by reverse causality or unobservable confounding factors. We next construct a county-level instrumental variable to measure exogenous variation in housing price movements, which conveys an economic narrative specific to China but of relevance to the broader literature that links housing demand shocks to business cycle fluctuations.

1.4.1 Baseline Regression Specification

Before we specify baseline regressions, it is necessary to stress the difference in data properties at the household and county levels. Since the UHS data provide no identifiers to track households

[24] As Wei and Zhang (2011) argue, Chinese parents are very concerned about the marriage of their sons. They save aggressively to support the purchase of homes for their sons, so that the young men can be more attractive in marriage markets. Since the underdevelopment of financial market in China poses substantial constraints on borrowing against future income (Chamon and Prasad, 2010), it is hard for Chinese young adults to borrow directly from banks. This forces them to rely more on informal finance, like borrowing from their parents or even extended relatives.

across time, our sample at the household level are pooled cross-sections, rather than a panel data. In contrast, at the county level, we group households in the same county as a "cohort" and then construct a balanced pseudo panel for 42 counties (and 59 prefectures). The difference in data structures inspires us to choose divergent empirical specifications for households and counties. It also affects our confidence in identifying the causal effect in different settings. More specifically, we treat household-level regressions as correlations with a rich set of controls, while county-level analyses with instrumental variables as causation.

Our main exercise is to explore how household consumption in China responds to shocks in housing markets, including housing wealth changes and housing price movements.[25] The empirical work is closely related to a vast number of theoretical studies exploring consumption risk-sharing and housing wealth effect. A main prediction from the representative agent model with complete markets (for instance, Cochrane, 1991) is that individual household consumption is completely insensitive to idiosyncratic shocks in wealth, which is known as the full risk-insurance hypothesis. Constantinides and Duffie (1996), among some others, further point out that the full risk-insurance hypothesis could even hold under less restrictive assumptions of incomplete markets and borrowing constraints.[26] As for the housing wealth effect, several studies like Sinai and Souleles (2005) argue that households are naturally hedged against negative housing wealth shocks since they have to consume housing services in a forward-looking way, which also implies that the current housing wealth movement should have zero effect on household consumption. To test the theoretical prediction along this strand of literature, our first baseline regression at the household level is specified

[25]We need to clarify that all baseline regressions (at both household and county levels) of this study focus on the consumption response of homeowners. It is clearly to see that we need households to be homeowners to measure housing price or home value at the household level. However, we could in principle evaluate the consumption response of renters to housing price shocks at the county level. This adds a new source of heterogeneity for the consumption effects of housing shocks.

[26]In contrast, Baxter and Crucini (1995) emphasize that persistent differences in income are necessary for *ex post* trade in bonds to diverge from *ex ante* trade in equities (i.e., full risk sharing), in order to generate large idiosyncratic changes in consumption. Given the high persistence of housing price increases in China, we expect that the full risk-insurance hypothesis tends to be rejected. Our estimation results in Section 5 confirm this anticipation.

as follows:

$$log(C_t^h) = \alpha_1 + \beta_1 \times log(HP_t^h) + \mathbf{Z}_t^h \cdot \gamma_1 + \sum_{j=1}^{8} \eta_{1,j} \times D_{2001+j} + \varepsilon_{1,t}^h \tag{1.1}$$

where C_t^h and HP_t^h are total consumption expenditure (not including consumption of housing) and housing price for household h in year t; $\mathbf{Z}_t^h$ is a vector of household-level controls like income, non-housing net worth, household size, home characteristics, etc. We choose housing price rather than home value to estimate the consumption elasticity to isolate the effect of housing price movements from variation in the quantity of housing. It is in accordance with the fact that most frequently observed disturbance to household housing wealth comes from price movements while the holding of housing quantity is quite stable, at least in the short run. Besides, the finding in Kaplan et al. (2016) implies that the main effect of changes in housing wealth on consumption originates from movements in housing prices.[27] Another quick reason for choosing housing price is just to be in line with the county-level specifications in Mian et al. (2013) and Kaplan et al. (2016) who focus on housing net worth shocks driven by housing price movements. Though we denote consumption and housing price with time subscript in equation (1.1), it is not a setting for panel data regression. The nature of pooled cross-sections at the household level forces us to treat time and cross-section dimensions equally. However, to account for nationwide macroeconomic shocks (including shocks to trend growth in major economic variables, as emphasized by Aguiar and Gopinath, 2007, for large emerging economies like China) that tend to affect all households, we include a linear combination of year dummies $\sum_{j=1}^{8} \eta_{1,j} \times D_{2001+j}$ in the equation (1.1), where D_{2001+j} is equal to one in year $2001 + j$ and zero otherwise.

We essentially employ cross-sectional variation in housing prices to evaluate the consumption response to housing price shocks at the household level. This is different from the theoretical stud-

[27] Actually, if we replace HP_t^h with HV_t^h on the right hand of equation (1.1), we get quite similar results, thus supporting the finding in Kaplan et al. (2016) using Chinese microeconomic data. However, it needs to re-emphasize that our household-level regression results are just suggestive, rather than causal effects. When Kaplan et al. (2016) analyze the direct effect of housing price shocks on consumption, they also present suggestive evidence, rather than causal effects guaranteed by an instrumental variable.

ies mentioned earlier that additionally emphasize variation in housing wealth over time. Our choice is a compromise to the theoretical setting, and is mainly restricted by the availability of data at the household level. Nevertheless, we manage to mitigate the discrepancy in two ways. First, following Mian et al. (2013) and Kaplan et al. (2016) who also lack household-level panel data to explore consumption response to housing shocks, we utilize variation in housing prices across both time and cross-sectional dimensions at the county/prefecture level to further assess the consumption response. Second, we control for the observed home characteristics that capture main properties of a home. The primary merit of including variation along the time dimension is to ensure the change in housing prices attached to the same home, that is, complete comparability of houses. We make the homes highly comparable across households by including characteristics like home age, size, architectural style, water and sanitary facilities, and heating and fuel supply equipments. In practice, we also expect the the majority of homes are fairly comparable across households in urban China because most of them are multistory or even high-rise buildings designed and constructed by a small number of large real estate developers who can easily learn from each other.[28]

At the county level, we specify the baseline regression by following Mian et al. (2013) and Kaplan et al. (2016), hence it is reasonable to do cross-country comparison of consumption response to housing shocks in terms of elasticity. Specifically, our first baseline regression at the county level is specified as:

$$\Delta log(C^c_{2002-2009}) = \delta_1 + \theta_1 \times HNW^c_{2002-2009} + \mathbf{Z}^c_{2002-2009} \cdot \boldsymbol{\omega}_1 + \varepsilon^c_{1,2002-2009} \tag{1.2}$$

where $\Delta log(C^c_{2002-2009})$ is the growth rate of total non-housing consumption for county c between 2002 and 2009; $HNW^c_{2002-2009} = \frac{HV^c_{2002}}{NW^c_{2002}} \times \Delta log(HP^c_{2002-2009})$ is the housing net worth shock, defined as the product of initial share of housing wealth in total net worth and growth rate of housing price during 2002-2009. A set of county-level controls including initial conditions in 2002 and income growth during 2002-2009 are stacked in $\mathbf{Z}^c_{2002-2009}$. In principle, equation (1.2) is a

[28]To avoid the disturbance of villas which could be more heterogeneous in terms of design and construction, we exclude homes valuing more than two million *yuan* as a further check. It turns out that our baseline statistical and empirical results are barely changed.

cross-sectional regression at the county level, but it is free of a problem suffered by the pooled cross-sectional regression in equation (1.1) where individual-specific unobserved heterogeneity is not accounted for. If the county-specific unobserved heterogeneity is time invariant, it gets cancelled out when we difference it between 2002 and 2009. This advantage, along with the other advantages like the availability of instrumental variables, encourage us to view estimates using county-level information as reliable results for causation. Following Kaplan et al. (2016), we also extend the specification in equation (1.2) to check whether the effects of housing price shocks ($\Delta log(HP^c_{2002-2009})$) on consumption operate through the household balance sheet channel. In that sense, our regression equation is:

$$\begin{aligned}\Delta log(C^c_{2002-2009}) =& \delta_1 + \theta_{1,1} \times (\frac{HV^c_{2002}}{NW^c_{2002}} \times \Delta log(HP^c_{2002-2009})) + \theta_{1,2} \times \frac{HV^c_{2002}}{NW^c_{2002}} \\ &+ \theta_{1,3} \times \Delta log(HP^c_{2002-2009}) + \mathbf{Z}^c_{2002-2009} \cdot \boldsymbol{\omega}_1 + \varepsilon^c_{1,2002-2009}\end{aligned}$$

where $\theta_{1,1}$ captures the role of household balance sheet channel, and $\theta_{1,3}$ captures the direct impact of housing price shocks on consumption.

Besides the elasticity estimates in equation (1.1) and (1.2), we are also interested in the marginal propensity to consume (MPC) out of housing wealth for two reasons. First, as Mian et al. (2013) suggest, a regression in levels rather than log values enables us to test the concavity of consumption function with respect to wealth. In addition, it allows us to assess various operational channels of consumption response to housing shocks, like the collateral constraint mechanism. Second, though the elasticity term provides a more comparable measure across countries in existing empirical studies, the MPC term has a more intuitive economic meaning. To estimate MPC, we first employ household-level information that bears rich cross-sectional variation, and then utilize the county-level variation in an analogous fashion to what Mian et al. (2013) do using U.S. data. Our second household-level regression in levels is:

$$C^h_t = \alpha_2 + \beta_2 \times HV^h_t + \mathbf{Z}^h_t \cdot \gamma_2 + \sum_{j=1}^{8} \eta_{2,j} \times D_{2001+j} + \varepsilon^h_{2,t} \tag{1.3}$$

where HV_t^h is the home value for household h in year t. Note that C_t^h does not include consumption of housing. Here we choose home value rather than housing price to be consistent with the MPC regression adopted in Mian et al. (2013) at the county level. However, replacing HV_t^h with HP_t^h does not make a big difference, given the facts that variation in home value is mainly driven by variation in housing prices and the importance of housing wealth in household total net worth is fairly homogeneous across households (i.e. a very low standard deviation for the share of housing wealth in total net worth). Likewise, equation (1.3) is essentially a pooled cross-sectional regression, though we denote it with time subscript. Analogously, our second county-level regression is designed as:

$$\Delta C^c_{2002-2009} = \delta_2 + \theta_2 \times \Delta HV^c_{2002-2009} + \mathbf{Z}^c_{2002-2009} \cdot \boldsymbol{\omega}_2 + \varepsilon^c_{2,2002-2009} \tag{1.4}$$

where $\Delta C^c_{2002-2009}$ and $\Delta HV^c_{2002-2009}$ are changes in total non-housing consumption and home value in county c during 2002-2009. We can easily extend equation (1.4) to explore how wealth status or collateral constraints influence the consumption response by adding interaction terms for net worth or the housing leverage ratio. Since the instrumental variable helps us to identify a causal effect at the county level, we only conduct these extensions for county-level analysis. Note that we intentionally distinguish the groups of regressions for estimating elasticity and MPC with the subscript 1 and 2, respectively, such as α_1 and α_2. To keep consistency, we also choose the same groups of Greek letters to denote coefficients at the same regression level, i.e. $\{\alpha, \beta, \gamma, \eta\}$ for households and $\{\delta, \theta, \omega\}$ for counties. We are cautious about city-level regressions because they are implemented with a small number of observations (42 for counties or 59 for prefectures). Since the asymptotic variance covariance matrix for estimators barely hold in such small samples, we utilize bootstrapped standard errors. The procedure to obtain bootstrapped standard errors is fairly standard. We first draw random samples from our data, next estimate $\{\delta, \theta, \omega\}$ for each draw, and then calculate standard deviations for the estimates of $\{\delta, \theta, \omega\}$ from multiple draws.

Under the full risk-insurance hypothesis, we expect $\{\beta_1,\ \beta_2,\ \theta_1,\ \theta_2\}$ to be zeros, meaning that there is no consumption response to housing shocks. Our main task is to test whether this null hypothesis can find support from Chinese microeconomic data. To gain an insight into correlations, we plot the dyad of consumption and housing variables in **Figure A.4-A.5**. It reveals in Panel (A) of **Figure A.4** that log values of consumption and housing prices are highly positively correlated at the household level. Though the correlation between level values appears less intuitive in Panel (B) of **Figure A.4**, it is actually just papered up by the increased dispersions of values when we substitute log values with levels. The bulk of observations are stacked together in the range of 500,000 *yuan* and 1,000,000 *yuan*, and show a strong positive correlation between levels of consumption and home value. This also explains the positive fitted line in Panel (B) of **Figure A.4**. At the county level, Panel (A) and Panel (B) of **Figure A.5** exhibits a quite noticeable positive correlation between consumption and housing shocks, regardless of growth rates or changes in levels. On the whole, the correlation patterns are working against the full risk-insurance hypothesis, and it would be unsurprising if we reject the null hypothesis. Next, we rely on formal econometric methods to establish causation.

1.4.2 **Endogeneity of Housing Price Movement**

To identify the causal effect, we need to ensure exogenous variation in our primary explanatory variable, housing price movements. It turns out that housing price movements are arguably the main source of variation for home value changes and housing net worth shocks as well, especially in the context of China where housing wealth is the dominant way of holding assets. This leads us to focus on addressing endogeneity concerns involved in housing price movements. We can only deal with the endogeneity issue at the county (or prefecture) level due to the availability of data for constructing reliable instrumental variables. At the household level, we lack data to instrument for such disaggregated variation in housing prices, which also motivates us to treat household-level regressions as correlation rather than causation.

Two endogeneity concerns emerge when we employ housing price movements as the driving force of primary regressors in baseline regressions. The first one is reverse causality. A positive correlation between housing price and consumption movements might not be due to higher increase in housing prices inducing higher increase in consumption via the wealth channel or some others, but to local governments raising land prices thus housing prices more in cities where households also have higher growth of consumption and are more likely to afford increasingly expensive homes. Given the limited supply of land for residential use and a heavy dependence on land sales revenue for financing government expenditures, Chinese local governments have strong incentives to raise and maintain high land prices.[29] Since land price acts as a major type of cost for real estate developers (see Glaeser et al., 2017), a rise in land cost is typically passed-through to final purchasers in the form of higher housing prices.[30] A second concern is the issue of unobserved confounding factors or misattribution. An unobserved variable, like a positive permanent income shock, tends to increase consumption and housing prices at the same time when it translates into a surge in household demand for non-housing consumption and housing services.

Since we cannot establish exogenous housing price movements orthogonal to all factors that might lead to reverse causality or misattribution, we exert our best efforts to account for confounding factors in the context of China. For reverse causality, a very probable confounding factor might be the *revenue collecting motive* from local governments. It basically describes the financial dependance of local governments on land sales revenue. We can proxy this motive using changes in the share of land sales revenue in total fiscal revenues of local governments at the county or prefecture level. If we observe an increase in the share of land sales revenue in a county, it is very likely that the county has an enhanced incentive to raise land and hence housing prices, provided that the supply of residential land is limited and the government is forward looking in maintaining continuous land sales revenue due to an absence of viable alternative sources of revenue. When this

[29] Aggregate data from the NBS and Ministry of Land and Resources of the People's Republic of China show that land revenues accounted for 40.4% of total fiscal revenues collected by local governments during 1998-2015. Between 2002 and 2009, it even reached 45.1%. Moreover, it went beyond 50% for some cities lacking other alternative sources of revenues like value added tax because of less developed manufacturing and service sectors.

[30] Glaeser et al. (2017) indicate that equilibrium housing prices consist of three components: land prices, construction costs, and profits plus taxes. They find that construction costs are typically less than one-third of the selling price of houses. Aggregate data from the NBS and Ministry of Land and Resources of the People's Republic of China show that land prices accounted for around 40.4% of housing prices during 1998-2015. Between 2002-2009, it even reached 45.2%. These statistical figures clearly demonstrate that land cost is a substantial part of housing prices in urban China.

phenomenon emerges, we expect it is more likely to occur in a county with higher income growth. The reason is that households residing in a city with higher income growth typically have more development opportunities and easier access to external finance like mortgages, hence could better tolerate a rise in housing prices arising from a hike in land prices. Owing to the fact that a higher income growth is in general accompanied with a higher consumption growth, a reverse causality between housing price and consumption movements is an expected result.

For confounding unobservables or misattribution, we suspect two major disturbing factors. The first one is an idiosyncratic permanent income shock, or differential trend shock to income growth at the county level. As a demand shock, a positive permanent income disturbance tends to raise spending in both non-housing consumption and housing services, thus boosting consumption and housing prices simultaneously. We proxy this shock using a change in permanent income recovered from labor income process because it is the major type of household income (with a share more than 60%). At the household level, the log wage (for the convenience of explanation, denote it as Y_t^h) is assumed to be a combination of a common factor and an idiosyncratic component, where the common factor is represented by cross-sectional mean (denoted as $Y_t^c = \frac{1}{H_t^c}\sum_h Y_t^h$, where H_t^c is the total number of households in county c at time t) within the county/prefecture while the deviation from that common factor is the idiosyncratic component (denoted as $Y_t^{h,I} = Y_t^h - Y_t^c$). Following Quah (1990), or more recently Blundell et al. (2008), the common factor can be specified as a random walk $Y_t^c = Y_{t-1}^c + \eta_t^c$, where η_t^c is the permanent shock to wage income. While the deviation from the common factor is typically viewed as an autoregressive process with transitory shocks, that is, $Y_t^{h,I} = \rho Y_{t-1}^{h,I} + \nu_t^h$, where ν_t^h is the transitory shock to household labor income. In the UHS data, we have no identifiers to track households across time, hence it is impossible to retrieve transitory shocks. However, given the randomness of sample at the county level, we can get a reliable time series of Y_t^c, which allows us to recover the permanent wage shock η_t^c for each county in the constructed pseudo panel. When we obtain the recovered series of $\widehat{\eta}_t^c$, we construct a weighted average of $\widehat{\eta}_t^c$ using the common factor Y_t^c over the time span of 2003-2009 for each city to get a synthetic measure of the permanent income shock. Mathematically, our proxy for city-level permanent income shock is $\widehat{\eta}^c_{2002-2009} = \sum_{t=2003}^{2009} \widehat{\eta}^c_t \cdot s^c_t$, where the weight $s^c_t = \frac{Y^c_t}{\sum_{t=2003}^{2009} Y^c_t}$.[31]

The second challenging factor originates from the domestic private sector. DPS is of special interest mainly due to its dominant role in driving China's aggregate economic growth, particularly since China's accession to the WTO in December 2001 (see, e.g. Brandt et al., 2012, and Khandelwal et al., 2013). The trade-driven growth may primarily work through an reduction in SOE and expansion of DPS which is more productive and has a higher trade exposure, as documented by Hsieh and Klenow (2009). Therefore, a higher initial share of DPS employment in total employment favors more gains from a trade liberalization shock of the same size. Also, a larger increase in the employment share of DPS might signal a stronger trade liberalization shock and thus a higher growth in DPS at the local level. If the trade liberalization generates a higher growth in a local economy that is economically dependent on the domestic private sector and thus experiencing a more pronounced growth in DPS, we naturally anticipate higher demand of non-housing consumption and housing services in that county/prefecture. To sum it up, for the purpose of properly dealing with endogeneity concerns on housing price movements, we need to establish an exogenous variation that is at least orthogonal to changes in the share of land sales revenue, the permanent shock to labor income during 2002-2009, the initial employment share of the domestic private sector, and the change in DPS employment share between 2002 and 2009.[32]

1.4.3 Identification with Instrumental Variables

In this section, we present the instrumental variable identification strategy to estimate the causal consumption response to housing price shocks.

The identification strategy we rely on is the classic instrumental variable method, which has been widely used in recent empirical studies on economic implications of housing shocks. The

[31] There exists a timing issue when we proxy permanent income shock in this way. It might take time for households to learn shocks, hence permanent income shocks prior to 2002 could be a better choice. We address the timing issue by checking the correlation between permanent income shocks during 2002-2003 and housing price movements during 2003-2009. It produces consistent results.

[32] City-level land sales share can be recovered from various issues of the China City Statistical Yearbook and China Land Resources Statistical Yearbook. Permanent income shock and DPS employment share are from author's calculation, constructed using the UHS microeconomic data.

essence of this identification is to find a source of variation that is: (1) predictive of housing price movements and, (2) exogenous to major disturbing factors that tend to cause issues of reverse causality or misattribution. To be specific, we search for an instrumental variable that is correlated with housing price movements over our sample period, but that does not affect consumption growth or changes through other channels. Formally, we need to ensure the exclusion restriction that requires the instrument affecting household consumption operates only through its impact on housing price movements.

When investigating the consumption response to housing shocks for the U.S. economy, an extensive literature has exploited the variation across long-run housing supply elasticity at the metropolitan statistical area (MSA) level to instrument for housing price movements (see, for instance, Mian et al., 2011, 2013; Adelino et al., 2015; Kaplan et al.; 2016, Stroebel and Vavra, 2016; and Giround and Mueller, 2017). The housing supply elasticity was originally developed by Saiz (2010) using geographic information system techniques, acting as an objective index of the ease with which new housing can be expanded in an MSA. The economic intuition behind this instrument is that for a given positive demand shock, housing prices should rise more in MSAs where housing supply is less elastic. Two arguments are typically cited to support the wide use of the housing supply elasticity as an instrument for housing price movements. First, the housing supply elasticity focuses on an exogenous source of variation in the flexibility of housing supply which is largely determined by exogenous geographical conditions, including terrain elevation and presence of water bodies.[33] Second, as emphasized by Stroebel and Vavra (2016), the housing supply elasticity generates differential regional housing price movements through a differential propagation of a national housing demand shock, not shocks at the local level. It means that the combination of a time-invariant elasticity with a national housing demand shock is enough to generate variation in

[33]There is an alternative measure of housing supply elasticity used by studies on consumption response to housing shocks in the U.S. economy, that is, the Wharton Regulation Index (WRI) from Gyourko et al. (2008). Unlike the elasticity constructed by Saiz (2010) that uses information on the geography, the WRI is a measure of local regulatory environments pertaining to land use or housing, including zoning and project review policies. Though both of the two elasticity measures are highly predictive of housing price movements in the U.S., the WRI suffers from more severe measurement and endogeneity issues. It is harder to measure on the one hand due to multidimensional land-use policies, and endogenous to preexisting land/housing prices on the other.

housing prices across both time and geographical dimensions.

In this study, we construct an instrumental variable for housing price movements that greatly differs from the housing supply elasticity and embodies a Chinese-specific mechanism. However, it will be evident that the underlying logic is comparable between our instrument and the widely utilized housing supply elasticity. The instrument we take advantage of is the college enrollment expansion shock in China between 1998 and 2005 (denoted as $CER^c_{1998-2005}$). It is the product of two components, the number of colleges located in a county in 1998 (COL^c_{1998}) and the expansion of college enrollment during 1998-2005 in the province in which the county is located ($ENR^p_{1998-2005}$), that is, $CER^c_{1998-2005} = COL^c_{1998} \times ENR^p_{1998-2005}$, where p denotes a province.[34]

Thus, our method also combines a time-invariant characteristic at the local level with an aggregate shock (at the province level) over time to generate a plausibly exogenous variation in local housing price movements across both time and cross-sectional dimensions. The primary difference between our instrument and the housing supply elasticity lies in how local characteristics translate the aggregate shock into location-specific variation. We utilize a local characteristic that propagates an aggregate college enrollment expansion shock through the local capacity of accommodating enlarged college enrollment, while researchers employing the housing supply elasticity exploit the propagation of an aggregate housing demand shock through the local flexibility of expanding new housing services. The characteristic we are working with is related to the demand side of housing market because a larger number of local colleges implies more college enrollment and graduates, and hence higher demand for local housing services when a large number of the graduates stay and work locally. In contrast, the housing supply elasticity is a straightforward measure from the supply side of housing market with more inelastic supply rationalizing a larger movement in housing prices. Simply put: our housing demand channel relies on the notion that the college enrollment expansion takes place rapidly (before supply of housing can adjust) and a different rates across locations. Consequently, it generates a rich source of exogenous variation in housing price move-

[34]College here means a broad set of post-secondary educational institutes, including universities, academies, colleges, seminaries, conservatories, and institutes of technology. It also contains certain college-level institutions, such as vocational schools, trade schools, and other career colleges that award academic degrees or professional certifications.

ments. With the instrumental variable $CER^c_{1998-2005}$, the baseline regression in equation (1.2) is changed to a two-stage IV regression:

$$HNW^c_{2002-2009} = \gamma CER^c_{1998-2005} + \varepsilon^c_{0,2002-2009} \tag{1.5}$$

$$\Delta log(C^c_{2002-2009}) = \delta_1 + \theta_1 \times \widehat{HNW}^c_{2002-2009} + \mathbf{Z}^c_{2002-2009} \cdot \boldsymbol{\omega}_1 + \varepsilon^c_{1,2002-2009} \tag{1.6}$$

where $\widehat{HNW}^c_{2002-2009}$ is the predicted value from the first-stage regression in equation (1.5). To improve robustness, we also include the set of county-level controls ($\mathbf{Z}^c_{2002-2009}$) in the first-stage regression, for the purpose of excluding other channels through which the college enrollment expansion shock might affect housing net worth shocks. The coefficient of interest, θ_1 in equation (1.6), captures the causal effect of housing net worth shocks on consumption growth over the sample period 2002-2009. Likewise, the county-level baseline regression in equation (1.4) can be rewritten into a two-stage IV regression:

$$\Delta HV^c_{2002-2009} = \gamma CER^c_{1998-2005} + \varepsilon^c_{0,2002-2009} \tag{1.7}$$

$$\Delta C^c_{2002-2009} = \delta_2 + \theta_2 \times \widehat{\Delta HV}^c_{2002-2009} + \mathbf{Z}^c_{2002-2009} \cdot \boldsymbol{\omega}_2 + \varepsilon^c_{2,2002-2009} \tag{1.8}$$

Similarly, $\widehat{\Delta HV}^c_{2002-2009}$ is the fitted value from the first-stage regression in (1.7) and θ_2 in equation (1.8) is deemed as the causal effect of home value changes on consumption changes.

The college enrollment expansion shock ($CER^c_{1998-2005}$) we exploit roots deeply in the context of Chinese economy and indeed works well as a valid instrument for housing price movements during 2002-2009. In China, almost all college students are admitted through a unified national college entrance exam (CEE). During 1999-2005, China sharply expanded college access for high-school graduates by relaxing the admission criteria based on CEE scores, which generated a subsequent (after four-year college studies) boom in college-educated workers over the period 2003-2009. The college enrollment expansion was implemented mainly as an economic stimulus measure to overcome economic downturn and unemployment surge in the aftermath of the 1997 Asian Financial Crisis. As noted by Che and Zhang (2017), the centrally-planned nature of the Chinese higher education system ensures that the college enrollment expansion policy is arguably a natural experiment that is exogenous to concurrent economic growth trend in China, hence providing us a unique opportunity to identify the impact of housing shocks on household consumption.

The economic channel through which college enrollment expansion drives up housing prices is the notion that these graduates will have more human capital and generate direct and indirect effects that shift housing demand in the locations in which they study and subsequently work. When the college enrollment expansion policy was initially implemented in 1999, an explosive growth in college enrollment emerged and thus a subsequent boom in college graduates occurred in 2003 after four years of education. The increase in college graduates is a massive adding to the stock of human capital (skilled workers) at the local level and translates into substantial demand for local housing services out of two reasons. First, college graduates tend to stay and work locally, which means that there is a strong potential demand for housing because they in general do not own a home when graduating. Even though there is a possibility that students may migrate from their places of study to a different place of work when they graduate, most studies using U.S. data find that on net the local areas that have college-level institutions experience a significant increase in human capital when the students graduate (see e.g. Groen, 2004, and Winters, 2011). The likelihood that a Chinese college graduate stays in the same location where he/she gets higher education is even greater than in the U.S., due to the fact that colleges in a local city typically preferentially admit more high-school graduates within the same province than those from other provinces.[35] They achieve this preferential treatment by either lowering CEE score requirement for within-province high-school graduates or imposing quotas of low quantity for other provinces.

Second, college graduates are skilled workers who, upon graduating, gain a premium in wages, hence have strong purchasing power of housing. For instance, Han et al. (2012) document large (60%-80%) and rising wage premium for Chinese college graduates (in comparison to high school dropouts) between 2002-2009. Though the sharp expansion tends to incur a decline in the quality of college graduates, Han et al. (2012) suggest that China's accession to the WTO created massive

[35] The preferential policy was so prevailing that China's Ministry of Education urged leading colleges to decrease the share of local students to be lower than 30% in 2008.

demand for skilled workers and overcame the decrease in quality through an increase in demand. Che and Zhang (2017) further find evidence that the expansion in college graduates generated a larger gain in total factor productivity for industries using more human-capital intensive technologies. This provides direct support for our reasoning that the sharp expansion in college graduates was absorbed by the disproportionately higher growth of human-capital intensive industries and did not lead to a drop in college premium (instead, the college premium increased). Therefore, when the college graduates start working, it usually takes a shorter time for them to buy a home with higher wages or other benefits than average workers. These two reasons mean that the accumulation of local human capital through higher education results in a substantial and prompt rise in housing demand.

In addition, the externality of human capital tends to further raise demand for housing in a college-intensive local area. Rauch (1993), Simon and Nardinelli (2002), Moretti (2004), and Fu (2007), among numerous others, using data from the United States, show that proximity to educated individuals makes other workers more productive, hence a city with a higher stock of human capital (a.k.a "smart city") induces an in-migration of workers through the knowledge spillover effects. Similar human capital externalities at the city level have been found using Chinese data as well, see Liu (2014) as an example. Hence, through the local accumulation and externality of human capital, *ceteris paribus*, we expect a larger housing price increase in a local area that has experienced a larger college enrollment expansion shock.[36]

We are also cautious about exogeneity when constructing the college enrollment expansion shock. Recall that we formally define county-level college enrollment expansion shock as $CER^c_{1998-2005} = COL^c_{1998} \times ENR^p_{1998-2005}$. To guarantee the exogeneity of college quantity, we use the initial number of colleges in 1998, prior to the higher-education expansion starting from 1999. In principle, we could follow Stroebel and Vavra (2016) by multiplying a national college enrollment expansion

[36]Notice that, to justify our instrument, we need to make an underlying assumption that the supply of housing is relatively inelastic in the short run. This is probably true in China because real estate developers cannot freely increase local housing supply in anticipation of a surge in housing demand. To build more houses, they first need to obtain use rights of more land from local governments, which typically takes a long time due to institutional hurdles like red tapes.

shock with the local college quantity in 1998. Since our sample size is comparatively smaller (42 counties or 59 prefectures), we resort to provincial college enrollment expansion shocks to generate richer variation at the city level. It not only helps us identify a reliable causal effect, but also reflects the vast heterogeneity in college admission across provinces.

Table A.2 demonstrates the validity of college enrollment expansion shock as an instrument for housing shocks. Row (1) and (2) of **Table A.2** reports the results of regressing the housing net worth shock and housing price shock on college enrollment expansion shock, respectively. More local college enrollment expansion sees a larger percentage increase in housing net worth mainly through a larger increase in housing prices. It also induces a greater level increase in home value. Consequently, the "inclusion restriction" or relevance of instrument is verified. Furthermore, the college enrollment expansion shock is orthogonal to all major endogeneity concerns that we could expect in the context of China. Even though we cannot test the exclusion restriction directly, those orthogonality results highly suggest that our instrument survives the most economically relevant challenges to the exclusion restriction. Specifically, we see positive signs for coefficients of change in land shales share, permanent shock to wage growth, change in DPS employment share, and initial DPS employment share in 2002. However, none of the coefficients is statistically significant, meaning that we cannot reject the null hypotheses that they are zeros. In addition, **Table A.2** shows that counties with larger college enrollment increases enjoy a higher population growth (consistent with the externality of human capital that encourages in-migration and urbanization) and wealthier households in terms of disposable income and net worth. To avoid the case that our instrument affects household consumption through population growth, income or wealth status, we also include these characteristics in county-level regressions.

1.5 Empirical Results

This section first presents estimation results for baseline regressions specified at both household and county levels. We start with household-level regressions which have no instrumental variables available, and hence should be viewed as suggestive evidence. We then proceed to more aggregated county-level regressions, in which we implement IV estimations to ensure a reliable casual impact. Next, we present estimation results for extended regressions that explore the heterogeneity in consumption response to housing shocks along several dimensions, including homeownership, income/wealth status, degree of collateral constraint, and durability/income elasticity of consumption goods.

1.5.1 Baseline Estimation Results

Table A.3 collects estimation results for equation (1.1) under various scenarios. Recall that the theoretical model predicts: there should be no consumption response to variation in housing wealth if the full risk-insurance hypothesis holds. To start with, we test the hypothesis by estimating a household-level log-log model as expressed in equation (1.1). Column (1) presents the simplest case where we only control for year and county fixed effects. It produces an elasticity of household consumption to housing price of 0.046 (statistically significant at the 1% level), which means that a 10% increase in housing price on average boosts a 0.46% rise in consumption. It thus soundly rejects the full risk-insurance hypothesis. This estimated elasticity is almost unchanged (0.044) when we control for home characteristics in column (2). Home characteristics are crucial to improve the comparability of homes across households, hence mitigate situations in which the consumption response might be a result of variation in home age, size, architectural style, or facilities and equipments. In column (3), we further address the effect of non-housing wealth on household consumption. Coefficient estimates indicate that household consumption responds to both types of wealth. Yet, the consumption response to housing wealth is larger than that to non-housing wealth, 0.053 versus 0.042. This dovetails with the fact that housing is the major type of wealth for Chinese urban households (with a share of 55% to 75% over our sample period). Besides wealth, we also control for flow variables like household disposable income in column (4). We find a higher response of consumption to income than wealth (0.08 versus 0.04-0.06), which implies that there is no strong motive for consumption smoothness by Chinese urban households.[37] Column (5) adds

[37] Chamon and Prasad (2010) also find contradicting evidence to the life cycle/permanent income hypothesis, which predicts that consumption should be smoothed over the life cycle, and hence not responding effectively to short-term

household characteristics as controls, which include household size (i.e. the number of household members) and share of employment within the household (i.e. the fraction of household members who are employed). It barely changes the consumption responses to wealth and income. We finally consider two additional scenarios in column (6) and (7) where we replace log housing price by log home value in one case and drop households owning homes valuing more than 2 million *yuan* (i.e. villas) in the other. The estimated elasticities are robust to these alternatives. Furthermore, the linear regression model fits the data quite well. The explanatory power of the regression model increases from 0.28 with only housing prices and fixed effects to 0.57 when we include all potential controls.

In **Table A.4**, we demonstrate regression results for equation (1.3) which estimates the average marginal propensity to consume out of housing wealth at the household level. The MPC specification further checks whether the full risk-insurance is tenable with microeconomic evidence from China. In total, we consider seven scenarios as those in **Table A.3**. Column (1) produces a statistically significant (at the 1% level) MPC of 0.9 *fen* per *yuan* for home value (recall that in Chinese currency, 1 *yuan* is equal to 100 *fen*), which says that a 1-*yuan* increase in home value on average leads to a gain of 0.9 *fen* in consumption. The MPC is attenuated by home characteristics and downsized to 0.7 *fen* per *yuan*. Then, the MPC bounces to more than 1 cent per *yuan* when we control for non-housing wealth and disposable income. Likewise, we find a larger MPC out of income than wealth (5.7 *fen* versus 0.06-1.4 *fen* per *yuan*). The estimated MPC remains steadily around 1.4 *fen* when household characteristics are accounted for, and it's also barely affected when home value is replaced by housing price or households with villas are excluded. The linear regression model for MPC also demonstrates a reasonable goodness of fit, which rises from 0.17 in the simplest column (1) to 0.45 in column (5) with more controls.

We present estimation results for county-level regressions in **Table A.5** and **A.6**. As we mentioned, the specifications in equation (1.2) and (1.4) are highly comparable to the empirical studies by Mian et al. (2013) who use county-level data for the U.S. The twin regressions also serve to

changes in income.

test whether the full risk-insurance hypothesis holds at the city level. Column (1)-(4) of **Table A.5** list the results estimated using OLS. The elasticity of consumption to housing net worth is statistically significant at the 5% level, and on average is around 0.13 in column (1) and (2), no matter whether we control for home characteristics or not. In column (3), when we assess the importance of household balance sheet channel as Kaplan et al. (2016), we find that housing price shock has such a strong and direct impact on household consumption that housing net worth shock has no statistically significant effect on consumption at all. Column (4) adds a set of controls including wage growth, population growth, initial employment condition, and initial income and wealth statuses. It reveals that the consumption elasticity is slightly decreased to 0.12 as a result of the impact of wage growth. In column (5) and (6), we instrument the potentially endogenous movements in housing price with college enrollment expansion shock. The elasticity estimate is fairly inflated to around 0.15 when no additional controls are added. The increase in estimated coefficient from OLS to IV might be driven by omitted variables or shocks that primarily induce a negative correlation between housing prices and consumption. For instance, a series of positive productivity shocks to the construction sector (which we fails to control for in OLS estimation due to the unavailability of data) tend to decrease housing price and increase consumption. Since the IV estimation employs exogenous variation in housing prices that is orthogonal to potential endogenous concerns, it also confirms that the OLS estimate is not driven by obvious sources of reverse causation or unobserved confounding factors, like permanent income shocks or trade liberalization shocks to the domestic private sector. The IV estimation drops to 0.13 when a full set of controls are introduced, which might be caused by the positive impact of wage growth. Same as the case of household-level regression, we find a higher elasticity of consumption to wage (a primary portion of household income) growth than that to wealth growth. Even though we have a small number of observations (42 counties), our linear regression model fits the data reasonably well, with an explanatory power ranges between 0.21 and 0.34. It is worthy to mention again that we rely on bootstrapped standard errors for inference at the county level, mainly in consideration of a relatively small sample.

Table A.6 displays the estimated MPC for housing wealth at the county level. Column (1)-(4)

present the OLS estimates. It indicates in column (1) and (2) that a 1-*yuan* increase in home value on average generates a rise in consumption by nearly 3 *fen*. The OLS estimate does not differ too much when the home characteristics or a set of controls (like initial DPS employment share, initial population, and initial income and wealth statuses) are included. Column (3) confirms a nonlinear effect of changes in home value on changes in consumption. The negative coefficient on the squared term of changes in home value suggests that the estimated MPC is larger for small increases in home value, but gets smaller as the rise in home value gets larger. The nonlinearity is consistent with the concavity of wealth effect on consumption, which predicts a decreasing MPC out of wealth when wealth increases. Again, in column (5) and (6) we instrument the change in home value with college enrollment expansion shock. The IV estimate in column (5) is slightly larger than the OLS estimate in column (1), 3.2 *fen* per *yuan* versus 2.9 *fen* per *yuan*. The discrepancy could be caused by omitted variables/shocks that induce a negative correlation between housing price and consumption. When we add a set of controls to the IV estimation, the MPC is almost unchanged. In terms of goodness of fit, the MPC specification has a R^2 lying between 0.36 and 0.60.

In a nutshell, our baseline regressions exhibit that the elasticity of consumption with respect to housing price is around 0.05-0.13, and the MPC out of housing wealth is around 1-3 *fen* per *yuan*, depending on the level of spatial disaggregation of the data employed. Since we can only implement IV estimation at the city level, we treat the county-level estimates as more reliable causal effects while the household-level estimates as suggestive evidence of correlation. In that sense, the elasticity (to housing wealth) estimate lies between 0.12 and 0.13 (or equivalently, the elasticity to housing price lies between 0.06 and 0.07, given the fact that average share of housing wealth in household net worth is around 56% for homeowners in 2002), and the MPC is 2.5-3 *fen* per *yuan*. Economically, these estimates are significant, given the fact that Chinese average housing prices has increased by more than 250% or 3,000 *yuan* per square meter over the period 2002-2009. We can illustrate the importance of housing wealth shock for household consumption using the MPC estimate as an example. Suppose that a typical Chinese urban household has a home of 75 square meters in 2002, then the increase in average housing prices by 3,000 *yuan* per square meter during 2002-2009 generates an increase in housing wealth as much as 225,000 *yuan*, which is further translated into a rise in consumption by 5,625 to 6,750 *yuan*. In 2009, this increase in consumption induced by housing appreciation amounts to 13.8%-16.6% of the current consumption expenditure for a representative homeowner in our sample.

Though we include different control variables in county-level elasticity and MPC regressions, these two estimates are consistent. Think of the formula of elasticity as $\frac{\partial C}{\partial W} \times \frac{W}{C}$, then the elasticity equals to $MPC \times \frac{W}{C}$, where W is housing net worth and C is consumption. During 2002-2009, average value for $\frac{W}{C}$ was 6.8 for homeowners. Then, with an MPC of 2.5-3 *fen*, it implies that the elasticity is 0.17-0.21. The difference between 0.17-0.21 and 0.12-0.13 is caused by the fact that when estimating elasticity and MPC we add different controls. However, in both qualitative and quantitative senses, the elasticity and MPC estimates are fairly consistent.

1.5.2 **Heterogeneous Consumption Response**

We extend our baseline regressions to different dimensions of heterogeneity in this subsection. We first investigate the consumption response of home renters to housing shocks, which manifests the role of homeownership. Then, we explore how variation in income/wealth status alters the consumption response. Next, we evaluate the role of debt or equivalently the importance of collateral constraint channel by exploring how housing leverage ratio affects the consumption response. Finally, we examine the heterogeneous impacts of housing shocks on different categories of consumption, which are differentiated by durability and income elasticity.

1.5.2.1 **Homeownership Matters**

Homeownership is an important source of heterogeneity for consumption response to housing shocks. It could be directly related to the income effect of housing price movements on consumption. When a household owns a home, an appreciation in home value generates a straightforward income effect and boosts household consumption. However, it causes no impact on the consumption of home renters via this channel because they do not reap wealth appreciation in housing. Homeownership is also related to the intertemporal substitution effect of housing price movements on consumption. If the housing market is complete, the equilibrium housing price will be the discounted sum of rents over time. In that case, home renters should anticipate an increase in rents in the future and the intertemporal consumption smoothing motive will encourage them to reduce current consumption. In the baseline regressions, we have found that the consumption response of homeowners to housing price movements is statistically and economically significant. A natural question to follow is whether the consumption of home renters responds to housing price movements via the intertemporal substitution channel (negative effect) or spillover effect from the consumption of homeowners (positive effect).

Since we cannot construct household-level housing price or home value for renters who do not have homes, we regress household consumption of renters on county-level house price to evaluate the consumption response for them. **Table A.7** reports the OLS estimates under different scenarios. When we do not control for household income, it shows in column (1)-(2) that the regional housing price at the county level tends to be positively correlated with the consumption of renters, though just marginally statistically significant. However, the correlation disappears when we control for household disposable income in column (3) and (4). This result suggests that renters do not respond to housing shocks. However, the insignificant consumption response of renters might reflect the fact that two counteracting forces just cancel each other. As we argued earlier, housing shocks within a county could affect renters' consumption via at least two channels. The first is the direct channel of intertemporal substitution effect that operates at the household level, hence it should have been accounted for in the first four columns of **Table A.7**. The second one is an indirect channel that works through the spillover effect of consumption increase by homeowners within the same county when they experience appreciation in their homes. In column (5), we control for this indirect spillover effect by including an interacted term of year and county fixed effects. It turns out that we still obtain an insignificant consumption response for renters. As a whole, these results reflect that housing shocks affect the consumption of households mainly via the direct wealth effect or other channels requiring the ownership of homes, rather than the intertemporal substitution effect or spillover effect explored in this subsection.

1.5.2.2 **Heterogeneity across Income and Wealth Distribution**

In the literature, theoretical researches like Carroll and Kimball (1996) have derived a concave consumption function in wealth and permanent income with choice under uncertainty. In this subsection, we aim to test the concavity of consumption directly by revealing how wealth and income status affect the consumption response of homeowners to housing shocks. A quick way to check it is to include an interacted term between changes in home value and initial household income or wealth in 2002. We introduce this interacted term to the regression specified by equation (1.4), and find a statistically significant (at the 5% level) and negative estimate for the interacted term with disposable income as -0.62 and with net worth as -0.018. For brevity, we do not report detailed regression results here. Since income and wealth are quite widely dispersed in China, with a Gini coefficient of 0.34 for household disposable income in 2002 and a Gini coefficient of 0.53 for household net worth in 2002, we could easily divide the income and wealth distribution within a prefecture into several meaningful intervals.[38] We then implement the prefecture-level regressions as specified by equation (1.4) for each income or wealth interval. This provides us an intuitive way to show the heterogeneous MPCs across the initial income and wealth distribution in 2002.

Panel (A) of **Figure A.6** displays heterogeneous MPCs across the distribution of real disposable income in 2002. All the MPCs are statistically significant at the 5% level. It exhibits that households within a group of higher income have a weaker consumption response to changes in home value, which resonates well with the theoretical prediction by Carroll and Kimball (1996). Specifically, the estimated MPC out of housing wealth is less than 0.3 *fen* (to be precise, 0.28 *fen*) per *yuan* for the group of households with an annual disposable income higher than 75,000 *yuan*.

[38] Since we have a relatively small sample size (with a mean value of 93.7) at the county level, we employ prefecture-level income and wealth distribution to implement the division. At the prefecture level, our average sample size is close to 300, which helps to generate a meaningful number of households for each income/wealth interval within a prefecture.

In contrast, it increases to 2.2 *fen* per *yuan* when the income interval downsizes to be no more than 15,000 *yuan*. It also shows a monotonic decrease in MPC when household income expands. The similar concavity and monotonicity is confirmed by Panel (B) of **Figure A.6** where we plot heterogeneous MPCs across the distribution of real net worth in 2002. The MPC estimate is around 1.5 *fen* per *yuan* for low-wealth households with net worth no more than 50,000 *yuan*. However, when households are from the wealth interval higher than 300,000 *yuan*, the MPC declines to be just slightly greater than 0.3 *fen* per *yuan* (to be precise, 0.34 *fen* per *yuan*).

1.5.2.3 **The Role of Collateral Constraint**

It has been well recognized that housing is the major or even only type of collateral accepted by lenders like commercial banks in China (see, for example, Fang et al., 2015) probably due to its primary importance in household wealth and impressive stability in maintaining value (i.e. low risk and strong potential for appreciation). In this study, we follow Mian et al. (2013) to construct a measure for collateral constraint, i.e. housing leverage ratio, defined as the ratio of mortgage plus home equity debt (HELOC) to home value. The representativeness of this measure is thus guaranteed by the position of prominence that housing holds in serving as collateral for borrowing. We also mentioned earlier that the housing leverage ratio we construct using the UHS data is much lower than its true value because we only have information on new mortgages added within the year while no records of all existing mortgages for a county. We argue that the underestimated measure is still quite informative of leverage at the local level if existing mortgages/HELOC is positively correlated with new mortgages. It also turns out that our housing leverage ratio is smaller than the U.S. counterpart constructed by Mian et al. (2013). The mean of housing leverage ratio is 14.3% in 2002 for our sample and 61.6% in 2006 for the U.S. data. However, we still treat the housing leverage ratio as an effective measure to characterize the variation in collateral constraint across counties in China, given the fact that the standard deviation is as high as 37.1%. This generates a considerably larger coefficient of variation than the U.S. case where the standard deviation is 22.9%, only slightly than one third of the mean. If the collateral constraint channel works, we should expect that counties which are more borrowing constrained due to a larger housing leverage ratio may respond more aggressively to housing shocks. We test the importance of this channel by adding a cross term for changes in home value and housing leverage ratio. To avoid endogenous change in leverage, we utilize the initial housing leverage ratio in 2002.[39]

Table A.8 presents the heterogenous average marginal propensity to consume out of housing wealth driven by the variation in initial housing leverage ratio in 2002. We conduct the regression analysis at two different levels. First, we employ household-level data, and regress total consumption on home value and the interacted term of home value multiplying county-level housing leverage ratio. Column (1) and (2) show a strong positive effect of leverage on consumption response to housing shocks. The estimated coefficient for the interacted term of home value and housing leverage ratio is also statistically significant at the 1% level. The result is unchanged when we control for household-level controls and home characteristics. This result implies that homeowners in a more borrowing constrained county respond more aggressively to housing shocks, hence suggesting an important role for collateral constraint. In column (3) and (6), we rely on county-level data to test the role of leverage. In the OLS scenarios, we find statistically significant (at the 5% level) estimates for the interacted term of change in home value and housing leverage ratio. It is fairly magnified when we implement IV estimation, no matter whether county-level controls and home characteristics are included or not. Also, notice that we control for year and county fixed effects for all household-level regressions and province fixed effects for all county-level regressions. As a whole, the regression results strongly indicate an important role of collateral constraint in affecting consumption response to housing shocks, which is in line with the finding by Mian et al. (2013) for the U.S. and Disney et al. (2010) for the U.K. Furthermore, our finding implies that the credit constraint matters on the demand/consumer side in China as well. There have been a lot of studies showing the nontrivial impacts of credit constraint on the supply/producer side, especially for

[39]To some extent, this helps us to explain why we choose not to construct a stock of mortgages out of newly added mortgages in a way like the neoclassical law of capital accumulation. Since we employ the housing leverage ratio in 2002 for regressions, there will be no gains to replace new mortgages with accumulated mortgages at the county level as the two are the same for the initial year. Moreover, it may be also quite discretionary for us to pin down the "depreciation" rate of existing mortgages, for which we have no microeconomic evidence.

private domestic firms (see Feenstra et al., 2014, and Manova et al., 2015, among a vast number of papers). We add to this strand of literature by showing that the borrowing constraint also tends to shape consumption in a nonnegligible way by enhancing the marginal household consumption response to appreciation in home values.

1.5.2.4 **Heterogeneity across Categories of Consumption**

The last source of heterogeneity in MPC is governed by its durability or income elasticity of consumer goods. In terms of durability, numerous existing studies (such as Monacelli, 2009; Engel and Wang, 2011; and Álvarez-Parra et al., 2013) have shown a higher responsiveness of durable consumption to exogenous disturbances like monetary or productivity shocks because the stock-flow relationship governing durables implies that a small change in desired stock leads to a large movement in purchases. In accordance with these predictions, we should expect that durable consumption like household appliances is more sensitive to wealth shocks than nondurables like food and clothing. Likewise, the income elasticity also manages to guide the consumption response to housing shocks according to the rationale that goods with higher income elasticity tend to respond more aggressively to a change in housing wealth, which can be interpreted as a change in perceived asset income.

Figure A.7 presents the heterogeneous MPCs across various categories of consumption. Notice that all the estimates of MPC are statistically significant at the 5% level, except for the insignificant MPC estimate associated with housing services. When tracking the role of durability, the graph clearly shows that durable goods have a higher MPC than nondurables, 0.9 *fen* per *yuan* versus 0.66 *fen* per *yuan*. Hence, we find supportive evidence for the stronger responsiveness of durables to exogenous shocks as we employ the exogenous variation in housing wealth by implementing an instrumental variable method. As for the role of income elasticity, **Figure A.7** demonstrates a result consistent with our expectation that services has an MPC estimate that is much larger than that for food and clothing. Actually, the former one is more than three times of the latter: 1.28 *fen* per *yuan* against 0.37 *fen* per *yuan*. In **Figure A.7**, we also plot the estimated MPC for housing services. Housing services is of particular interest for us because it is a type of services with a high income elasticity on the one hand and a type of consumption that homeowners rarely adjust in the short run due to the ownership of homes on the other (also recall that we have excluded housing consumption from total consumption and services consumption when evaluating their responses to housing wealth shocks). We regress the consumption of housing services for homeowners (which are self-estimated by homeowners) in this graph and it turns out that the persistence in ownership attached to homes curbs the response of housing services to housing wealth shocks (statistically insignificant even at the 10% level). This is because the service flows of housing services is proportional to the stock of housing which is slow to adjust. The irresponsiveness in principle might result from the high adjustment costs involved in changing homes, like searching for new homes, relocating children to new schools, and direct charges for moving.

1.6 **Robustness Checks**

In this section, we provide various robustness checks for baseline regressions. We only focus on city-level regressions as they manage to produce reliable causal consumption responses to housing shocks with instrumental variables. We start with prefecture-level regressions, then adjust for error in variables, next try a hedonic housing price, and finally account for the habit formation in consumption. It turns out that our baseline county-level regressions are quite robust to these concerns as they produce consistent estimates for elasticity and MPC. The statistical significance and qualitative feature of the baseline estimates are largely unchanged.

1.6.1 **Prefecture-level Regressions**

We first check whether our estimates of consumption responses are a result of the specific sample of counties we employ (42 counties). Like the case of counties, we construct variables including consumption (excluding housing services), housing, and other characteristics for prefectures. Since we need to calculate growth or change in variables during 2002-2009, we keep prefectures that have observations in both the starting and ending years. By this criterion, we obtain a balanced panel for 59 prefectures over the period 2002-2009.

First of all, to gain some intuition, we also plot the correlation between housing shocks and consumption movements for those 59 prefectures in Section F of this paper's **Online Appendix**. It clearly shows that there is a strong positive correlation between growth in housing price and consumption during 2002-2009 at the prefecture level. The coefficient of correlation is 0.56, very close to that in the case of counties (0.62). It further exhibits a strong positive correlation between change in home values and change in consumption over our sample period. The coefficient of correlation reaches 0.77, just slightly lower than 0.82 for the case of counties. These correlation patterns again lend strong support for a nontrivial consumption response to housing shocks. Likewise, this is only suggestive, and we need to employ more rigorous econometric methods to identify the causal impact of housing price movements on household consumption.

We construct college enrollment expansion shock at the prefecture level to serve as an instrumental variable for housing price movements, in a similar way as for counties. To illustrate that the prefecture-level instrument also captures an exogenous source of variation in housing price movements, we regress housing shocks and other economic variables out of endogeneity concerns about the college enrollment expansion shock in Section F of this paper's **Online Appendix**. It turns out that the instrument is highly predictive of movements in housing net worth, housing price, and home value, which means that the "inclusion restriction" of the instrument is achieved. The college enrollment expansion is also orthogonal to various endogeneity concerns, like change in land sales share for the concern of reverse causality and permanent income shocks for the concern of misattribution. Moreover, the results indicate that the instrument is positively correlated with population growth, initial income level, and initial wealth status. To rule out the possibility that housing shocks affect household consumption via its impact on these variables, we add them as controls in prefecture-level regressions.

Table A.9 and **Table A.10** report estimation results for equation (1.2) and (1.4) at the prefecture level, analogous to what was reported earlier in the case of counties. Column (1)-(4) of **Table A.9** are OLS estimates of elasticities, which are all statistically significant at the 5% level. The OLS estimates are somewhat smaller than those in county-level regressions when we do not include a full set of controls (0.11-0.12 versus 0.13). However, the difference disappears when controls like population growth and other initial conditions in 2002 are added. It again confirms that the household balance sheet channel discussed by Mian et al. (2013) is not important, as shown in column (3). In column (5), the prefecture-level IV estimate of elasticity is 0.126, slightly smaller than the counterpart at the county level (0.149). And, the elasticity is not much affected when we account for a full set of controls. Compared with county-level regressions, a noticeable change is that the goodness of fit rises substantially at the prefecture level, from 0.34 to 0.65 for the scenario of IV estimation with a full set of controls, probably resulting from an increase in the number of observations. In **Table A.10**, the OLS estimates in column (1) and (2) are also statistically significant at the 5% level and are slightly smaller than those obtained at the county level. When we add in the full set of controls, the prefecture-level MPC surpasses that at the county level (0.033 versus 0.025). This pattern remains when we implement IV estimation in column (5) and (6). The prefecture-level regression also exhibits a nonlinear effect of change in home value on change in consumption, and the explanatory power of the linear regression model increases when compared with the county-level regression.

1.6.2 Error-in-variable Estimation Results

Errors in variables emerge when we employ sample means across households to represent population means at the city level.[40] The derivation of error-in-variable (EIV) estimator is analogous to Deaton (1985). We present it in Section E of this paper's **Online Appendix**. To account for error in variables, we proceed in the following two steps. First, we introduce the correction for error-in-variable bias in the context of OLS estimation. Second, we combine the correction for error-in-variable bias and endogeneity in housing price movements, which literally means that we

[40] One concern might be that why we do not treat the estimates with error-in-variable correction as baseline results. The reason is that averaging across households within cities tends to reduce measurement error of variables at the household level while it introduces a new type of measurement error at the city level by construction when we employ sample averages to replace population means. Since we are not sure which measurement error is more severe, we treat the error-in-variable estimation as a robustness check rather than the baseline result.

adjust for the issue of measurement error in the context of IV estimation. To remind the reader, we use bootstrapped standard errors for these EIV-adjusted estimates, mainly for the reason of a small sample at the city level.

The EIV estimates are presented in **Table A.11** and **A.12**, for the elasticity and MPC, respectively. Column (1)-(2) of **Table A.11** are the EIV estimates of elasticities for the OLS case. The correction for measurement error enlarges baseline OLS estimates, no matter whether a full set of controls are included or not. For instance, it is increased from 0.118 to 0.126 with controls. This suggests that the measurement error, which haunts all county-level variables we construct, biases baseline OLS estimates downwards. In column (3)-(4), the correction for measurement error is implemented with IV estimation. It generates consistent results with the OLS estimation. The EIV estimates are also larger than original IV estimates, 0.153 versus 0.133 with a full set of controls. As for the MPC, we find similar inflation of OLS or IV estimates when the correction for measurement error is introduced. Column (1)-(2) of **Table A.12** reveal that the OLS estimates of MPC increases and goes marginally beyond 3 *fen* per *yuan*. While in the IV case, the MPC inflates to 3.5 *fen* per *yuan* with all potential controls.

1.6.3 **Hedonic Housing Price**

Another concern for our baseline estimates relates to the measurement of housing prices. Though we have adjusted for the measurement error resulting from aggregation using the EIV estimator *à la* Deaton (1985), we still face another measurement problem with respect to housing prices. That is, how to ensure the comparability of homes when constructing housing prices at the city level. In our baseline regressions, we have tried to mitigate the issue of comparability by controlling for a broad set of home characteristics including home age, size, architectural style, facilities and equipments. However, there might be other unobservable factors that affect the comparability of homes, such as locations within the city and developers who build homes. To further ensure the comparability of homes, we utilize the prefecture-level hedonic housing price index constructed by Fang et al. (2015) to measure housing price movements. The hedonic housing price index compares homes developed by the same developer within the same project that is implemented successively over years, hence improving the comparability of homes to a quite large extent. Since the hedonic price index is standardized, we are not allowed to estimate MPC using this housing price. We hence only focus on the elasticity estimate for this alternative housing price.

To show how the hedonic price index compares to our housing price, we plot the housing price growth between 2002 and 2009 using these two measures in Section F of this paper's **Online Appendix**. The two growth rates track each other very closely except for three prefectures, that is, Guangzhou, Wenzhou, and Ningbo. Furthermore, the correlation between them is as high as 0.85. This indicates that we should expect very similar elasticity estimates when replacing our housing price with the hedonic one. **Table A.13** presents the regression results under six scenarios with the alternative housing price. It shows clearly that the baseline estimates are somewhat augmented in almost all scenarios, yet the difference is not large. For example, the IV estimates with a full set of controls generates an elasticity of 0.133 while the baseline prefecture-level regression produces an IV estimate of 0.124. Notice also that we only have a sample of 34 prefectures overlapping with that in Fang et al. (2015). This helps to explain the declined goodness of fit in **Table A.13** as the sample size decreases from 59 to 34.

1.6.4 **Panel Regression and Habit Formation**

The last robustness check we are exploring is to extend our baseline cross-sectional estimation at the city level to a panel structure. The aim of this extension is to deal with the concern on the small sample size we have in the baseline city-level regressions (42 counties or 59 prefectures). We present in this study the panel data regressions at the county level, and it could be easily done for prefectures as well. When implementing the panel regression, we define annual housing net worth shock and consumption growth in a similar way as we construct them for the seven-year interval (2002-2009). The annual frequency excludes us from IV estimation because our instrumental variable (college enrollment expansion shock) is time-invariant. We first employ the standard fixed-effect estimation for the panel regression and then correct for measurement error.

The standard errors for the EIV estimates are computed with the help of the asymptotic variance covariance matrix because we have relatively large sample size in the panel case.

Besides the static panel regression, we also rely on the dynamic panel data model to account for habit formation in consumption. Since the seminal work of Abel (1990), a vast number of studies have pointed out the existence of habit formation in consumption and derived its implication in various situations like the design of optimal monetary policy. In our work, the habit formation might render the static panel regression unreliable because the omitted lagged consumption term could cause a correlation between the idiosyncratic error and housing price on the condition that there are trend components in both consumption and housing prices. If housing prices and lagged consumption are positively correlated (which is the case in our sample), the coefficient of house prices will be overestimated. Furthermore, to deal with standard endogeneity issues that arise in dynamic panel regression, the GMM estimator proposed by Arellano and Bond (1991) is deployed.

Table A.14 and **Table A.15** report panel regression results for elasticity and MPC, respectively. Column (1)-(2) of **Table A.14** demonstrate that the elasticity estimate decreases to 0.07-0.09 from 0.12-0.13 in the baseline county-level regression. Though correction for measurement error pulls up the estimates (0.09-0.12), they are still lower than the baseline results. Since a causal effect relies on the instrumental-variable estimation, which we cannot implement in the panel setting, it is impossible for us to reach a decisive conclusion by comparing the FE estimates with OLS estimates in the baseline cases. Nevertheless, the panel regressions produce qualitatively consistent results with our baseline county-level regressions. When habit formation in consumption is considered, the dynamic panel regression confirms that omitting lagged consumption will biases the FE estimates upwards. The dynamic panel estimates of elasticities lie between 0.06 and 0.08, lower than the FE estimates in the range of 0.07-0.09 under corresponding scenarios. It also shows that there is strong evidence for habit formation in consumption, the lagged consumption growth has an estimated coefficient as high as 0.2-0.3, statistically significant at the 5% level. **Table A.15** exhibits a similar pattern, the MPC estimates in the FE estimation are smaller than those in the baseline county-level regressions, 2 *fen* per *yuan* versus 3 *fen* per *yuan*. However, the FE estimates grow larger than the baseline OLS estimates after we adjust for the measurement error. Also, the FE estimation produces overestimated MPCs when it abstracts from habit formation in consumption.

1.7 Conclusion

After the marketization of housing service in 1998, China has experienced an extraordinary housing boom in which real housing prices have risen by over 10% per year starting from 2003. At the same time, Chinese household consumption has been increasing dramatically and steadily at a comparable magnitude. A natural question emerges when we are trying to explore the comovement between housing prices and consumption, that is, how does the change in housing prices contribute to the rise of household consumption? Equivalently, the question could be stated as: how does consumption respond to housing shocks in the context of a large developing economy like China where housing price movements are strongly positive?

We draw on a comprehensive microeconomic dataset on Chinese urban households, i.e. the Urban Household Survey data, to explore the consumption response to housing shocks. Our primary task is to identify a clear and reliable causal effect of housing price movements on household consumption. To this end, we employ an instrumental variable method which helps us to exploit an exogenous source of variation in housing price movements. Specifically, we instrument housing price movements with college enrollment expansion shock at the city level. The college enrollment expansion shock measures the variation in housing price movements originated from an exogenous expansion in higher education which leads to an increased demand for housing services in local urban areas. We show that the college enrollment expansion is quite predictive of housing net worth shocks, housing price shocks, and changes in home value, and also orthogonal to major endogeneity concerns related to housing price movements, like reverse causation and misattribution.

Using the Urban Household Survey data over the period 2002-2009, we estimate an elasticity of consumption with respect to housing price of 0.06 to 0.07 for homeowners. Moreover, we find that the average marginal propensity to consume out of housing wealth is 0.025 to 0.03. The estimates are economically significant because they imply that the increase in consumption induced by housing appreciation during 2002-2009 amounts to 14%-17% of current consumption in 2009 for a representative urban homeowner. We also show that the consumption response to housing shocks is insignificant for renters. Furthermore, we explore the heterogeneity in consumption response along several dimensions. We find that the marginal propensity to consume out of housing wealth is higher for households residing in poorer and more collateral constrained cities. Durability and income elasticity of consumption goods also help to enhance the consumption response to housing shocks. As for our baseline estimates, we show that they are robust to error in variables that emerges when we replace population means with sample averages. They also survive robustness checks on the price adjustment for hedonic characteristics of individual homes and habit formation in consumption.

CHAPTER 2

The International Diffusion of the Automobile from 1913 to 1940

with Mario J. Crucini, Hyunseung Oh and Hakan Yilmazkuday

2.1 Introduction

This paper tells a coming-of-age-story of the automobile. Our narrative begins in 1913, one year prior to Henry Ford's introduction of full assembly-line production of the Model T chassis. This process innovation has been credited with reducing labor input requirements for chassis production from 12.5 to 1.5 (hours), a factor of more than 8 (Baldwin et al., 1987). Shortly thereafter the average U.S. wholesale price of the automobile plummeted from $ 1,227 in 1913 to $ 659 in 1917 (in 1940 USD).[1]

Figure A.8 presents a more complete history of the average U.S. domestic wholesale price and export unit values from 1913 to 1940. The distinctive convex shape of the time-path of the relative price of the automobile is characteristic of new product diffusions. The relative price declines very rapidly in the first decade and the rate of change decelerates until the mid-1920s. The path of the relative price over time and across countries will be the key driver of product diffusion in our model. Notice also that the average markup of export prices over domestic sales is about 20% at the start of the sample and falls to zero by the mid-1920s. As we shall show later in the paper, there is considerable cross-country variation around this average.

Based on theories of product diffusion one would expect automobile registrations per capita to rise quickly from a zero base toward a saturation point as the relative price settles into its long-run stationary position (which is about $550 in real 1940 dollars in the case of the automobile). **Figure A.9** presents time series on automobile registrations for the United States and an aggregate of 23

[1] By wholesale price we mean the average unit value of domestic sales divided by the quantity sold domestically. Automobile industry census of manufacturing data in 1929, 1931, 1933 and 1935 allow benchmark comparisons of the these wholesale prices to average prices of automobiles at the factory gate. All are within 4% of the annual data from the U.S. statistical abstract.

other nations for which we also have macroeconomic data.[2] As the relative price of automobiles falls, aggregate adoption levels rise and when relative prices stop falling, adoption rates tend to stabilize. Once the stock reaches its steady-state, neoclassical theory predicts the stock of automobiles will follow business cycle dynamics. This behavior is evident from 1930 onward with a large drop in the U.S. and foreign automobile stock in Great Depression. A striking feature of **Figure A.9** is that the U.S. automobile stock reaches 200 automobiles per 1,000 population compared to 20 in foreign countries. The apparent voracious appetite for automobiles in the U.S. has been noted in the historical literature, but as far as we know there has been no systematic documentation of international diffusion patterns over time nor explanations for differences across them. Our goal is to fill this gap.

In doing so, we place an emphasis on international trade frictions between the globally dominant producer (the United States) and consumers in destination markets. The level of the friction necessary to account for the magnitude of the automobile adoption gap will, of course, hinges on the elasticity of substitution between home and foreign goods. While the precise magnitude of the trade elasticity remains the subject of active investigation, the recent literature has narrowed the range of plausible values from a low of about 3 (the lower bound reported in Simonovska and Waugh, 2014) to a high of about 13 (the upper bound of estimates in Romalis, 2007). Estimates can be difficult to compare across the literature due to the different data, time periods and estimation methods employed. Anderson, Larch and Yotov (2015) use a single panel (82 countries from 1990 to 2011 from the 8.0 version of the PWT) and report elasticity estimates spanning almost the complete range found in our survey of the literature: 4 to 11.

Notice the challenge this poses in the present context. If the true elasticity of substitution is 11, the ad-valorem-equivalent wedge needed to account for the 10-fold automobile stock difference (in **Figure A.9**) would equal 23%. In contrast, if the true elasticity is 4 the wedge rises to 78%. This would seem to imply that the trade elasticity relevant for the automobile is at or above the upper

[2]The 23 countries in **Figure A.9** are: Argentina, Australia, Brazil, Canada, Chile, Colombia, Denmark, France, Greece, Italy, Japan, Mexico, New Zealand , Norway, Peru, Philippines, Portugal, Spain, Sweden, Switzerland, the United Kingdom, Uruguay, and Venezuela.

range of estimates in the existing literature because the only measured trade friction in **Figure A.8** (the markup in export unit values) is almost always below 23%. Perhaps more damaging, the markup is basically zero from the late 1920s onward. This casual inference, however, would be wrong.

Our first result turns this casual inference on its head. We measure additional international frictions (beyond the export markup) including tariffs, shipping costs, and retail distribution costs at the destination and estimate the total friction between U.S. and foreign markets averages 82% across countries. This friction rationalizes an elasticity of about 4, which is at the lower end of estimates in the existing literature. The lesson here is both simple and profound. The equilibrium volume of trade depends on elasticity-wedge pairs – (11, 0.23) and (4, 0.78) – and so forth. When trade frictions are under-estimated the trade elasticity is over-estimated (and visa versa).

Our second result provides a decomposition of the total empirical wedge into various frictions. Here we focus on the average across the 23 countries. The largest component, the Penn Effect, accounts for 36% of the total friction. The Penn Effect takes into account the fact that poor countries have relatively low price levels and thus the automobile will be relatively expensive compared to a rich country facing the same common-currency price for an automobile. The next largest friction is the tariff which accounts for 26% of the total friction. The markup accounts for 20% and trade costs another 18%. To place these numbers into a modern perspective, Crucini and Yilmazkuday (2014) use an extensive microeconomic price panel and find that 32% of international price dispersion is due is accounted for by the Penn Effect, 18% by borders (tariffs and other official barriers to trade), 23% due to distance (trade costs) and 26% is unattributed. Their model did not include a role of markups so the upper bound on markups would be the residual, 26%. That tariffs are elevated is not surprising given the historical period, the remaining proportions are similar to the extent comparisons are appropriate. One caveat is that the standard deviation of prices averages about 34% in the 1990-2005 period when pooling all goods and services in the consumer basket in the Crucini and Yilmazkuday (2014) study, modest compared to the 82% wedge between U.S. and foreign trading partners in the interwar period in the automobile industry.

Our third result delves into cross-country differences in the theoretical and empirical wedges. The cross-country standard deviation of the total wedge is 30%. The Penn Effect and markups account for about 40% of the cross-sectional variance each. The tariff picks up the rest because our current trade cost estimate is equal across countries (thus contributes nothing to cross-sectional differences).

Finally, our empirical wedge accounts for 85% of the cross-country average theoretical wedge and 43% of the cross-country variance in the theoretical wedge.

2.2 Product Diffusion in Historical Data

Economists have measured product diffusion in two different ways. One measure takes flow purchases of the good (or accumulated stocks in the case of durables) and normalizes the quantity purchased by an aggregate scale variable with the aim of finding an economic saturation point for the ratio of the two. The automobile is a good case in point. Because very few households will find it optimal to exceed one passenger vehicle per driving-age household member, a natural scale variable for the automobile is population. Going back far enough in time, the level of adoption is zero and it rises toward a stationary level per capita. Typically there is a period of acceleration and deceleration in the adoption level with an inflection point in between (where the adoption profile switches from convex to concave).

An alternative measure is intended to document how new products or technologies replace old ones. The simplest such measure takes a simple count of a new and old vintage (or vintages) and divides each count by the total count across existing vintages. This approach has the desirable feature that it measures the market share across vintages. However, it can be misleading if not carefully constructed because new products are different from old ones and some attempt must be made to convert them into a comparable flow of consumption services (or production services in the case of inputs).

A surprising feature of the existing diffusion literature is how often just two vintages of products are used to characterize the relevant product space. In part this may reflect researchers selection of products or technologies that are differentiated enough to be economically interesting.[3] One prominent historical example is the replacement of the steam engine with the diesel engine in the powering of U.S. locomotives. This process started in about 1935 and was not completed until the early 1960's.[4] A more recent example is rapid access memory (RAM) which also has consisted of at most two vintages at any point in time. In sharp contrast to locomotive engines, the product cycles of RAM capacities are completed in only a few years (see, Jovanovic and MacDonald (1994)). It is worth noting that the socioeconomic impact of the move from steam to diesel is demonstrably more amenable to economic measurement than the advance of RAM in personal computers. Part of the reason is that energy can be measured on a uniform scale (e.g., joules) and using the laws of physics a conversion into real friction reduction is tractable. While the stock of RAM is readily quantifiable, the end economic uses to which it is put goes largely unmeasured.

In the context of the passenger vehicle similar modeling challenges present themselves. Perhaps the most complete chronology for automobiles is the volume by Baldwin et al. (1987) who summarize makes and models of automobiles spanning a century (1885 to 1987). The authors devote at least a few paragraphs to each the top 1,000 makes out of approximately 4,000 business historians have unearthed. In the interest of empirical and theoretical tractability, our focus on the displacement of pre-existing personal transportation services (e.g., bicycles, horse and buggy, and public transportation) by the passenger automobile. Effectively we treat the automobile and 'other' as two distinct product vintages and leave the microeconomic details of varieties within each category to future work.

2.3 **The Data**

The lion's share of data used in this study is a newly constructed historical panel of U.S. automobile exports to worldwide destinations. These data were collected from an annual report by the Director of the Bureau of Foreign and Domestic Commerce to the Secretary of Commerce called:

[3]For example, focusing on electric appliances that replace mechanical ones rather than roller blades replacing roller skates.

[4]Here, "started" and "completed" mean dominance of one of the two technologies in powering locomotives.

the Foreign Commerce and Navigation of the United States (FCNUS). Specifically, the data retrieved are the dollar value and number of passenger vehicle exports to each destination country reported in Table No. 3, Exports of Domestic Merchandise From the United States, by Articles and Countries, During the Calendar Year. This is an unbalanced panel spanning 81 nations from 1913 to 1940. Due to the empirical demands of our project most of the analysis focuses on a balanced panel of 23 countries for which macroeconomic and tariff data are available.

The export quantity data is combined with foreign production data and a stock-flow model to estimate the stock of passenger vehicles in each destination market. These constructs are compared to international passenger vehicle registration data come from the Cross-country Historical Adoption of Technology (CHAT) database compiled by Comin and Hobijn (2009).[5] Because the CHAT registration data are available for only a handful of countries during the time span of our study, the role of these data in the analysis is limited to cross-validation of our estimates of automobile stocks.

Export prices are not recorded in the FCNUS, they are computed by taking the ratio of the value of exports to a particular destination and dividing it by the number of units exported to that destination. These are literally average prices by destination country and year pairs. In the trade literature these average prices are called 'export unit values,' which is the phrase we use throughout this paper. Aside from the level of detail they provide about the international price distribution of automobiles, important for our purposes is that they are not index numbers since our goal is to relate price differences in levels to differences in the volume purchased.

As our focus is on the role of relative prices is determining foreign automobile adoption compared to that of the U.S. it is crucial to understand the valuation method in these export data. As stated on page v. of FCNUS (1928): "*Articles of domestic production when exported shall be valued at their actual cost or the values which they may truly bear at the time of exportation in the ports of the United States from which they are exported. In contrast, foreign exports which are goods not changed in condition and previously imported are recorded when exported in terms of*

[5]CHAT is an unbalanced panel dataset with information on the adoption of over 100 technologies in more than 150 countries since 1800. The data is available for downloading at: http://www.nber.org/data/chat.

their import value." Thus our export unit values are free-on-board prices.

Our approach follows the recent macroeconomics and trade literature in which Euler equations are used with a set of observable variables to back-out wedges that make the Euler equations hold exactly over time (and here across nations). The goal, then, is an empirical deconstruction of a panel of 23 nation-specific wedges over the period 1913 to 1940 into a set of observable frictions (or proxies for them). The frictions include: 1) markups at the dock, 2) tariffs, 3) trade costs (freight and insurance), 4) distribution costs at the destination and 5) nominal price frictions. The data sources and methods used to construct these underlying wedges are discussed as they are introduced into the analysis.

Finally, a number of macroeconomic variables feature in the analysis. This includes historical estimates of real GDP intended for international comparisons. This is supplemented by an extensive international cross-section of macroeconomic, trade and exchange rate data used in Madsen (2001).

2.4 **The Model**

The international trade literature has made extensive use of the Armington assumption, the notion that domestic and foreign produced goods are differentiated by country of origin. In part, to have a point of contact with this literature and the vast empirical literature that estimates demand elasticities using trade flow data, transportation services are modeled in an analogous fashion. The consumer's choice is between an old and a new vintage product that each provide personal transportation services.

Conceptually, think of the old vintage of transportation services as a horse-powered carriage and the new vintage as an automobile powered by a combustion engine.[6] From a practical perspective, each vintage should be viewed as a composite of product varieties where horse-drawn carriages come in varieties produced by companies like Wilson and Studebaker and the combus-

[6]Steam engines pre-date the combustion engine and electric engines were also introduced, but both have trivial market shares in our period of study.

tion engine vehicle product lines are rolled out by Ford, GM and Chrysler. The data demands of such an investigation are well beyond the scope of this paper and unlikely to alter the main thrust of our narrative about the international diffusion of U.S. passenger automobiles. What is key for the narrative is that U.S. manufacturers produced the lion's share of automobiles in the world market and U.S. factory gate and export prices drive the marginal conditions at home and abroad leading to rising levels of global automobile adoption. Crucially, allowance is made for various wedges in accounting for international price differences across destination markets and which produce different levels of adoption.

Turning to specifics, the choice between the two modes of transportation in destination j is governed by a standard Armington aggregator with elasticity of substitution denoted by σ:

$$G_j(A_{j,t}, H_{j,t}) = [\theta_j^{\frac{1}{\sigma}} (A_{j,t})^{\frac{\sigma-1}{\sigma}} + (1-\theta_j)^{\frac{1}{\sigma}} (H_{j,t})^{\frac{\sigma-1}{\sigma}}]^{\frac{\sigma}{\sigma-1}} .$$

The standard approach to modeling the utility flow of durable goods is to treat them as proportional the existing outstanding stock. Under this interpretation, the objects in the aggregator are the outstanding stocks of automobiles ($A_{j,t}$) and horses and buggies ($H_{j,t}$).

Combined with the assumption of costless stock adjustment, the first-order conditions for the choices of automobiles and horse and buggies are, respectively:

$$\begin{aligned} D_1 G_j(A_{j,t}, H_{j,t}) &= G_{j,t}^{\frac{1}{\sigma}} \theta_j^{\frac{1}{\sigma}} (A_{j,t})^{-\frac{1}{\sigma}} = P_{j,t}^A \\ D_2 G_j(A_{j,t}, H_{j,t}) &= G_{j,t}^{\frac{1}{\sigma}} (1-\theta_j)^{\frac{1}{\sigma}} (H_{j,t})^{-\frac{1}{\sigma}} = P_{j,t}^H \quad , \end{aligned}$$

where $P_{j,t}^A$ and $P_{j,t}^H$ are the prices of automobiles and horses and buggies relative to a numeraire in destination country j.

Taking the ratio of the two gives the familiar expression for the relative quantity demanded as a function of their relative price:

$$\frac{A_{j,t}}{H_{j,t}} = \frac{\theta_j}{(1-\theta_j)} \left\{ \frac{P^H_{j,t}}{P^A_{j,t}} \right\}^{\sigma} .$$

Typically index numbers and log first-differences are used to estimate the elasticity. In the context of diffusion however it is important to estimate the point at which the transportation services of the automobile become competitive with those of the horse and buggy. In practice this requires a normed value of the service flow to relate the two vintages and thus make the absolute relative price meaningful. The approach taken here is to use the inflection point in the diffusion curve as the point at which the quality-adjusted relative price equals unity (effectively also setting taste biases across vintages to 0, so $\theta_j = 0.5$). This is elaborated next in the context of product diffusion curves.

2.4.1 Theoretical Diffusion Curve

The Euler equation for the two vintages of products may be conveniently transformed into a diffusion curve bounding the fraction of transportation services coming from the stock of automobiles between 0 and 1:

$$\begin{aligned} \omega_{j,t} &= \frac{A_{j,t}}{H_{j,t}+A_{j,t}} = \frac{\frac{A_{j,t}}{H_{j,t}}}{1+\frac{A_{j,t}}{H_{j,t}}} \\ &= \frac{\frac{\theta_j}{(1-\theta_j)} \left\{ \frac{P^H_{j,t}}{P^A_{j,t}} \right\}^{\sigma}}{1+\frac{\theta_j}{(1-\theta_j)} \left\{ \frac{P^H_{j,t}}{P^A_{j,t}} \right\}^{\sigma}} . \end{aligned}$$

A more readable version is obtained by dividing through the numerator and defining the relative prices as $RP_{j,t} = P^A_{j,t}/P^H_{j,t}$ and the home bias term as $\Psi_j = (1-\theta_j)/\theta_j$, we have:

$$\omega_{j,t} = \frac{1}{1+\Psi_j \left(RP_{j,t}\right)^{\sigma}} .$$

Here the limit points of diffusion are theoretical asymptotes which are reached only in the limit as relative prices move from 0 to infinity. In practice, it is uncertain how much historical data is needed to trace out the entire diffusion curve. The point to emphasize in applications is the value of having panel data. Given that the pass-through of relative prices is incomplete and that there may be distortions (tariffs and transportation costs) across locations the diffusion curve will be evident both in a cross-section at a point in time and in a single country over time.

To see this clearly, **Figure A.10** provides a simple illustration by simulating the diffusion curves by country using the export-unit values deflated by the U.S. CPI as the inputs and setting the elasticity equal to 9. Each frame is a slice through the cross-section in a particular year (1913, 1920, 1930 and 1940). Each dot in the graphic is an individual country's position on the diffusion curve (between 0 and 1). For readability the x-axis is transformed so that a fraction indicates automobiles are relatively expensive. Notice that in 1913, most countries are predicted to be in the very early stages of diffusion where the horse and buggy is very economical (conversely, the prices of automobiles are high). So, while there is considerable international dispersion in export unit values (see the price density on the x-axis), the quantities are tightly clustered at a floor close to zero (see the diffusion rate density on the y-axis). As the time window moves forward there is sufficient sustained relative price dispersion that the cross-sectional distribution in 1940 traces out a complete diffusion curve!

2.4.2 **Theoretical Wedge**

The Euler equation may also be written in a fashion that is more amenable to the analysis of various trade and macroeconomic frictions. Our focus will be on observables of which taste bias is not one. Accordingly, we set $\Psi_j = 1$ and the workhorse equation becomes:

$$\omega_{j,t} = \frac{1}{1 + \big((1+\tau_{j,t})RP_{u,t}\big)^{\sigma}} \,.$$

Notice the replacement of the destination price, $RP_{j,t}$ with a time-varying wedge, $\tau_{j,t}$, relative to the U.S. wholesale price, $RP_{u,t}$.[7]

Finally, taking this equation for the U.S. and nation j, allows a solution for the wedge needed to account for the ratio of adoption between the two:

$$(1+\tau^*_{j,t}) = \left(\frac{((1+RP^{\sigma}_{u,t})(\frac{\omega_{u,t}}{\omega_{j,t}}) - 1)}{RP^{\sigma}_{u,t}} \right)^{\frac{1}{\sigma}} .$$

The first question to ask is: How large a friction is needed to account for the consistent 10-fold difference between U.S. and foreign diffusion (**Figure A.9**). This involves feeding the ratio of adoption in the U.S. and aggregate (ROW), $\omega_{u,t}/\omega_{f,t}$, and the U.S. relative price, $RP_{u,t}$ into the Euler equation to generate the implied ROW wedge, $(1+\tau^*_{f,t})$. The relevant relative price series is the automobile wholesale price relative to the U.S. CPI (the blue line in **Figure A.8**).

Figure A.11 shows the wedges implied by model for elasticities of substitution equal to 4, 6, 9 (green, blue and red lines) which are chosen to match the range of estimates in the trade literature. The black line is the ratio of export unit values to U.S. domestic prices of automobiles (pooling all destinations). What the figure is telling us is that either these conventional trade elasticities are too low for the automobile market or there are additional frictions beyond markups at the dock. Put differently, if the true elasticity of substitution is 4 we would need a time-varying friction equal to the difference between the green and black line to explain the entire history of the U.S. and ROW quantity data. Focusing on the later part of the sample (when the markup of export unit values to U.S. domestic prices is close to zero), a constant trade-weighted ad-valorem-equivalent tariff of about 100% would square the behavior of relative price and quantities internationally. The wedge falls to about 60% and 40% as the substitution elasticity rises from 6 to 9.

[7] We are holding the price of horses fixed in this exercise.

2.5 **Estimating Stocks**

The discussion of **Figure A.11** emphasizes the importance of international price wedges in helping to account for the large gap in automobile diffusion between the U.S. and an aggregate of foreign countries. In order to make further progress in our understanding of what these wedges are, it is productive to move to a cross-country level analysis. This section discusses methods of estimating national stocks of automobiles from our novel archival data panel of U.S. automobile exports. We also utilize parametric time series models to characterize quantity dynamics. The next section conducts similar analysis for the panel of automobile prices.

Most economic models assume the utility flow from durable goods is proportional to the outstanding stock. The starting point therefore is an estimate of the outstanding stock of automobiles at each point in time and in each location. As with the construction of any capital stock, there are two common methods of estimation. One entails taking a census of the outstanding stock (analogous to a population census), the alternative cumulates the flow of new purchases (investment) and depreciates the pre-existing stock by a fixed proportion. Given data availability it is necessary to employ both methods. In the international context, our stock-flow estimates using novel archival data will provide the lion's share of cross-country estimates. For countries and time periods where registrations and our constructed stocks overlap they will be compared to one another as a form of empirical cross-validation.

Before turning to the results, it is useful to consider the potential advantages and disadvantages of the two estimation approaches. The use of registration data is the most direct method as it is the closest to a full census count of automobiles in use. And yet the potential for significant estimation bias is real. The available registration data does not record the year the automobile was produced. This leads to an upwardly biased estimate of the effective units of transportation services embodied in the stock because there is no account made of depreciation. Consider a newly purchased vehicle, it receives the same weight in the registration data as vehicle that is five years old. At a depreciation rate of 0.2, the five-year old vehicle should receive a weight of 0.6. This problem is exacerbated in the context of a newly diffusing durable good, such as the automobile, because the average age is rising fast in the initial period of the diffusion before settling into a steady-state. Registration data may also lead to downward bias in actual stocks because of lags in the introduction of registration requirements or lax enforcement of existing ones. In the U.S., automobile production starts in the late 1800's, but many U.S. states do not introduce registration as a *legal requirement* until the 1930s. The timing of product entry and registration was close in California, the first state to introduce registration requirements. It did so in 1901. While we know less about the timing of the introduction of registration requirements and their enforcement in other nations, the expectation is even less uniformity in practice internationally relative to across U.S. states.

Flow data in the form of automobile sales are available for only a handful of nations, most of whom are also producers of automobiles. In order to achieve something close to an international census of the stock of automobiles by country, some creativity is called for. This is where the U.S. export quantity data gains currency, provided the following three assumptions hold to a reasonable approximation.

Assumption 1: Initial conditions. When registration data is lacking, the initial condition for the stock of automobiles in country j, $A_{j,0}$ is set to zero in 1913 (the first year of our study). With the exception of the United States and a handful of advanced countries that produce automobiles, this seems to be a reasonable initial condition. Moreover, the fact that we are studying diffusion of a new product works in our favor as the impact of the initial estimation error of the stock will have a very small impact as the diffusion gets underway (due to the depreciation rate).

Assumption 2: Domestic production. When no domestic production data is found, it is set to zero. For most nations of the world (there are over 80 destination markets in the full panel) this is literally true. Even in the case of the advanced industrialized countries, with a few exceptions foreign production levels paled in comparison to the United States. In 1928, world production is estimated to be 5,273,941 of which the United States produced 4,359,759 vehicles (82.6%), compared to 242,382 in the next largest single producer, Canada (4.6%).[8] The largest producing nations in Europe (U.K, France, Germany and Italy) account for an additional 629,400 (11.9%) of world production. Nine other European nations produced the remaining 1%. The dominance of the U.S. industry in production and trade is truly staggering.

Assumption 3: Re-exports. For the export flow to a destination to add to the stock of automobiles consumed in that destination, requires that the automobiles not be re-exported to a third country. We have no information on the extent of re-exports and there is an argument to be made that they become important as the global tariff war creates arbitrage opportunities to cross-border trade (smuggling).

Turning to the details, estimation of automobile stocks using the stock-flow method is given by the following equation:

$$A_{jt} = (1-\delta)A_{jt-1} + X_{jt} + Y_{jt}$$

where A_{jt} is the stock of automobiles in country j, year t, and X_{jt} are the additions to the stock via imports from the U.S., Y_{jt} is domestic production and δ is the depreciation rate (set to 0.2). The reader should keep in mind that there is no domestic production in most international destinations (i.e., $Y_{jt} = 0$ for most j).

Figure A.12 presents estimates of the stock of automobiles per 1,000 population for Canada, the UK, France and Italy. The first observation to make is that while the volume of automobile production by these countries was dramatically lower than that of the United States, it was sufficient to supply most of domestic demand. This fact is evident in the enormous gulf between the black dashed lines and the red dashed lines, which represents the contribution of domestic production to the evolution of the stock. The reader is cautioned to the fact we lack production data before 1922. Comparing the registration-based estimate to the stock-flow estimate, the correspondence is close. This is a reassuring cross-validation of measures given the concerns about measurement

[8]Note that foreign subsidiaries of U.S. companies operated in Canada and these are included in the Canadian production numbers.

error raised earlier. It also bodes well for reliance on export data to estimate foreign stocks in the larger sample where registration data is unavailable. Finally, it is worth noting that the notion registrations may under or over estimate the stock is not just a theoretical possibility, it is consistent with the fact that the stock-flow estimates (black dashed lines) are sometimes above and sometimes below the registration-based estimates (blue solid lines).

The utility of our US export archive to fill gaps in the data when automobile registration data is unavailable and production limited or non-existent also has support. By way of example, **Figure A.13** plots the registration and the US export-based estimates of the stock of automobiles in the Philippine Islands. The two series track each other very closely with the exception of the 1930s.[9]

2.6 Reduced Form Quantity Dynamics

An established empirical literature characterizes diffusion of products and technology using a logistic function of the following form:

$$A_{jt} = \frac{\alpha_j}{1+\exp^{-(\beta_j t-\tau_j)}} \ .$$

The index j will denote states within the U.S. or nations of the world and t denotes the year. Two properties of this function are important to keep in mind.

Property 1: $\lim_{t\to\infty} A_{jt} = \alpha_j$; thus the parameter α_j is the long-run level of adoption of the automobile.

Property 2: $A_{t_j^*} = \alpha_j/2$ with $t_j^* = \tau_j/\beta_j$ is the point in time at which the stock of automobiles is halfway to the long-run diffusion level. This point in history is also the inflection point of the logistic curve (turning from convex to concave). Note that given our sample starts in 1913, the calendar year of the inflection point is $1913+t_j^*$.

[9] In practice, it is difficult single out causes for the divergence for the reasons described in the methodology section. An aging fleet of cars later in the sample could account for the divergence since registrations fail to account for depreciation. However, Philippine production or re-exports from a third country could also account for the difference.

2.6.1 Diffusion as Measured by the ROW Aggregate

Our analysis begins with logistic estimates at the aggregate level for foreign countries comparing the CHAT registration data and our constructed stocks using the stock-flow equation (see **Figure A.14** for the two data series and their respective estimated diffusion curves). **Table A.16** reports estimates of the empirical diffusion curve. The logistic function fits the stock of automobiles very well and the estimated parameters are comparable across the two alternative estimates of the foreign automobile stock. The long-run adoption level is predicted to be 14.5 automobiles per capita using the registration data and 16.9 automobiles per capita using our estimates. These are in the ballpark of the values reached by the raw data (**Figure A.14),** suggesting that most foreign markets were near saturation levels of demand despite their low values compared to the United States. Notice also, that while the phase shift parameter and speed of adoption are different across the two measures, the point at which the diffusion process reaches the mid-point to the asymptotic steady-state is estimated to be 1927 in both cases.[10] To place the aggregate foreign diffusion in perspective, the estimated inflection point for the aggregate U.S. data is 1922. So the diffusion of automobiles in the United States is dramatically higher than in foreign countries and occurs 5 years earlier.

2.6.2 Diffusion by Country

Since diffusion curves are highly non-linear, there are reasons to expect the aggregate estimates to be misleading of microeconomic decisions that underlie them. To explore this idea the logistic curves are estimated individually for each of our benchmark sample of 23 foreign countries. Here we use our estimates of the stocks.

Beginning with the estimated long-run diffusion level ($\widehat{\alpha}_j$), the median estimate across the 23 nations is 4.5, less than one-third of the aggregate estimate. In contrast, the inflection year is not sensitive to aggregation: the median estimate across nations is 1926, just one year earlier than esti-

[10] These values are computed as $1913 + \widehat{t}^* = 1913 + \widehat{\tau}/\widehat{\beta}$. Using the estimate using registration is 1913+6.08/0.425=1927.3 and from the production and export flow data is 1913+4.49/0.316=1927.2.

mated using the aggregated data. The reason for the poor aggregation properties of the automobile stock data is a combination of skewness of the world population distribution and heterogeneity of diffusion.[11]

Having developed some cross-sectional facts about quantity dynamics and long-run steady-states, we turn now to the behavior of export unit values.

2.7 **Reduced Form Price Dynamics**

Recall that the original source data (FCNUS) reports both the value of exports and the number of passenger vehicles exported to each destination market. The former were used to construct the international automobile stocks by country in the previous section. This section studies the export unit values by destination so that the quantities and prices may be related at a more granular level than in the introductory section.

Toward this end, the export unit values in USD terms are defined as the US dollar value of exports of passenger vehicles to country j ($V^A_{j,t}$) divided by the number of passenger vehicles exported to that destination, ($X^A_{j,t}$):

$$EUV^A_{j,t} = \frac{V^A_{j,t}}{X^A_{j,t}} \,.$$

To convert these into relative prices, we divide them by the U.S. CPI index:

$$REUV^A_{j,t} = \frac{EUV^A_{j,t}}{P_{u,t}}.$$

As a starting point, it is essential to understand the cross-sectional and time series behavior of our export unit values normalized by the U.S. numeraire (CPI). **Figure A.15** plots the median and first and third quartile of the cross-destination price distribution of these relative prices. The most interesting feature of the relative price distribution is that relative prices start high and finish low.

From a theoretical perspective, one would like to know how well a country-specific markup

[11] Below we also explore the possibility that more populous countries have higher adoption rates per capita which would skew the aggregate toward high population adoption levels.

model fits the international price distribution. This motivates the following non-linear functional form which is estimated country-by-country:

$$REUV^{A}_{j,t} = (\overline{RP}\exp(-\delta_j t) + \underline{RP})\mu_j + \varepsilon_{j,t} \ .$$

Notice that as the time index moves from the zero to infinity, the relative price is predicted to move from $(\overline{RP} + \underline{RP})\mu_j$ down to $\underline{RP}\mu_j$. Provided the slope parameter is common across j, $(\delta_j = \delta)$, the parameter μ_j, is interpreted as a constant, country-specific markup over the U.S. source price. The relative price falls at rate δ_j between these extremes up to an additive error term.

The upper and lower bounds on the price path from the pooled data are estimated to be \$1,500 and \$500. As evident in **Table A.19**, most of the slope parameters (δ_j) are between 0.1 and 0.3. The estimated markups vary from a trivial 1% in Australia, Norway, Peru Sweden and the U.K., to more than 50% in Austria, Columbia, Finland, France, and Spain.

2.8 **Results**

The previous two sections described the cross-section and time series properties of the stock of automobiles by destination market and U.S. dollar export unit values deflated by the U.S. CPI. These international relative prices would be the correct ones to relate to the quantity of demand in the destination if the following two assumptions held:

Assumption 1: The Law-of-One-Price is satisfied for automobiles at the retail level. Specifically, consumers in the United States and destination country j must face the same common currency price for automobiles.

Assumption 2: Purchasing Power Parity holds between the U.S. and each destination. Specifically, the absolute version of the proposition is that the U.S. and foreign consumption baskets, after conversion to a common currency, cost the same amount.

Notice that the combination of the two assumption assures that the relative price of automobiles (relative to the domestic consumption basket) are equal across nations. As Crucini et al. (2005) show using micro-price data across European countries from 1975 to 1990, LOP holds to a very close approximation for a subset of goods even for country pairs in which PPP fails and, conversely, PPP holds for some country pairs even though LOP is violated for most individual goods. Less is known about LOP and PPP during the interwar period.

To fill this gap in the context of the automobile, we proceed in two steps. First, we back out wedges between U.S. and foreign relative prices using the Euler equation used in the theoretical section. These theoretical wedges are then deconstructed into a number of observable frictions. We turn to these details next.

2.8.1 **Nation-Specific Theoretical Wedge**

Recall that the theoretical wedge, $(1+\tau^*_{j,t})$, is back-solved from the Euler equation using U.S. and destination automobile stocks (normalized by U.S. and foreign real income) and the U.S. relative price of automobiles, $RP_{u,t}$:

$$(1+\tau^*_{j,t}) = \left(\frac{((1+RP^{\sigma}_{u,t})(\frac{\widetilde{\omega}_{u,t}}{\widetilde{\omega}_{j,t}})-1)}{RP^{\sigma}_{u,t}} \right)^{\frac{1}{\sigma}} .$$

where $\widetilde{\omega}_{j,t} = A_{j,t}/Y_{j,t}$ is the automobile stock normalized by real per capita income in country j.

The theoretical wedge is the friction between foreign destination and U.S. relative price:

$$RP^{A}_{j,t} = (1+\tau^*_{j,t})RP_{u,t} .$$

The problem is that destination retail prices are not observed, we have only the USD export unit values. The approach taken is to augment the export unit values with three additional trade frictions that have been the focus of the contemporary research and undertake an analysis of how well they account for the unobserved wedge, $(1+\tau^*_{j,t})$ and thus the quantity data.

Toward this end, consider the following identity that relates the observed U.S. relative price of the automobile ($RP_{u,t}$) to the conjectured destination relative price ($RP^A_{j,t}$) of the automobile:

$$RP^A_{j,t} = (\frac{P^A_{j,t}}{P^A_{u,t}})(1+\tau_{j,s})(1+t_j)(\frac{S_{j,t}P_{u,t}}{P_{j,t}})RP_{u,t}\ .$$

The retail price differences are attributed uniquely to four trade frictions. The first friction is the markup of the export over the domestic price, $(P^A_{j,t}/P^A_{u,t})$. This markup is a standard feature of trade models that feature forms of imperfect competition. In these settings, the producer has the ability to segment markets and engage in third-degree price discrimination (pricing-to-market). The second friction is an official barrier to trade, the ad-valorem-equivalent tariff on automobile imports, $(1+\tau_{j,s})$. In principle tariff levels vary across countries and time. Sources of time variation are the relatively infrequent legislative changes (thus the index by s, rather than t).[12] The third friction lies at the center of the trade and gravity model, the trade cost. Typically this margin is estimated by regressing bilateral trade data on distance (a proxy for the cost) and other controls. The fourth friction is a macroeconomic friction, the aggregate real exchange rate (i.e., deviations from unity are PPP deviations).

2.8.2 Wedge Accounting

Combining the previous two equations allows a mapping between the theoretical wedge and its observable components plus an unobserved residual wedge, $(1+\varepsilon_{jt})$:

$$(1+\tau^*_{j,t}) = (\frac{P^A_{j,t}}{P^A_{u,t}})(1+\tau_{j,s})(1+t_j)(\frac{S_{j,t}P_{u,t}}{P_{j,t}})(1+\varepsilon_{jt}).$$

Before turning to the variance decomposition, we discuss the historical context, particularly as it relates to these frictions. As we have already described the behavior of export unit values (the first

[12]Some interwar duties were specific, rather than ad-valorem, charging a nominal local currency duty as a fraction of the import value. In this case, the ad-valorem–equivalent rate moves in the opposite direction of the local currency price. See Crucini (1994) and Bond et al. (2013) for microeconomic evidence relevant to the U.S. interwar tariff schedules. See Blattman et al. (2003) for the most comprehensive analysis of tariffs across countries and time (1870-1938) at the aggregate level.

term), the discussion next turns to the other three wedges.

2.8.3 Interwar Commercial Policy

The U.S. narrative begins with campaign promises of increased tariffs by Herbert Hoover in 1928 (the business cycle peak). Originally envisioned as measure to help agriculture after the post World War I decline in the relative price of agricultural goods, the tariff revisions quickly extended to virtually all sectors of the economy (Irwin and Kroszner, 1996). Most of the available tariff history relies on the ratio of customs duties to dutiable imports as a proxy for protectionism. Aside from the obvious problem of substitution bias, there is no reason to expect the duty on automobiles to be close to this aggregative measure. The most comprehensive analysis of microeconomic tariff distortions in the United States is Bond et al. (2013) which studies more than 5,000 tariff-line items. The mean ad-valorem-equivalent tariff rose from 32% to 46% after the passage of Hawley-Smoot in June 1930. The tariff levels ranged from 0% to over 200%. Specific to automobiles, U.S. import duties on automobiles were actually cut from 25% to 10% (though duties on automobile parts remained constant at 25%).

In the 1920s, many countries had much higher duties on automobiles than did the United States and raised them further in response to higher U.S. duties on their exports to the United States. We have two cross-sections spanning 23 countries. The median ad-valorem-equivalent tariff rate across these countries increased from 17% to 34% in a single year (from 1920 to 1921). Average across the two years, the cross-country average tariff was 25%. For eleven of these countries the median tariff rose to 48% by 1937.[13] **Table A.20** reports country level ad-valorem-equivalent tariff rates for all 23 countries in 1920, 1921 and the average of these two years. The calibration results below use the average tariff across 1920 and 1921.

[13]One advantage of the 1937 study is the breadth of scope of tariffs relevant to the automobile and automobile industry. Besides duties on motor vehicles, the 1937 cross-section includes duties on steel sheets, crude rubber, tires, window glass, gasoline and crude petroleum. This cross-section allows are more complete picture of both the cost of inputs into production of automobiles (e.g., steel sheets, window glass, crude rubber) and its user costs (e.g., gasoline and tires).

2.8.4 **Trade Costs**

One of the most robust empirical relationships in the trade literature is the fact the bilateral trade between nations is declining in the distance between them. This 'gravity' model of trade also has found that the volume of bilateral trade is roughly proportional to the product of output. As our emphasis is on the stock of automobiles with trade facilitating the accumulation, the focus in this section is the role of distance.

Trade costs are estimates in two steps. First, a standard gravity equation is estimated by regressing the logarithm of the ratio $\widetilde{\omega}_{j,t}/\widetilde{\omega}_{u,t}$ (the inverse of the ratio of our Euler equation) on a constant and the logarithm of distance (between capital cities):

$$z_{jt} = \ln \widetilde{\omega}_{j,t} - \ln \widetilde{\omega}_{u,t} = \underset{(0.73)}{0.52} - \underset{(0.08)}{0.44} \ln(d_j) + \varepsilon_{j,t} \ .$$

Note that all of the time series variation is attributed to the residual as income effects are assumed to be proportional to the stock and distance is time invariant. The coefficient on distance is -0.44, which is remarkably close to the average reported in the meta analysis of the gravity literature (Disdier and Head, 2008).

As is well-known from the theoretical gravity literature, the reduced form coefficient on distance is the product of a substitution elasticity and the slope coefficient linking distance to trade costs (typically: $\ln(1+\tau) = \beta \ln d_j$). In principle it is possible to disentangle these two margins, by substituting the predicted values of the gravity equation for quantities, $\widehat{\omega}_{u,t}/\widehat{\omega}_{j,t}$ =exp$(-\widehat{z}_{jt}) =$ $\exp(-\widehat{\alpha} - \widehat{\theta} \ln(d_j))$ into the Euler equation with the U.S. domestic relative price set to unity, to generate a cross-section of trade cost wedges.

$$(1+\widehat{\tau}_{j,t}) = \left(2(\frac{\omega_{u,t}}{\omega_{j,t}}) - 1\right)^{\frac{1}{\sigma}} \ .$$

While this gives a natural ranking of trade cost wedges with Canada an outlier at the low end given its proximity to the United States and, not surprisingly Australia and New Zealand at the top end, the absolute size of the wedge is implausibly large (over 50% ad-valorem-equivalent for a plausible elasticity). Direct estimates of freight costs are available from the seminal work of Hummels (2001). While his work uses contemporary data, the ad-valorem-equivalent levels are more plausible, in the neighborhood of 15%. Our calibration uses this common trade cost.[14]

2.8.5 **Exchange Rate Arrangements**

The international monetary standard in place during our period of study was the gold standard, though strict adherence was the exception rather than the rule, at least for the 23 countries in our current sample. Eichengreen (1992) is one of the few narratives that link the operation of the gold standard with trade and macroeconomics during the international Great Depression. He argued that countries that devalued in the 1930s exited the slump more quickly than those that did not. One limitation of his thesis is that neither he nor any other researcher (as far as we know) has conducted a systematic investigation of the adjustment of the nominal prices of traded goods to nominal exchange rate changes during this period of history. Unfortunately, at this stage our data is also too limited to fill this important gap in the literature.

Instead, we focus on long-run deviations from absolute purchasing power parity based upon the Penn effect. The Penn effect here is microeconomic: calibrated to match the relationships between LOP and PPP deviations and relative wages document by Crucini and Yilmazkuday (2014). The slope of the price level with respect to wages of unskilled labor is estimated to be about 0.5. Since automobiles contain about 20% distribution costs, the slope effect of the price level in the relative price of automobiles under producer currency pricing and long-run pass-through should be calibrated to 0.3. Effectively this means that a country with half the U.S. level of per capita income will find U.S. automobiles 15% (0.3 x 0.5) more expensive when faced with the same common currency price.

[14] Future work will combine the Hummels estimates with the regression above to allow for bilateral pair differences in trade costs. However, given the geography of the cross-section, only Canada is likely to be materially affected as the other countries are a comparable distance from the U.S.

2.8.6 Cross-sectional versus Time Series Variation

The theoretical wedge is a high-dimensional object: it varies both across nations and over time. Before launching into our deconstruction of the wedge into its microeconomic parts, it is productive to carefully describe these underlying dimensions of variance in the panel. To accomplish this, we use the variance decomposition employed by Crucini and Telmer (2012) in the context of LOP deviations.

$$
\begin{aligned}
var(\ln(1+\tau^*_{j,t})) &= var_j[E_t(\ln(1+\tau^*_{j,t}))] + E_j[var_t(\ln(1+\tau^*_{j,t}))] \\
V &= C + T
\end{aligned}
$$

In words: the total variance of the logarithm of the theoretical wedge in the panel (V) may be decomposed into the cross-sectional variance (across nations) of the conditional mean (the time averaged wedges), denoted by C, and the mean (average across countries) of the conditional variance (time series variance by country), denoted by T.

Beyond its descriptive value, the decomposition is helpful in sorting various theoretical channels, some of which emphasize time-invariant wedges and other which emphasize time varying wedges. For example, it seems reasonable that tariffs and trade costs (distance) will manifest themselves mainly in the long-run mean of the wedges and thus influence the long-run level of automobile adoption. Equally plausible is that devaluations, while potentially large in size, will have persistent but transitory impacts on relative prices and consequently transient impacts on automobile demands. In other words, contribute mainly to the time series component. The standard deviation of the theoretical wedge is 14%, 73% of this is associated with the long-run or cross-sectional component.

2.8.7 Comparison of Theoretical and Empirical Wedges

Figure A.16 shows the distribution of nation-specific theoretical wedges implied by the Euler equation (and the observed automobile stocks). These are time-averages of the annual nation-specific wedges. Our focus on the time-averaged wedges is due to the fact that this component accounted for 73% of the total variance. Countries are ranked from the lowest theoretical wedge to the highest. Recall that our model assumes there are no taste biases or elasticity differences across nations so the ranking of theoretical wedges is the inverse of the ranking of automobile stocks. Canada ranks first with the lowest theoretical wedge because it has a very high adoption rate relative to the other nations in the cross-section, though still about 0.5 the level in the U.S.

There are three theoretical wedge lines in the figure, one for each calibrated elasticity: 4, 6 and 9. The empirical wedge is the black line. The cross-sectional distribution of the empirical wedges falls mostly within this range of elasticities. The elasticity of 4 is calibrated to produce a reasonable match of both the average wedge across countries and the variation of wedges across countries. Coincidentally, this is almost exactly equal to the value of the automobile import demand elasticity estimated by Hummels (2001) using contemporary data.

Notice that the cross-sectional range of wedges is higher the lower is the trade elasticity. This makes intuitive sense: if consumers have a low elasticity of demand, a larger range of wedges is needed to account for a given amount of variance of adoption across countries. Early we saw this property manifest in the time series of the aggregate ROW wedges since the time series variation of the wedge is also higher the lower is the trade elasticity. This latter point was first emphasized by Backus et al. (1992), using an Euler equation similar to that applied here, but applied to aggregate demand rather than an individual product.

Recall that the analysis here focuses on the time-averaged wedges for each country. That is, the Euler equation is used to simulate a set of wedges by country and time period using the benchmark elasticity of 4. The theoretical wedges are then averaged over the sample period 1913 to 1940 and summarized in **Table A.21**. The cross-country average theoretical wedge is close to an implied 100% ad-valorem-equivalent duty (0.97). Our empirical wedge (including all the micro-frictions) averages 0.82. As indicated in the second row of the table, the fraction of the theoretical wedge accounted for by our measured frictions is 0.85, (0.82/0.97) and 0.15 is unaccounted for (the residual wedge). The largest fraction of the empirical wedge is the Penn Effect at 0.36, next is the tariff at 0.26 and the markup at the dock and trade costs each accounts for about 0.20.

The second panel of number is a decomposition of the variance across countries. That is, it computes the covariance between the country-specific values of the theoretical or aggregate empirical wedge and the components that make up these wedges. The empirical wedge accounts for somewhat less than half the cross-section variance of the theoretical wedge (0.43). The cross-sectional variance in the empirical wedge is mostly due to the Penn Effect and the markup; they each contribute about 0.40 to the total. That said, tariff variation is a substantial contributor at 0.19. One caveat in the cross-sectional results is that the trade costs are presently assumed to be common across countries. While this is likely a reasonable approximation for most countries except Canada (which is much closer to the U.S. than other countries in the sample), in future work we plan to allow for these differences.

2.9 Conclusion

Using a newly constructed U.S. panel of automobile export quantities and prices, this paper has documented the global diffusion of the automobile from its infancy at the turn of the 20th century to the eve of World War II. Despite starting at similar initial conditions at the turn of the 20th century, this diffusion, while generally rapid, was highly asymmetric across nations. To better understand these asymmetries, a simple two-vintage CES consumption aggregator is used to produce a logistic model of diffusion as a function of the relative price of the two vintages. The model is used to back-out wedges needed to reconcile quantities with the Euler equation linking relative quantities and relative prices (relative with the U.S. as the benchmark). The wedges were then deconstructed into frictions that have long been emphasized in the literature: markups, tariffs, trade costs, distribution costs and sticky prices.

We find that observed frictions are capable of accounting for both the cross-country average theoretical wedge and a considerable amount of country-specific variation around the average when the trade elasticity is calibrated to 4. This elasticity is consistent with the emerging empirical trade literature where elasticities estimates seem to be converging on a consensus in the range of 3-5.

The fact that the existing empirical literature does not explicitly measure trade frictions and yet matches our calibration results for automobiles when frictions are explicitly included is reassuring. However, this should not be taken to mean that measuring the exact trade frictions underlying the gravity model of trade is unnecessary. Quite the contrary, it is important to know what the frictions are in order to undertaken relevant counterfactual analysis and to anticipate the impact of trade liberalization (or rising protectionism). To consider one example relevant to the industry and historical context here, the removal of tariffs would generate some convergence in automobile adoption but would be far from a frictionless work due to the dominance of markups and distribution costs (and the unavoidable friction of distance, of course). In addition, there may be equilibrium interactions across the frictions that are important to understand such as the role reductions in tariffs and shipping costs might have on equilibrium markups.

CHAPTER 3

Time-Varying Impacts of Financial Credits on Firm Exports: Evidence from Export Deregulation in China

with Zhongzhong Hu and Yong Tan

3.1 Introduction

Both internal and external financial credits are of major importance for a firm's export decisions.[1] Entering export markets typically involves large start-up costs (Arkolakis, 2011; Aw et al., 2011; Dai and Yu, 2013; Chaney, 2016; Bai, et al., 2017), as firms need to collect and analyze information on foreign markets, adapt products and packaging to fit foreign preferences, learn local bureaucratic procedures for market access, set up distribution networks and advertize for marketing penetration. The start-up costs in international markets are economically significant, and hence financial credit is more important for direct exporters than indirect exporters and non-exporters. In this paper, we attempt to investigate different roles played by financial credits on exporters that switch their exporting mode from indirect to direct exporting.[2]

With the availability of micro firm-level data, a growing body of recent literature examines the link between financial credits and firm-level export performance (e.g., Campa and Shaver, 2002; Greenaway et al., 2007; Berman and Héricourt, 2010; Minetti and Zhu, 2011; Manova, 2013; Manova et al., 2015; Channey, 2016). Greenaway et al. (2007), for example, find that financial health has a trivial effect on firm-level export participation decision in the UK, while a firm's export participation decision can significantly improve this firm's financial health. Berman and Héricourt (2010), to the contrary, document that firm-level external and internal financial health enhances firms' export through the extensive margin, although their effect on the intensive margin

[1] Here, "financial credits" refers to the resources that a firm could rely on to finance for a broad range of economic activities, such as investment, working capital, and entry of international markets. The credits could be either internal, like firms' retained earnings, or external, like loans from outside creditors.

[2] Following Bai et al. (2017), we refer to exporting through intermediaries as indirect exporting mode.

is negligible. Minetti and Zhu (2011) find that financial rationing reduces firm-level exports on both extensive and intensive margins by employing data from the Italian manufacturing sector. The conflicting conclusions may be due to the heterogeneous influence of financial credits on different firms. Manova et al. (2015) show that financial constraints have a more pronounced impact on low-productivity firms, and firms belonging to financially vulnerable sectors. Jarreau and Poncet (2014) indicate that the export performance of foreign-owned firms and joint ventures relies more on their own financial credits than private domestic firms in China.

In this paper, we explore the heterogeneous influence of financial credits on firms that are engaged in indirect (exporting through intermediaries) and direct exporting. Further, we exploit the time-varying impact of financial credits on firms that switch from indirect to direct exporting due to changes in export deregulation during China's WTO accession. As discussed in Bai et al. (2017), indirect and direct exporters exhibit very different export structures, and productivity and demand evolution are more favorable under the direct exporting mode. Specifically, direct exporters, who engage in frequent contact with foreign buyers, have more opportunities to improve their productivity and demand stock (e.g. Egan and Mody, 1992).[3] This may suggest a more efficient utilization of financial credit among direct exporters.[4] As such, relative to indirect exporters, we expect a larger impact of financial credits on exporters that switch their exporting mode from indirect to direct exporting. Besides, Khandelwal et al. (2013) indicate that for textile and cloth sector, the gains from quota removal mainly arise from the elimination of quota misallocation rather than trade itself. If export licenses have also been misallocated before China's WTO accession, we expect a more pronounced impact of financial credits on switchers in the post-WTO accession period.[5]

[3] Egan and Mody (1992) demonstrate that a collaborative suppler-buyer relationship, on the one hand, improves exporters' learning-by-exporting efficiency; on the other hand, the buyers are less likely to change suppliers, which make investment in demand stock more effective.

[4] Facing more opportunities to improve export profitability, direct exporters have a higher incentive to invest in reputation building, consumer information collecting, foreign distribution system constructing, etc. This may suggest that direct exporters have a higher efficiency in finance utilization.

[5] If export licenses were misallocated before China's WTO accession, the switchers in the post-WTO accession period would exhibit higher export expanding potential as they are more productive but more financially constrained, and hence, they can better utilize financial credits to support their expanding in product scope, production capacity or R&D investment.

China offers an ideal setting to conduct this research in two respects. First, it relaxed regulation on firms' manner of trade, especially exporting modes to fulfill its WTO membership commitment during 2001-2004. More specifically, before China joined the WTO, small firms (mainly private domestic enterprises) with low registered capital (or sales, exporting values, etc.) had to rely on state-owned exporting intermediaries to export abroad (*indirect export*) due to the regulation on direct trading rights. When China became a member of the WTO, the accession clauses required that all firms should be permitted to export directly (*direct export*). We thus observe a rising share of small private domestic firms in the pool of all direct exporters in **Figure A.17**. Second, in China, severe export distortion and resource misallocation exist (Khandelwal et al., 2013; Hsieh and Klenow, 2009). A considerable share of high-productivity firms are prevented from direct exporting before China's WTO accession because of their small scale. Export licenses favored large and state-owned enterprises (SOEs) before China's WTO accession (see Khandelwal et al., 2013). As a result, the trade deregulation induced by China's WTO accession offers a quasi-natural experiment to examine the degree of export distortion, uncovering the time-varying impact of financial credits on direct exporters.[6]

Using a comprehensive data set of Chinese manufacturing firms, we find supporting evidence. First, by employing a difference-in-differences (DID) estimation approach, we find that a 10% increase in firm-level internal (resp. external) finance will on average lead to a 4.33% (resp. 2.93%) more increase in switchers' (treatment group) export values relative to indirect exporters (control group). Meanwhile, a 10% increase in the firm-level internal (resp. external) finance will on average improve the productivity of the switchers by 0.78% (resp. 0.66%) more relative to indirect exporters. Second, to examine the time-varying influence of financial credits on the export performance of switchers, we employ a difference-in-difference-in-differences (DDD) method. The results demonstrate that conditioning a firm switching from indirect to direct exporting, a 10% increase in the firm-level internal (resp. external) finance will on average lead to a 0.62% to 3.89%

[6]As our paper discusses the role of finance in the context of switching exporting modes (either indirect or direct exporters), we are excluded from talking about the extensive margin of exports (such as selection into exporting, product scope, number of destinations, and sales within each destination-product market). Thus, only the intensive margin of trade is investigated in this paper.

(resp. 2.61% to 8.60%) more increase in export values after China's WTO accession.[7] The main findings remain when we use instrumental variable methods to account for the potential endogeneity issues associated with firms' switching in exporting modes, as well as the reverse causality issues associated with financial credits.[8]

We further examine the channel through which financial credits manifest heterogeneous influences. The results show that direct exporters have a higher efficiency in finance utilization than indirect exporters.[9] This higher financial utilization efficiency for direct exporters offers economic intuitions for our DID and DDD results: on the one hand, financial credits have a more pronounced effect on export performance for switching firms (firms switching from indirect to direct exporting); on the other hand, less regulation during 2001-2004 further enhances financial utilization efficiency for direct exporters and hence financial credits manifest a time-varying impact on switched firms.

Our work is closely related to Manova (2013) and Manova et al. (2015), in which the authors find a significant impact of financial credits on firm-level export performance. The impact is more pronounced for less productive firms and firms that belong to more financially vulnerable sectors. Differing from Manova (2013) and Manova et al. (2015), we emphasize the heterogeneous influence of financial credits on firms that are engaged in different exporting modes. Our story is also in line with Bai et al. (2017), in which they examine how the exporting mode (direct or indirect exporting) affects firm-level export performance, and provide a theoretical foundation on the firm-level heterogeneous performance under different exporting modes. However, our work distinguishes itself from Bai et al. (2017) by paying particular attention to the time-varying impact

[7]Notice that financial credits do not exhibit increasing importance on switchers' productivity. This is partly because, in the post-WTO accession period, switchers are more productive (see **Figure A.17** in the Appendix for more information). Lileeva and Trefler (2010) find that productivity growth is declining in the firm-level initial productivity. As such, these later more productive switchers exhibit a slower productivity growth, which makes financial credits less important in boosting firm-level productivity increase.

[8]In particular, we instrument the switching in exporting mode variable with the product of the firm-level base-year productivity and one-period lagged province-level capital supply shock. In the meanwhile, we proxy the current value of firm-level finance by its first-order lagged value in all baseline estimations. We also follow Manova et al. (2015) constructing province-industry level financial credit ratios as a proxy for firm-level financial credits, all results remain.

[9]Switchers have a higher efficiency in finance usage in terms of a lower current liquidity ratio, higher inventory turnover ratio, and a shorter operation cycle.

of financial credits on switchers. The statistically significant time-varying impact, on the one hand, suggests export license misallocation among Chinese exporters before China's WTO accession; on the other hand, it implies an increasing role that an effective financial market plays in boosting exports of China.[10] The conclusions further relate our study to Khandelwal et al. (2013) and Klenow and Hsieh (2009), who both emphasize the resource misallocation and distortion in China, which are of nontrivial influence on welfare. Khandelwal et al. (2013), for instance, show that most gains from trade in China are through the alleviation of distortions. If the diminishing distortion is the underlying source that increases the importance of financial credits for direct exporters, gains from trade might have been underestimated.[11] Many existing models do not account for the effect of trade liberalization on eliminating distortions, which further increases the effectiveness of the financial markets.

The rest of the paper is organized as follows. Section 2 reviews the policy and institutional background, especially how the regulation on exporting modes evolved over the period 2001-2004 in China, which also inspires us to propose the hypotheses that we attempt to test in the study. Furthermore, we discuss how to construct the matched dataset and provide some summary statistics in Section 2. In Section 3, we describe the construction of key variables and empirical methodology utilized to conduct statistical inference. Section 4 presents baseline empirical results and robustness checks. Finally, we conclude in Section 5.

3.2 Policy Background and Data Description

We first present institutional background information on the policy change with regards to restrictions in firm direct exporting rights in China, which is the source of the time-varying effects of financial credits on firm exporting we focus on. It also inspires us to formally propose the two hypotheses that we want to test. We then describe the two data sets we employ and also explain the procedure by which we construct the matched sample that we use for the econometric analysis.

[10] According to Bai et al. (2017), if there is no distortion, a time-invariant impact of financial credits on direct exporters should be identified, since the cost structure difference between direct and indirect exporters are unchanged.

[11] Gains from trade are only 6% from Arkolakis et al. (2012) and slightly larger in Melitz and Redding (2015).

3.2.1 **Policy Background**

The policy change that we emphasize here is China's deregulation on firms' direct exporting rights. The international exporting market was highly regulated before China's WTO accession. In 1978, less than 20 specialized Foreign Trade Corporations and around 100 subsidiaries of these corporations dominated Chinese exports with government-issued monopoly trading rights. If a firm wanted to export abroad at that time, it could only go through these Foreign Trade Corporations that acted as exporting intermediaries. It means that only indirect exporting mode was allowed for a typical Chinese firm in that period.

China relaxed the restrictions on direct trading as the reform and opening up policy went into effect. All foreign-owned firms were granted direct exporting rights when the Foreign Trade Law was adopted in 1994. In 1998, the Chinese State Council approved the issuing of direct exporting rights to state-owned and private domestic firms over a threshold size in terms of registered capital or other criteria like sales, net assets, and prospective exporting values (after January 2001, only the registered capital remained as the criterion). Yet, the registered capital requirement was quite demanding in the beginning, around 8.5 million yuan (approximately 1.03 million dollars in 2001) for private domestic firms.

The restrictions on redirect trading were eliminated over the 2001-2004 period when China tried to fulfill WTO accession agreement, at different paces for various ownership types and locations.[12] For example, the registered capital requirements for private domestic firms to get direct exporting rights decreased from 8.5 million yuan to 5 million yuan in January 2001, and further reduced to 3 million yuan in July 2001.

After China entered the WTO in December 2001, the requirement dropped to 0.5 million yuan in September 2003, which in practice means there were almost no restrictions on firm exporting as those who want to export typically have a higher registered capital than 0.5 million yuan. Finally, starting from June 2004, the registered capital requirement fell to zero, and the restriction

[12] To have a more detailed perception of how the reform or policy change was accelerated over the period 2001-2004, please see Bai et al. (2017).

was fully removed. Though the registered capital requirement showed a dramatic drop over the 2001-2004 period for most of China, Special Economic Zones like *Shenzhen* and *Xiamen* were treated differently. To be specific, the registered capital requirement for Special Economic Zones stayed at a very low level of 2 million yuan ever since 1998, and dropped to 0.5 million yuan in September 2003. Given this difference, we rule out firms located in Special Economic Zones from our matched sample, as they were essentially unaffected by the trade deregulation, especially in the initial years.

It is worthy to mention that even though the restriction on direct exporting rights was eliminated then, there still exist numerous international trade intermediaries in China, since many small firms are relying on them to export under optimal decision processes.[13] Chinese intermediaries appear to have a lower product concentration and export more varieties per country than direct exporters. Moreover, in terms of underlying specific roles, as Ahn et al. (2011) suggest, Chinese intermediaries probably provide services ranging from promoting matches with foreign customers, exploring quality specifications required in foreign markets, and helping firms adapt their products to the needs of foreign consumers. In general, they help firms establish channels to export their products in destinations where small firms themselves could not cover the massive additional fixed/variable costs to reach international markets.

3.2.2 **Hypotheses**

Comparing direct exporters and exporters through intermediaries, we expect firms choosing the direct exporting mode to experience a better growth path. Firms relying on the intermediary sector incur a one-time global fixed cost that provides indirect access to all markets and allows firms to save on market-specific bilateral fixed costs. The disadvantage is that intermediation results in higher marginal costs of foreign distribution and fewer opportunities to learning by exporting. In contrast, engaging in frequent contacting with foreign consumers, direct exporters can enhance

[13] As discussed by Ahn et al. (2011), the set of intermediary firms could be identified from the ASIP (Annual Survey of Industrial Production) data set using Chinese characters that have English-equivalent meanings of "importer" "exporter", and/or "trading" in firms' name.

their export profitability more efficiently through investing in brand building, constructing their distribution system, product quality upgrading, etc. All these activities incur large fixed costs, and hence financial credits are expected to be more important for direct exporters. More formally, we have the following hypothesis:

Hypothesis 3.1 *Financial credits have a more pronounced effect on firm-level exports for firms that switch their exporting mode from indirect to direct exporting, relative to continuous indirect exporters.*

Furthermore, export distortions commonly exist in China. Khandelwal et al. (2013), for instance, document that export licenses are misallocated among textile exporters in China. In particular, small-sized firms, especially private domestic enterprises (PDEs henceforth), are less likely to be allocated export licenses because of small scale, but these firms are usually more productive and credit constrained. If deregulation gradually eliminates export distortions, we expect more small-size (financially constrained) but high-productivity firms would switch their exporting mode from indirect to direct exporting.[14] After switching from indirect to direct exporting, these small-sized but more productive switchers can better utilize financial credits to improve their exports through investing in brand reputation or size expanding.[15] As such, we expect that financial credits play a more pronounced role on switchers in the post-WTO accession period. More formally, we have the following hypothesis:

Hypothesis 3.2 *Conditioning on switching from indirect to direct exporting, financial credits have a more pronounced effect on firm-level exports in the post-WTO accession period.*

[14] In this paper's **Online Appendix**, we show that the majority of firms which switch their exporting mode are PDEs. We infer that the benefits by switching from indirect exporting to direct exporting are larger for PDEs. The **Online Appendix** further confirms our inference: financial credits play a more pronounced role on PDEs (relative to SOEs) that switch from indirect exporting to direct exporting, in terms of export growth and productivity improvement.

[15] More productive firms often produce higher quality products or charge a lower price. As such, after paying market penetration costs, these firms are more likely to increase their exports through accessing more foreign consumers.

3.2.3 **Data Description**

To empirically examine Hypothesis 1 and Hypothesis 2, we match two separate Chinese micro-level data sets to get the sample we are employing in the econometric analysis. The first data set is the Annual Survey of Industrial Production (ASIP) spanning the period 1998-2007. This survey, which collects annual firm-level data, is conducted by Chinese National Bureau of Statistics (NBSC). The data set is quite inclusive, in the sense that it incorporates all Chinese State-owned Enterprises (SOEs henceforth) and non-SOEs with annual sales over 5 million yuan (roughly speaking, 650,000 dollars at that time). In the survey, detailed firm-level information was collected, such as firms' geographic location, year of operation (i.e. the age of the firm), ownership type (state-owned, collective, private, foreign, etc.), employment, production and sales, balance sheet variables, and tax. As for this research, we focus on sales (especially exporting sales values) and balance sheet information, from which we construct exporting and finance variables in the econometric exercise. The second data set we use is product-level data from Chinese Customs (GACC), which were collected at a monthly frequency over the period 2000-2006. The Customs data cover the universe of transactions going through Chinese Customs, and contain firm-level information like geographic location, ownership type, exporting and importing variables (values, quantities, and unit prices), type of trade, mode of shipment, transit country, export destination country, and import source country.

First, we provide basic statistics for each data set. In the firm-level data set, ASIP, we list statistics of variables needed to compute firm-level productivity and the calculated productivity in **Table A.22**.[16] We inflate labor share (i.e. the ratio of total wage payment to value added) to match the number reported in Chinese input-output tables and national accounts (roughly 50%) as Hsieh and Klenow (2009) suggest. For the deflators of output, intermediate inputs and capital depreciation rate, we follow the tables constructed by Brandt et al. (2012). It is worth noting that when comparing domestically-selling firms to exporting firms, exporters have larger values of

[16] More specifically, we calculate the revenue productivity, denoted as TFPR, following the methodology introduced by Hsieh and Klenow (2009). Note also that TFPR is dimensionless in **Table A.22**.

TFPR and value added in **Table A.22**, which is consistent with the finding in the literature that firms with higher productivity export.[17]

Basic statistics for the Customs data set are presented in **Table A.23**.[18] We notice that Chinese exporters do expand rapidly during our sample period as Manova and Zhang (2012) find. During these seven years, the number of exporting firms has increased from 62,746 to 171,144, which is nearly a 200% gross growth. The average number of products each exporter shipped aboard, measured by the distinct 8-digit HS codes, has also increased from 30 to 36. Firms, on average, exported to 7 countries in 2000 and this increased to more than 8 countries in 2006. To some extent, this evidence suggests that joining the WTO has improved Chinese firms' exporting performance in the global market.

Following Manova and Yu (2016), we carefully match the two data sets. The detailed matching process is in the Appendix. Using the matched sample, we document summary statistics to gain some intuition for our econometric analysis in the following sections. To conduct the econometric analysis, we need to distinguish different types of exporters. Firms, primarily private domestic enterprises, which switched from indirect to direct exporting under the relaxed WTO regulations, are those that may have been most helped by an improvement in their financial conditions. Following Bai et al. (2017), we infer firms' exporting modes as follows. Firms from the ASIP data set are tagged as exporters if they report positive exports (otherwise they are non-exporters), and as direct exporters if they are also observed in the Customs data set. The fact that we observe the universe of transactions going through Chinese Customs allows us to tag the remaining exporting firms (those which are not observed in the Customs data set) as indirect exporters.[19] Firms that report exports larger than their exports in the Customs data set are exporting both directly and indirectly and are

[17] In addition to our main focus on the impact of finance on firm exports, we also check the effect of finance on firm productivity (measured by TFPR) in the empirical analysis because several studies in the literature suggest that exporting has a positive impact on firm productivity through learning, see Kraay (1999) on China, Aw et al. (2000) on Taiwan and South Korea, Girma et al. (2004) on UK, Van Biesebroeck (2006) on sub-Saharan Africa, and De Loecker (2007, 2013) on Slovenia.

[18] For the product-level Customs data, we first add up the entries to firm-level by exporting values. That is, if a firm exports more than one good, we add up the export values of all goods and then obtain just one entry for that firm.

[19] We also show in **Table A.24** the composition of different firms in our matched sample. Specifically, column 3 reports the number of direct exporters, indirect exporters, and non-exporters over years.

labeled direct exporters in this paper. Firms that do not sell domestically are removed from the sample. We notice that a classification bias might show up when direct exporters are misclassified as indirect exporters. This occurs when identical firms that have different Chinese names recorded in the two data sets are unmatched. By definition, in our sample, those unmatched direct exporters will be treated in the same way as indirect exporters. This misclassification only renders our estimation results downward biased, provided that direct exporters are generally more productive and have a higher degree of exposure to trade than indirect exporters.[20]

In **Table A.24**, we are comparing the three types of firms. Above all, we notice that the average export value of direct exporters is systematically higher than that of the indirect exporters over our sample period. Though both exporting values increased dramatically after 2004, the huge level value difference between them remained largely unchanged. The persistent difference suggests that switching from indirect to direct exporting may help firms to grow. This in turn probably provides firms an incentive to switch exporting modes. Next, we find large productivity differences between direct exporters and indirect exporters/non-exporters. The average productivity difference between direct exporters and indirect exporters is in the range of 5% to 20%. This is consistent with the literature that more productive firms are exporting directly as they can afford large additional exporting costs (Ahn et al., 2011). The average TFPR gap between direct exporters and non-exporters is also quite large. It lies between 10% and 30% across years. Also, more firms have been engaged in exporting and more exporters have decided to export directly. From 2000 to 2006 the percentage of exporters has increased from 26.6% to 29.3%. In 2000, 10.9% of firms are inferred to be direct exporters, while 14.7% are indirect exporters. However, in 2006, 15.7% of firms are direct while only 13.5% are indirect. The finding of more direct exporting firms is consistent with Ahn et al. (2011) and Bai et al. (2017), and can probably be explained by the fact that more productive PDEs are engaged in exporting directly in the hope of taking advantage of the favorable productivity and demand evolution.

[20] Based on the discussion in the Introduction Section, we argue that direct exporters have a more efficient finance usage than indirect exporters. Therefore, if we misclassified a direct exporter into the indirect exporter group because of matching failure, the effect of financial credits on indirect exporters would be over-estimated. Thus, our DID/DDD estimation results will be downward biased.

Our identification hinges on the variation in the composition of direct exporters, which says that more PDEs shall enter the group of direct exporters in the post-WTO accession period. To see whether more PDEs were participating in direct exporting after the trade deregulation, we plot in **Figure A.17** the evolution paths for the share of private-domestic-enterprise (PDE) direct exporters in the pool of all direct exporters. It shows that the share of PDE direct exporters had increased significantly since China's WTO accession in December 2001. Specifically, the share of PDE direct exporters within all direct exporters increase from 22% to more than 45%. It is worthy to notice that the peak of PDE direct exporting appeared when the regulation was fully lifted. This could be ascribed to the reason that PDEs that were exempted from regulation in 2001, 2002, or 2003 have planed to switch but start switching in 2004 after a preparation period. This explanation holds in general, considering that direct exporting involves such massive costs and revenue uncertainty that only fairly sizable firms (which were enfranchised in earlier years) can manage it and it takes time to get prepared. Alvarez and López (2005) also find strong evidence, using Chilean data, supporting the conclusion that firms consciously prepare for becoming direct exporters. Moreover, **Figure A.17** displays that the average productivity of new switchers had risen remarkably in the trade deregulation period. This may suggest that the trade regulation resulted in substantial misallocation in exporting licenses. When the regulation was lifted, the degree of distortion had been alleviated, which led to more productive but financially constrained PDEs to switch into direct exporting, and hence, improved the average productivity of switchers. Notice that after 2004, the final deregulation stage, the average productivity of new switchers further increases, which might suggest that the most productive but initially financial constrained PDEs start switching into direct exporters.[21]

As for the accuracy of the matched sample, we also pay attention to the issue of trade types. In recent work, Bernard et al. (2010, 2012) argue that carry-along trade is important in the data. This refers to firms who export final goods for other firms when exporting their own products,

[21]Lileeva and Trefler (2010) find that initially more productive firms experience a slower productivity growth through the learning-by-exporting effect. We would expect a smaller productivity gain for firms that switch their exporting mode latter, since these firms have a higher initial productivity.

thereby acting partially as intermediaries. However, in our benchmark regressions, we do not distinguish between such firms and those exporting only their own products, since the data *per se* provide no direct information for classification.[22] We dropped pure intermediaries between domestic producers and foreign buyers, i.e. those who show up in the Customs data set but do not report exporting in the survey data.[23] We also dropped processing and assembly trade firms as they do not use trade intermediaries.

3.3 Measurement and Empirical Methodology

In this section, we first construct key variables related to firm-level exporting, finance, and productivity. Then we set up the baseline econometric model to identify the increased and and time-varying increased effect of finance on firm-level export value when the firm switches exporting mode.

3.3.1 Construction of Key Variables

Before implementing the econometric analysis, we construct the following relevant measures for our study from the two raw data sets and the matched sample. We first construct measures of financial credits. There are various ways to measure internal and external finance based on firms' balance sheet information. We follow Berman and Héricourt (2010) and Guariglia et al. (2011) by defining internal finance (IF_{it}) as the ratio of cash flows (CF_{it}) over total assets (A_{it}), i.e. $IF_{it} = \frac{CF_{it}}{A_{it}}$, since it is a direct measure of the ability of a firm using its own accumulated liquidity to finance new investment. Like Berman and Héricourt (2010), we define external finance (EF_{it}) as the reciprocal of the ratio of total liabilities (L_{it}) over total assets, i.e. $EF_{it} = \frac{1}{L_{it}/A_{it}}$. It measures

[22] We check the robustness of carry-along trade by dropping the firms that have export shares higher than 25%. The export share is defined as the ratio of export value from the Customs data to total sales in the ASIP data. When we exclude these carry-along traders, the summary statistics in **Table A.24** and our main empirical results are barely changed.

[23] The Customs data do not label the intermediaries. Ahn et al. (2011) and Manova et al. (2015) identify them using keywords in firms' Chinese names, like the Chinese counterparts of "trade company", "export-import company", and so on. We address the issue by following this identification method and find that our benchmark results stay unchanged to a large extent.

the firms' ability to borrow from the outside, with a lower liability ratio entailing firms more space to get external funds.

We estimate firm-level productivity using the method introduced by Hsieh and Klenow (2009).[24] Since we do not have firm-level output price data, we focus on the "revenue productivity", i.e. TFPR.[25] The estimation of TFPR is conducted using the ASIP data set and the relevant variables for this estimation are value-added and inputs of labor and capital (at the firm level). Next, we define a key measure for this research, i.e. *exporting mode*, as a dummy variable that takes value 1 when a firm moves from indirect exporting in the previous year to direct exporting in the current year (note that it takes value 0 when the firm remains an indirect exporter from one year to the next).[26] Finally, we obtain measures of export values directly from the Customs dataset, in which exporting values measure the intensive margin of firm export.

3.3.2 **Empirical Methodology**

The empirical strategies we employ in this paper are panel data difference-in-differences (DID henceforth) and difference-in-difference-in-differences (DDD henceforth) regressions. With divergent cost structures and growth paths between direct and indirect exporting, Chinese exporters bearing different exporting modes could serve as an interesting subject for applying the DID methods. To study the positive effect of firm-level financial credits on export values (Hypothesis 1), we consider firms that switch from indirect to direct exporting as the treatment group and firms that

[24]To account for the robustness of firm-level productivity measure, we compare it with the widely used proxy variable methods with semiparametric estimation, including Olley and Pakes (1996), Levinsohn and Petrin (2003), Wooldridge (2009) and Ackerberg et al. (2015). We find no significant changes relative to our baseline results. To save space, we present only the results using the method by Hsieh and Klenow (2009). It is nontrivial to mention that all these measures are revenue based, given the limitation that there is no firm-level output price data.

[25]There is a concern that the TFPR might not reflect the real movement in firm-level productivity, thus not acting as an appropriate efficiency measure (See Garcia-Marin and Voigtländer, 2017). The reason is that TFPR is a combination of output price and physical productivity, i.e. TFPQ. When output price decreases, an increase in TFPQ might not be accompanied by an increase in TFPR. That is to say, the efficiency gain will not be captured by TFPR when it is translated into lower output price for consumers. In our study, this potential measurement issue will only downward bias the estimated result when physical productivity is available, conditioning on the fact revealed in Brandt et al. (2017) that trade liberalization upon China's WTO accession induced a drop in output price and more so for direct exporters that are large and productive.

[26]We report the transition matrix of three exporting status (direct exporting, indirect exporting, and non-exporting) in the **Online Appendix**.

continue to use indirect exporting as the control group. During the WTO accession period, the Chinese government lowered the registered capital requirement, which then encouraged more PDEs to switch from indirect to direct exporting. The policy change thus provides a quasi-experiment that allows us to study the impact of export deregulation on the promoting role of finance on firm export performance (Hypothesis 2). The impact of switching exporting mode promoted by financial credits on firm-level export performance might not be time invariant before and after the WTO accession since the trade deregulation provides better opportunities for financial credits to contribute in direct exporting. To capture the time-varying impact of the treatment effect promoted by financial credits, we divide the sample into pre- and post-WTO accession periods to generate cross period differences when applying the panel data difference-in-difference-in-differences method.

First, we test Hypothesis 1: financial credits play a more pronounced role on those who switch their exporting mode from indirect to direct exporting than continuous indirect exporters. Furthermore, by directly participating in export markets, exporting firms are more likely to invest in productivity-enhancing activities, and innovate in a more efficient way. Thus, relative to indirect exporters, financial credits drive a faster productivity growth for switching firms (see Chen and Guariglia, 2013; Bai et al., 2017). Following the research designs of Meyer (1995) and Imbens and Wooldridge (2007), we conduct our first estimation using an individual-level panel data difference-in-differences model for multiple time periods:

$$\begin{aligned} y_{it} = \alpha + \beta \times x_{it} + \tau_1 \times dExportingmode_{it} + \tau_2 \times dExportingmode_{it} \times x_{it} \\ + \mathbf{z}_{it}\gamma + c_i + \eta_t + u_{it}, \qquad (3.1) \end{aligned}$$

where y_{it} is the firm-level export or productivity, x_{it} is our measure of financial credits, and $\mathbf{z}_{it}$ are individual-specific controls which include x_{it}. The dummy variable $dExportingmode_{it}$ captures the change from indirect to direct exporting, it equals 1 if a firm switches from indirect to direct exporting and equals to 0 if it remains an indirect exporter.[27] η_t captures year fixed effects. The

[27]We define treatment as switching from indirect to direct exporting, and then evaluate relevant economic implication of this treatment. The choice of the treatment is not merely in line with the theory on the cost and benefit

coefficient τ_1 captures the average treatment effect (to be precise, on the treated) of switching the manner of exporting, and τ_2 is the average treatment effect further promoted by financial credits. We expect a significant and positive τ_2 for both export values and productivity (TFPR) regressions.

We estimate the empirical equation above using the fixed effect (FE) panel data method to control for firm-level unobserved heterogeneity c_i. However, it must be noted that in our context the empirical analysis based on the classic panel data difference-in-differences model might be unreliable since it is subject to an endogeneity (or self-selection) issue. If a firm's intensive exporting decision (i.e. to export more) encourages the firm to switch from indirect to direct exporting, then the $dExportingmode_{it}$ variable in the difference-in-differences equation is endogenous and the FE estimation is invalid.[28]

We address the endogeneity issue using the instrumental variable approach. First, we instrument the switch in the exporting mode variable $dExportingmode_{it}$ with the product of firms' base-year productivity and one period-lagged province-level aggregate capital supply shock. Firm-level base-year productivity, $TFPR_{i,t_0}$, is firm i's productivity in the base year,[29] and province-level aggregate capital supply shock is the availability of regional capital stock devoid of State interventionism and accessible to all individual firms Economically, a higher regional capital supply shock can help to ease the financial needs of firm-level export mode switching, but does not correlate with firm-level exports. Whereas, this aggregate shock lacks within-region variation. As such, we multiply one period-lagged regional capital supply shock by firm-level base-year productivity, to generate sufficient variation for the instrument at the firm level. Firm-level base-year productivity

heterogeneity of alternative exporting modes, but also motivated by our data. It shown in our sample that on average the transition probability from indirect to direct exporting is double of that from direct to indirect exporting. To be specific, the average annual transition probability is 6.3% versus 3.5% (see the **Online Appendix**). Thus, our data suggests that switching from indirect to direct exporting is a relatively more important than the reverse case, which also inspires us to focus on this phenomenon in the current study.

[28] Moreover, a selection problem might occur as a result of our differencing calculation in the FE method, when firms do not disappear from our sample but become unobserved for some periods (e.g. some firms stop exporting for a few years and re-enter later). Firms that stop exporting for a few years may not be as productive as constant exporters, thus the probability of their being observed is correlated with our independent variables, individual effect and the error term. Yet, the selection problem does not undermine our estimation because it leads to a downward bias and is less severe if the panel is short, which is just our case.

[29] We also keep only firms which enter our sample no later than 2000, and all base-year productivity is computed in year 2000. All results are only marginally different, which are available upon request.

is a key determinant in firm-level export mode, but it does not correlate with current exports since it is predetermined at the very beginning.

Exploring the idea proposed by Jarreau and Poncet (2014), we characterize aggregate capital supply using a financial market deepening variable, which is the market share of banking credits extended by banks other than China's four biggest state-owned banks (namely, Industrial & Commercial Bank of China, Bank of China, China Construction Bank, and the Agricultural Bank of China). A higher market share of these non-Big4 banks in total bank credits implies a higher degree of financial market liberalization, and thus more financial access or capital supply for individual firms.[30] Since only province-level information on banking credits is available, we construct this variable for each province of China, thus all firms within a province share the same capital supply shock. To further mitigate the endogeneity concern, we use one-year lagged market share to construct the instrumental variable. Specifically, the instrumental variable for $dExportingmode_{it}$ is $TFPR_{i,t0} \times NonBig4_{p,t-1}$, where $t0$ is the base year, $NonBig4_{p,t-1}$ is the one-year lagged share of banking credits extended by banks other than Big4 state-owned banks in province p.

Second, to alleviate the possible endogeneity in internal and external finance, we proxy the current value of finance by its lagged value in all baseline regressions. This approach eliminates reverse causality between firm-level finance and exports. As a robustness check, we further follow Manova et al. (2015) to proxy firm-level finance using province-sector finance measures.

Finally, to examine Hypothesis 2, we want to show the time-varying impact of the treatment augmented by financial credits on export values before and after the trade deregulation. As suggested by Meyer (1995) for treatments that involve higher-order interactions, we conduct our difference-in-difference-in-differences (DDD) estimation for multiple time periods:

[30] In China, the market share of these big state banks in total bank credits was basically declining, which was a natural outcome following the gradual financial reforms since the 1990s. Primarily completed financial reforms include the promulgation of the Commercial Bank Law that provides a legal basis for changing the specialized state banks to state-owned commercial banks. It also meant the transformation of the share holding system in the four biggest state-owned banks, which helped establish a standardized corporate governance and an internal system of rights and responsibilities in accordance with the requirements for modern commercial banks. Other reforms like establishing privately owned small banks, accelerating interest rate liberalization, developing a deposit insurance scheme and improving financial institutions' market exit mechanism are already well underway.

$$
\begin{aligned}
y_{it} = {} & \alpha + \beta_1 \times x_{it} + \tau_1 \times dExportingmode_{it} + \beta_2 \times dPost_t + \tau_2 \times dExportingmode_{it} \times x_{it} \\
& + \beta_3 \times dExportingmode_{it} \times dPost_t + \beta_4 \times dPost_t \times x_{it} \\
& + \tau_3 \times dExportingmode_{it} \times dPost_t \times x_{it} + \mathbf{z}_{it}\gamma + c_i + \eta_t + u_{it}.
\end{aligned} \tag{3.2}
$$

In the DDD regression, we are interested in the triple interaction term of finance (x_{it}), treatment (switch in exporting mode, $dExportingmode_{it}$), and policy intervention (before or after the export deregulation induced by WTO accession). All year and individual fixed effects are captured by $\{\eta_t, c_i\}$ in our fixed-effect panel regression. The other key terms in the regression are the double interaction term of treatment and WTO accession (which characterizes the time variation in the treatment of interest) and the term for treatment *per se*. The dummy variable $dPost_t$ captures the impact of China's policy change in exporting mode induced by the WTO accession; it equals 1 if the year is after 2001 (or 2002, or 2003, depending on how we divide the sample into pre- and post-WTO accession periods, since the trade deregulation was phased in rather than once for all). The variable $dExportingmode_i \times dPost_t$ will be 1 if a firm switched from indirect to direct exporting and the year is later than the policy year (it could be 2001 or 2002, or 2003). The coefficient τ_3 measures the difference in average treatment effect promoted by financial credits before and after China's WTO accession across firms, i.e. the time-varying treatment effect promoted by finance. Again, we estimate the empirical equation above using the fixed-effect (FE) panel data methods to control for firm-level fixed effects and control for the endogeneity issue in switching exporting modes using the instrumental variable method we introduced above.

3.4 Baseline Results and Robustness Checks

This section presents and discusses the empirical results of this paper. We begin with the panel data difference-in-differences estimation to show the increasing role of finance in promoting firm-level exports and productivity when a firm switches its exporting mode from indirect to direct export (Hypothesis 1).[31] Next, we employ panel data difference-in-difference-in-differences estimation to examine how the role played by finance on switching firms varies over time, especially before and after China's WTO accession (Hypothesis 2). For both types of estimation, we include the results with and without the instrumental variable to account for the endogeneity issue in switching exporting mode.

3.4.1 Difference-in-Differences Estimates

Table A.25-A.26 show the difference-in-differences estimation results for firm-level export value with internal and external finance.[32] We estimate four scenarios distinguished by two dimensions: whether the switch in exporting mode is instrumented and whether firms' age and size (measured by firms' capital stock) are controlled for. As the young and small firms tend to rely more on financial credits to grow, we control for firm age and size to isolate the impact of export mode switching on firms' export performance.[33] Column 1 and 2 of **Table A.25** and **A.26** present results for the scenarios without instrumenting the switch in exporting mode. It turns out that the estimates are barely changed when we control for firms' age and size. The estimates show that there is a significant increase in the role of financial credits in encouraging firm's export value when the firm switches from indirect to direct exporting. Specifically, a 10% increase in internal (resp. external) finance on average increases the promoting effect of finance on firms' export value by 1.10% (resp. 1.72%) when the firm switches its exporting mode. Column 3 and 4 indicate that after instrumenting the switching in exporting mode with the product of firms' base-year productivity and one period-lagged province-level capital supply shock, the increased encouraging effect is even larger.[34] Specifically, a 10% increase in internal (resp. external) finance boosts the promot-

[31] To account for the potential endogeneity issue of financial credits, we proxy the current value of finance by its first-order lagged value in all baseline estimations. We will further explore this issue using province-sector-level financial variables. Moreover, it is worthy to point out that we take log values for all continuous variables in our regressions.

[32] Note that we report estimated coefficients for all independent variables in **Table A.25-A.37**. To save space, in tables which conduct robustness checks, we only report estimated coefficients for key variables.

[33] In all estimations, we also control for the yearly aggregate effect that would cause the changes in the difference-in-differences or difference-in-difference-in-differences estimates even in the absence of treatment, i.e. the switch in exporting mode.

[34] We have instrumented the switching dummy whenever it appears in the specifications of column 3 and 4.

ing effect on firm-level export value by 4.33% (resp. 2.93%) (controlling for firms' age and size makes no difference).[35]

The difference-in-differences estimation results for firm-level productivity with internal and external finance are presented in **Table A.27** and **A.28**. We consider the same four scenarios as in **Table A.25** and **A.26**. Also the estimates show that the increase in the encouraging effect of finance in promoting firms' productivity is both statistically and economically significant. In the scenarios without the instrumental variable for the switch in exporting mode (columns 1 and 2 of **Table A.27** and **A.28**), a 10% rise in internal (resp. external) finance on average increases firms' productivity by 0.14% (resp. 0.06%). If we use the instrumental variable, it indicates that the increase in firms' productivity will be 0.78% (resp. 0.66%), which is substantially larger than the OLS (ordinary least squares) estimates. Compared to the magnitudes for export values, it is suggestive that there is not a perfect transmission (i.e. incomplete pass-through) from the increase in firms' export value to that in productivity even though the transmission channel is positive.

In addition, we also find that the coefficients of $dExportingmode$ and internal (resp. external) finance have the expected signs. In particular, if a firm switches from indirect to direct exporting, its exports and TFPR increase by 21.96% and 1.98%, respectively.[36] Meanwhile, for continuous indirect exporters, a 10% increase in firm-level internal finance (external finance) will increase firm-level exports and TFPR by 1.45% (0.92%) and 0.11% (0.06%), respectively. All results support Hypothesis 1.

Before moving on to the comparison between internal and external finance, we find it necessary to discuss the difference between OLS and IV estimates in **Table A.25**-**A.28**. A salient pattern in these tables is that the IV estimates are much larger than OLS estimates, with an inflation

[35]We implement the weak-identification test for all estimations with instrumental variables to address the potential weak-instrument problem following the routines proposed by Baum et al. (2007). As they suggest, it is better to use the robust analog of the Cragg-Donald (1993) F statistic, i.e. the *rk* Wald F statistic to replace the original Cragg-Donald F statistic. Though there does not exist a test for weak instruments in the presence of non-i.i.d. disturbances, the *rk* Wald F statistic is a sensible option as it is the state-of-the-art in the presence of heteroskedasticity, autocorrelation, or clustering. All of our IV estimations pass the weak-identification test, as the *rk* Wald F statistics are far larger than 10, which not only surpasses the critical values compiled by Stock and Yogo (2005) but also conforms to the "rule of thumb" of Staiger and Stock (1997).

[36]This calculation is based on column 4 of **Table A.25** and **Table A.27**, respectively. Specifically, $exp(0.1985) - 1 = 0.2196$, and $exp(0.0196) - 1 = 0.0198$.

even more than fivefold in the case of productivity. We ascribe this difference to the fact that the instrumental variable method assigns more weights to firms that expect large gains from switching exporting modes and accumulating finance, thus inflating the average treatment effect from firm-specific or heterogeneous causal impact. To be specific, following the logic of Imbens and Angrist (1994) and a recent application by Lileeva and Trefler (2010), we write the average treatment effect from OLS estimation as $\tau + \mathbf{E}(U)$, where τ is the same for all firms and U is the firm-specific or heterogeneous causal impact. The (local) average treatment effect from IV estimation can be written as $\tau + \big(\mathbf{E}(U \times \Delta p)/\mathbf{E}(\Delta p)\big)$, where Δp is the change in probability of switching exporting modes induced by the instrumental variable in the first stage estimation. Δp acts as the weight used to average U across firms. In the OLS case, the weight is the same across all firms since $\mathbf{E}(U)$ is estimated just using simple sample average. Yet, the IV estimation puts more weight on firms that expect to gain substantially from switching exporting modes and accumulating finance, thus $\big(\mathbf{E}(U \times \Delta p)/\mathbf{E}(\Delta p)\big) > \mathbf{E}(U)$.

Noticeably, the difference-in-differences estimates in the internal and external finance cases are strikingly different. As for the export values, the IV estimation in **Table A.25** and **A.26** shows that the case of internal finance produces much larger estimates. In particular, it turns out that the effect of a 10% increase in internal finance in promoting firm's export value on average increases by 1.40% (4.33% minus 2.93%) more than that of external finance when the firm switches exporting mode. This finding is consistent with the argument in Manova et al. (2015) that direct exporters are believed to be more dependent on outside funds than indirect exporters and domestic producers, in order to cover large entry and fixed costs when entering international markets. Take a representative firm as an example, it incurs large upfront entry and fixed costs (like studying the profitability of potential markets, product adjustment, and setting up distributional networks) when starting to export directly. These mostly once-and-for-all exporting costs (i.e., entry costs into export markets) are substantial and could not be covered in general by firms' retained earnings or internal cash flows from routine operations. As a result, direct exporters typically rely more heavily on outside rather than internal financing to prepay entry and fixed costs. Alternatively, it means that external credit is more crucial in financing for entry and fixed costs of direct exporting. The variable costs of direct exporting (such as intermediate input, salaries, and equipment rental fees), however, are not as lumpy, which leaves plentiful room for internal finance to take effect. Since export value is a flow variable and the associated costs are variable costs, we should expect internal finance will have a larger impact than external finance. Our difference-in-differences estimates provide solid support for this argument.

As for the productivity, **Table A.27** and **A.28** show that the internal finance case produces much larger estimates again. To be specific, it turns out that a 10% increase in internal finance promotes firm's productivity on average by 0.12% (0.78% minus 0.66%) more than that of external finance when the firm switches its exporting mode. This is consistent with the case for export values above. We find two reasons are potentially responsible for the smaller promoting role of external finance in raising firms' productivity. First, as more external finance is allocated to cover entry and fixed costs, most of the increase in external finance cannot be counted as capital. It in turn means that the external finance is primarily not relevant for firms' production process, at least in a sense of direct relevance. Second, even though some part of the external finance that is used in exporting could be counted as capital (like the part for making market-specific investments in capacity and product adjustment), it basically helps firms upgrade product composition rather than directly helping firms produce more products. Since our revenue productivity measure cannot reflect the upgrade in product composition, external finance exhibits a smaller promoting role in our regressions.

3.4.2 **Difference-in-Difference-in-Differences Estimates**

In **Table A.29** and **A.30**, we report the results for the difference-in-difference-in-differences estimation of export value with financial credits.[37] To save space, we report only the IV estimation

[37] Since we concentrate on investigating the time-varying impact of finance on firm export performance in this study, to save space, we do not present DDD estimates for firm productivity as we do so for the DID case. However, it is necessary to mention that we get negative DDD estimates for productivity, which can be rationalized via a "negative selection" proposed by Lileeva and Trefler (2010). It basically says that when initially more productive PDEs switch to direct exporting in the post-WTO accession, productivity growth is expected to be slower.

results. Estimations capture the changes in the promoted impact of treatment (switching from indirect into direct exporter) by finance on firms' exporting value before and after China's exporting deregulation. Since the WTO accession and associated trade deregulation was phased in, we consider different threshold years to divide our whole sample (2000-2006) into pre- and post-WTO accession periods. Specifically, as discussed in the policy background, we consider three threshold years: 2002, 2003, and 2004.[38] Above all, **Table A.29**-**A.30** show that the estimates basically remain unchanged when we control for firms' age and size. Specifically, we find an increasing treatment effect of finance on switchers' exports, i.e. relative to exporters that switch their exporting mode in the pre-WTO accession period, on average, financial credits increase firm-level exports more for firms that switch exporting mode in the post-WTO accession period, no matter which threshold year we choose to distinguish pre- and post-WTO accession periods. This increase in the augmented treatment effect by finance substantiates the time-varying hypothesis of this paper (Hypothesis 2). Export distortion is a possible interpretation for the time-varying impact of financial credits: i.e., in the pre-WTO accession period, export licenses favored larger and state-owned enterprises (SOEs). In contrast, more productive but financially constrained PDEs have to export through intermediaries.[39] Switchers are less financially constrained firms, but not necessarily firms that expect larger export or productivity growth after switching export mode. When the distortion has been alleviated in the post-WTO accession period, firms that expect larger export growth switch their exporting mode, which leads to an increasing impact of financial credits.

Table A.29 and **A.30** also show that the increase in the promoting effect of finance on firms' exporting value is larger when we choose an earlier threshold year to divide our sample into pre- and post-WTO accession periods. More specifically, if we treat 2002-2006 as the post-WTO accession period, conditioning on that a firm switches exporting mode, a 10% increase in firm-level internal (resp. external) finance on average leads to a 3.89% (resp. 8.60%) more export sales in the post-WTO accession period. When we postpone the threshold year to 2003, the average increase

[38]Threshold year equal to 2002 means that the post-WTO accession period includes 2002-2006. The same logic applies to other choices of the threshold year.

[39]Khandelwal et al. (2013) find that before the quota removal of textile and cloth products in China, SOEs are more likely to obtain quotas than other firms, but they are featured with low production efficiency.

in export values promoted by a 10% increase in internal (resp. external) finance falls substantially to 0.97% (resp. 2.72%). And it is further decreases to 0.62% (resp. 2.61%) if the threshold year is 2004. The differences resulting from the choice of the threshold years are related to the fact that China's deregulation in direct exporting rights is a gradual process.[40] It allowed different groups of firms to satisfy the direct exporting requirement in different years. As discussed in Section 2, the registered capital requirement in direct exporting for PDEs dropped dramatically from 8.5 million yuan (or 5 million yuan if the firm was publicly owned) to 3 million yuan in 2001, which grants a great number of PDEs to be eligible to export directly in our sample. When those credit-constrained PDEs started to export directly in 2002, they were enormously more in need of finance than previous direct exporters that were primarily non-constrained SOEs. As a consequence, a boost in the encouraging role of finance on firm exporting value occurs when many PDEs were enfranchised to export for the first time. The encouraging role then fell quickly in later years because the further deregulation just released more PDEs with lower registered capital to export directly. Those PDEs essentially were similar as the enfranchised PDEs in 2002, though a bit more credit constrained due to their smaller scale and thus still generating positive estimates when a later threshold year is chosen.

One surprise is that we observe a higher increase in the promoting role of external finance than internal finance in our difference-in-difference-in-differences estimation for export values, no matter how we choose the threshold year for dividing pre- and post-WTO accession periods. Since the difference is along the time dimension, it might reflect that firms' access to internal finance was self-determined and largely unchanged in pre- and post-WTO accession periods, yet the access to external finance has been greatly improved with concurrent financial reforms. The relaxation in acquiring banking and trade credits thus provides a greater possibility for external finance to make a contribution.

[40]It is equivalent to treating the elimination of export distortion as a gradual process.

3.4.3 **Utilization of Finance Matters**

In this section, we investigate the firm-level heterogeneous efficiency in utilizing finance to uncover the mechanism that results in our difference-in-differences and difference-in-difference-in-differences regression results. To this end, we construct four types of measure related to firms' usage of finance, and check how they change when firms engage in switching from indirect to direct exporting.

Working capital management has long been regarded as an effective way to increase firms' profitability (e.g., Shin and Soenen, 1998; Petersen and Rajan, 1997; Deloof, 2003; Eljelly, 2004). The four measures that characterize the efficiency of firms' usage of finance are current liquidity ratio, receivable turnover ratio, inventory turnover ratio, and operation cycle (Eljelly, 2004; Ding et al., 2013). First, current liquidity ratio (CL_{it}) is the ratio of liquid liability (LL_{it}) to liquid assets (LA_{it}), i.e. $CL_{it} = \frac{LL_{it}}{LA_{it}}$, which expresses a company's ability to repay short-term creditors out of its total cash. A lower liquidity ratio indicates that a company is more liquid and has better coverage of outstanding debts, thus suggesting a higher efficiency in managing liquidity. Second, receivable turnover ratio (RT_{it}) is the ratio of net credit sales (NCS_{it}) to average accounts receivable (AR_{it}) in previous and current periods, i.e. $RT_{it} = 2 \times \frac{NCS_{it}}{AR_{i,t-1}+AR_{it}}$. It quantifies a firm's effectiveness in extending credit and in collecting debts on that credit. The receivable turnover ratio is an activity ratio measuring how efficiently a firm uses its assets. Third, inventory turnover ratio (IT_{it}) is defined as current sales (S_{it}) divided by average inventory (IN_{it}) in two recent periods, i.e. $IT_{it} = 2 \times \frac{S_{it}}{IN_{i,t-1}+IN_{it}}$. It is a ratio showing how many times a company's inventory is sold and replaced over a single period. A high turnover implies strong sales and, therefore, weak inventory, which then indicates that the firm is more efficient at generating returns from its assets and thus maintaining healthy financial conditions. Forth, operation cycle (OC_{it}) is the sum of two parts, days receivables outstanding and days inventory outstanding within a year, that is, $OC_{it} = \frac{365}{RT_{it}} + \frac{365}{IT_{it}}$. It is also known as the cash conversion cycle, measuring how long a firm takes to convert it sales into cash holdings. A shorter operation cycle means better management performance and more efficiency in utilizing cash.

Figure A.18 plots the dynamic paths of four financial variables defined above. It shows that, over the period 2001-2004 when the trade deregulation on direct exporting rights phased in, switchers exhibit higher efficiency and larger efficiency gains in finance usage than non-switchers, where switchers are firms switching from indirect to direct exporters while non-switchers are continuous indirect exporters. In particular, switchers not only have lower liquidity ratio but also exhibit a steeper decline than non-switchers, from 1.08 to 1.04 versus from 1.10 to 1.09. Similar patterns apply to inventory turnover ratio and operation cycle. Switchers have a higher inventory turnover ratio and shorter operation cycle. They also experience a steeper increase in their inventory turnover ratio and more significant drop in operation cycle. One exception is that the receivable turnover rate divergence between switchers and non-switchers occurs after 2005, rather than over the phase-in period of 2001-2004. This might be caused by the aggressive expansion of direct exporters when the direct exporting was fully liberalized. In that case, direct exporters tend to sell aggressively even when they cannot receive payments immediately, which then leads to massively accumulated accounts receivable and suppresses the receivable turnover ratio.

We further run panel data difference-in-differences regressions for all the four types of financial variables. As in the baseline case, the treatment is defined as the switch from indirect to direct exporting. Results are reported in **Table A.31**. It reveals that exporters experience lower liquidity ratios, higher inventory turnover, and a shorter operation cycle when they switch from indirect to directing exporting, in comparison with the case where firms continue as indirect exporters. As for the receivable turnover rate, the treated group barely gains any efficiency. The coefficient is not significant, neither statistically nor economically.

The efficiency measures of finance utilization strongly suggest that switchers are better users of financial credits, which helps to explain the positive average treatment effect in our DID regressions. This finding is consistent with the learning channel for direct exporters. First, switchers need to effectively utilize finance to support the learning process. After switching into direct exporting, firms have access to frequent contacts with foreign consumers and producers (see Egan and Mody, 1992, for more details), which encourages them to better design products and raise competitiveness via technology upgrading. All the learning activities require support from more finance, thus in turn urging firms to more efficiently exploit existing financial credits that typically are scare when firms are serving international markets. Second, direct exporting brings about better growth opportunities for productivity and demand, the higher expected returns also spur switchers to hike finance utilization rates. Bai et al. (2017) demonstrate that direct exporting generates much more favorable productivity and demand evolution for switchers. In that case, a profit-maximizing firm will naturally be incentivized to speed up the velocity of financial credits so that it can reap more future benefits from exporting given a fixed amount of financial credits.

The channel of finance utilization also works well to explain our main findings in the panel data difference-in-difference-in-differences estimation. Relative to continuing indirect exporters, firms that switch their exporting mode from indirect to direct export, on average, have higher efficiency in utilizing finance. As such, we would expect a higher efficiency gain in the post-WTO accession period when more PDEs participated in this type of switch, since these PDEs are firms which are financially constrained but have large expected export or productivity growth after switching from indirect to direct exporting. This type of switchers has higher efficiency in finance usage. When finance is more difficult to acquire for PDE firms, we also naturally anticipate that they would even more efficiently utilize financial credits. The byproduct of increased financial efficiency lends support for the heightened time-varying effect of finance on firm exporting in the post-WTO accession period.

3.4.4 **Robustness Checks**

First, province-year policy shocks and sector-year macro shocks may influence firm-level export and productivity growth. We address this concern by simultaneously controlling for province-year and sector-year fixed effects instead of year fixed effect in the benchmark regression. The DID results are reported in **Table A.32**-**A.35**, and DDD results are reported in **Table A.36**-**A.37**, respectively.

Results reported in **Table A.31**-**A.35** are highly comparable with those in **Table A.25**-**A.27**.

After controlling for province-year and sector-year fixed effects, we still find that on average the encouraging effect of finance on firm-level exports and productivity increases when firms switch their exporting mode from indirect to direct exporting. Meanwhile, the results in **Table A.36-A.37** also manifest similar patterns as those in **Table A.29-A.30**. After controlling for province-year and sector-year fixed effects, the results in **Table A.29-A.30** indicate an increasing influence of finance on switchers's exports in the post-WTO accession period.

Second, if productivity exhibits high persistence, base-year firm-level productivity may still be endogenous. Export license favors state-owned enterprises and larger firms. Therefore, the share of non-SOEs in each industry will affect the possibility that the domestic firms acquire their direct export licenses.[41] This further influences the firms' incentive of switching their exporting mode after export deregulation. Economically, a higher share of non-SOEs in an industry in the base year increases firm-level incentive to switch from indirect to direct exporting after deregulation (because it means that firms face less competition to acquire export licenses from those preferentially treated SOEs), but does not correlate with firm-level productivity or exports. To instrument $dExportingmode_{it}$, we multiply the share of non-SOEs in each industry in the base year by the the lagged regional capital stock that is generally accessible to all individual firms. In addition, to avoid industry-year and province-year specific factors affecting the treatment effect, in all regressions, we control industry-year and province-year fixed effects rather than year fixed effects. **Table A.38** and **A.39** report DID and DDD results, respectively. All results indicate that using the new instrument and control for different fixed effects only slightly change the magnitude of our benchmark results.[42]

Third, firm-level internal and external finance rely on firm-level performance. Therefore, firm-level exports and TFPR may potentially influence firm-level internal and external finance, and endogeneity in finance arises. We shall show that a more rigorous proxy for endogenous firm-level financial credits that captures relatively exogenous variation in these variables does not change our

[41] We choose year 2000 as the base year and compute the share of non-SOEs in each industry in this base year.

[42] In all regressions, we include the individual terms which appear in the interaction terms, but we only report the key coefficients in the following tables to save space. The full results are available upon request.

main findings. We follow the idea of Manova et al. (2015) to proxy firm-level internal and external finance with their sectoral counterparts. We expand it to province-sector-level proxies to generate more reasonable variation in finance, which can then be employed to identify the augmented treatment effect from firms' financial credits. With province-sector proxies for firm-level internal and external finance, we re-estimate our benchmark model. The DID and DDD results are reported in **Table A.40** and **A.41**, respectively.

Results in column 1 and 2 of **Table A.40** reveal that replacing firm-level finance using province-sector level counterparts marginally changes our baseline results. The statistical significance keeps unchanged and economic magnitude is just slightly changed. We still have the conclusion that on average the encouraging effect of finance on firms' export value increases when a firm switches its exporting mode. Results in column 3 and 4 of **Table A.40** confirm the robustness of our baseline results in **Table A.27**-**A.28** for firms' productivity, i.e. TFPR. They exhibit marginally changed estimates, and the main conclusion we drew previously still holds, that the encouraging effect of finance on productivity is higher when the firms engage into the switch from indirect to direct exporting. Moreover, it still shows an incomplete pass-through from gains in export value to gains in productivity.

The results in **Table A.41** are also highly comparable to those in Table **Table A.29**-**A.30**, with only a sensible change in magnitudes. It reinforces our baseline finding that the increased encouraging effect of finance on firm's export value is higher when the firm switches from indirect into direct exporter and if the firm is observed in the post-WTO accession period. The results are robust to how we separate pre- and post-WTO accession periods. It also underscores a declining difference-in-difference-in-differences estimate when we choose a later threshold year to separate pre- and post-WTO accession periods, which essentially reflects the phase-in feature of China's deregulation on directing exporting rights.

Lastly, we conduct a number of further checks to examine the robustness of our benchmark results, including but not limited to controlling for firms' ownership (PDEs or SOEs), exporting regime (processing or ordinary trade), alternative specifications, etc. All results are in line with our benchmark results, which are reported in the Appendix.[43]

3.5 Conclusion

This paper examines the heterogeneous and time-varying feature of the impacts of finance on firm exporting behaviors when a firm switches from indirect to direct exporting mode in the context of China's WTO accession. To fulfill WTO accession commitments, China gradually lifted the restriction in direct exporting rights over the period 2001-2004. It is noticeable that the regulation on exporting modes primarily inhibited PDEs from exporting directly while more SOEs were exempted, as their registered capital requirements were quite different. Direct exporters feature more favorable future outcomes, e.g. productivity and demand stock growth (Bai et al., 2017). Using panel survey data, we show that financial credits improve the firm-level exports and productivity more for firms that switch from indirect to direct exporting.

Knowing that PDE firms were typically credit constrained, we conjecture that the impact of financial credits on firm exports when the firm switches from indirect to direct exporting mode would be larger after China's export deregulation upon its WTO accession. This is because that many more PDEs were granted the opportunity to engage in direct exporting and the direct exporting typically incurs massive additional fixed/variable costs as well as subsequent investment in upgrading technology.

Using a panel data difference-in-difference-in-differences approach combined with instrumental variable methods to control for potential endogeneity issues associated with the switch in exporting modes, we find strong evidence to substantiate our time-varying hypothesis. The difference-in-difference-in-differences estimation produces a further increase in the role of finance in promoting firms' export value in the post-WTO accession period. The main results remain unchanged after controlling for possible endogeneity issues.

Though we are focusing on the time-varying impact of finance on firm performance, our work

[43]The results indicate that PDEs and firms engaging in ordinary trade benefits more from switching than SOEs and firms engaging in processing trade.

has strong implications in two dimensions. First, we show the heterogeneous impact of financial credits on different firms. We demonstrate that finance could make a pivotal contribution to firm-level exports and productivity growth when firms have a higher efficiency in finance usage. Second, our study implies additional welfare gains of trade liberalization. Our empirical findings suggest that when distortions exist, trade liberalization is an effective way to eliminate the distortion. Further, the elimination of distortions makes financial markets play a more pronounced role in improving firm-level exports, which results in additional welfare gains as export share further increases.

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APPENDICES

A.1 Appendices for Chapter 1

A.1.1 Figure Appendix

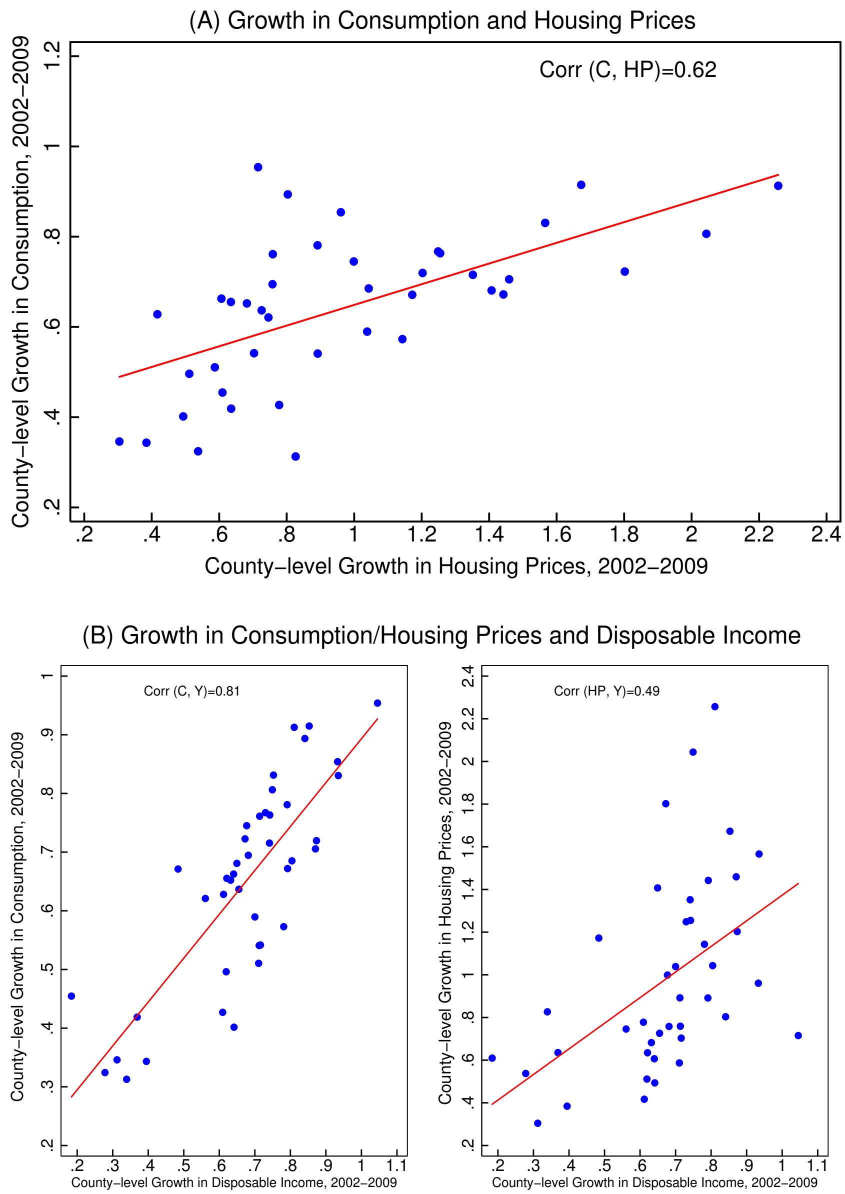


Figure A.1: Correlation patterns: consumption, housing prices, and income

Notes. This figure plots cross-sectional correlation patterns between growth in consumption, disposable income, and housing prices across counties during 2002-2009.

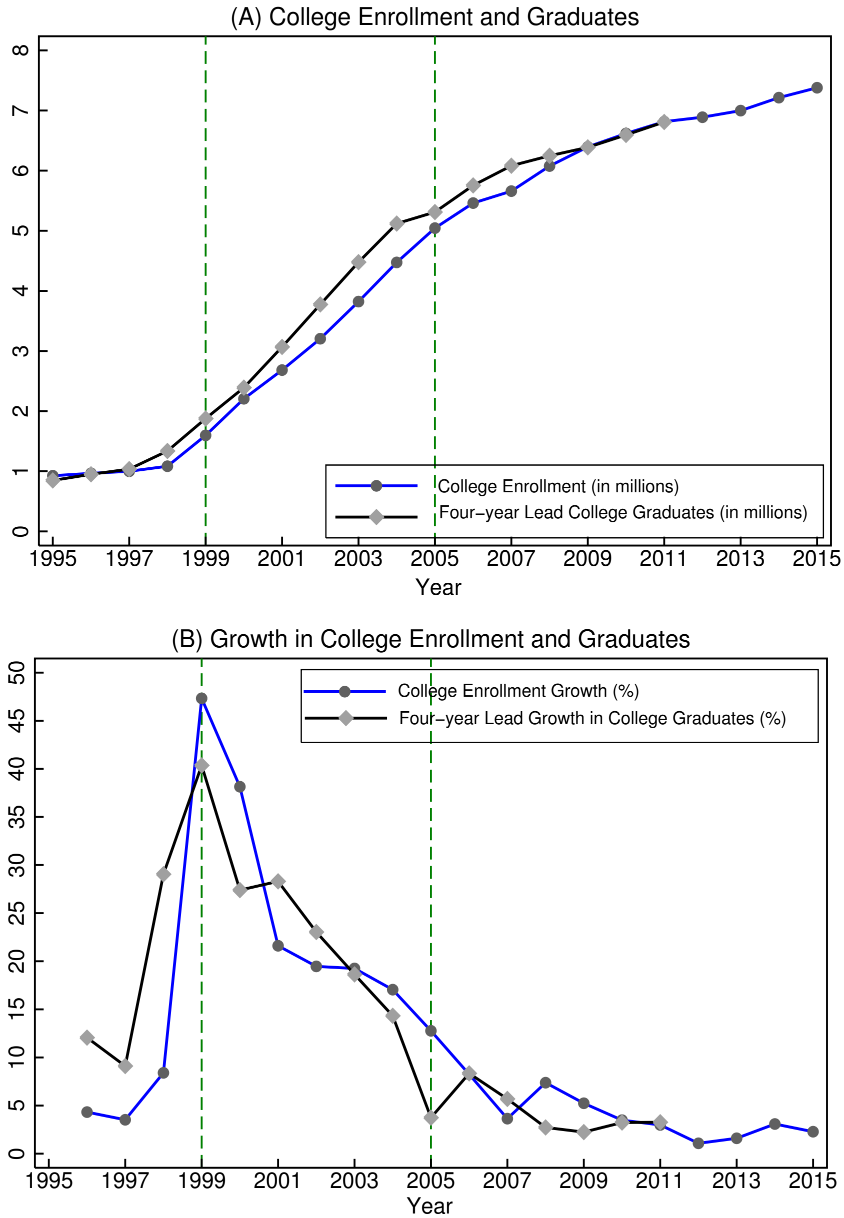
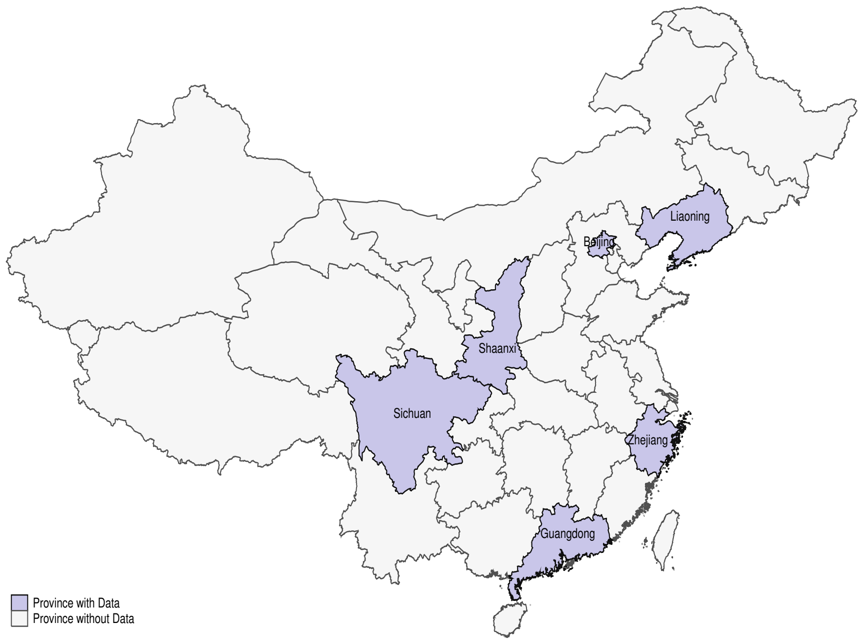


Figure A.2: College enrollment and college graduates

Notes. This figure plots levels and growth rates of college enrollment (during 1995-2015) and four-year lead college graduates (during 1995-2011). College enrollment and four-year lead college graduates are in million persons. The dashed green lines define the higher-education expansion during 1999-2005.

(A) Provinces Included in Benchmark Sample



(B) Counties Surveyed in Benchmark Sample, 2002

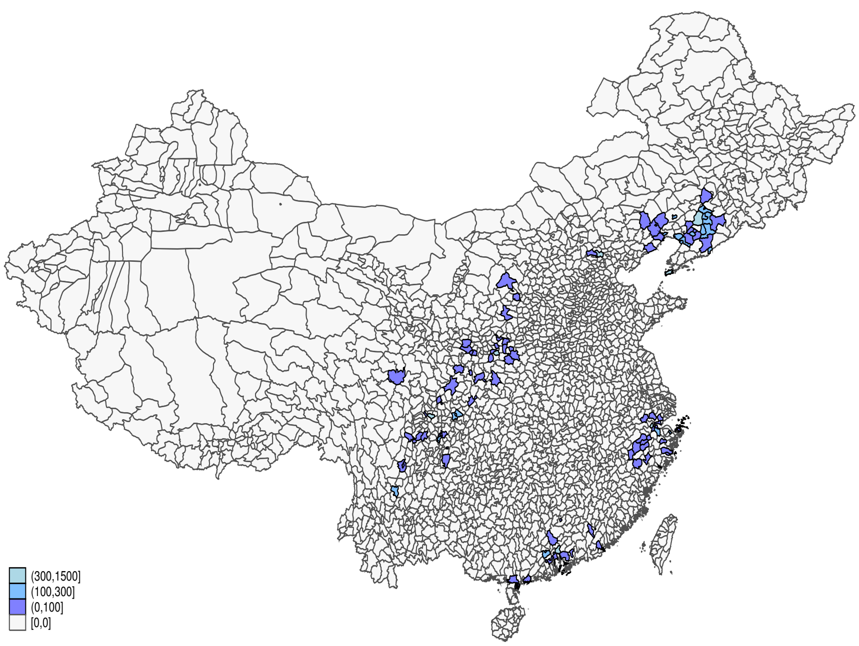
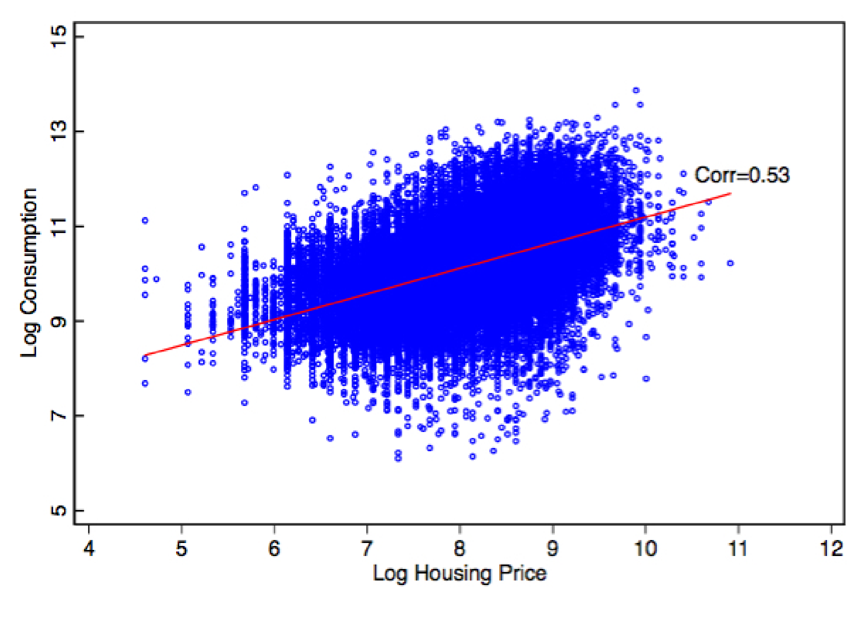


Figure A.3: Geographic span of benchmark sample from the UHS

Notes. This figure plots the geographical distribution of provinces/counties in our benchmark sample. In Panel (B), the change in colors reflect the change in the number of households within counties. The majority of counties have a sample size ranging between 50 and 100.

(A) Household-level Log Consumption and Housing Price



(B) Household-level Consumption and Home Value

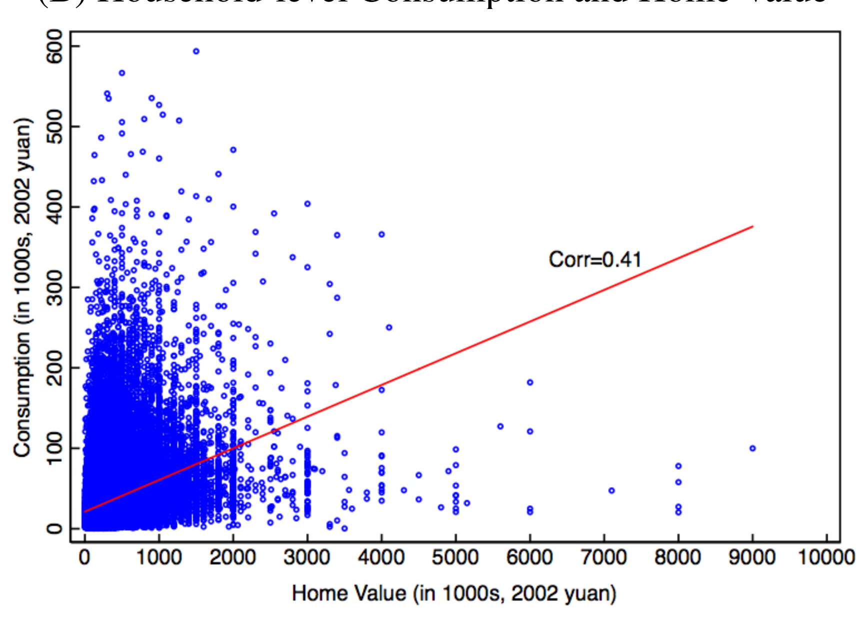


Figure A.4: Household-level correlation patterns: consumption and housing

Notes. This figure plots household-level correlation between consumption and housing variables. The blue solid line is the linear fitted line. "$Corr = 0.53$" and "$Corr = 0.41$" indicate a coefficient of correlation equal to 0.53 and 0.41, respectively.

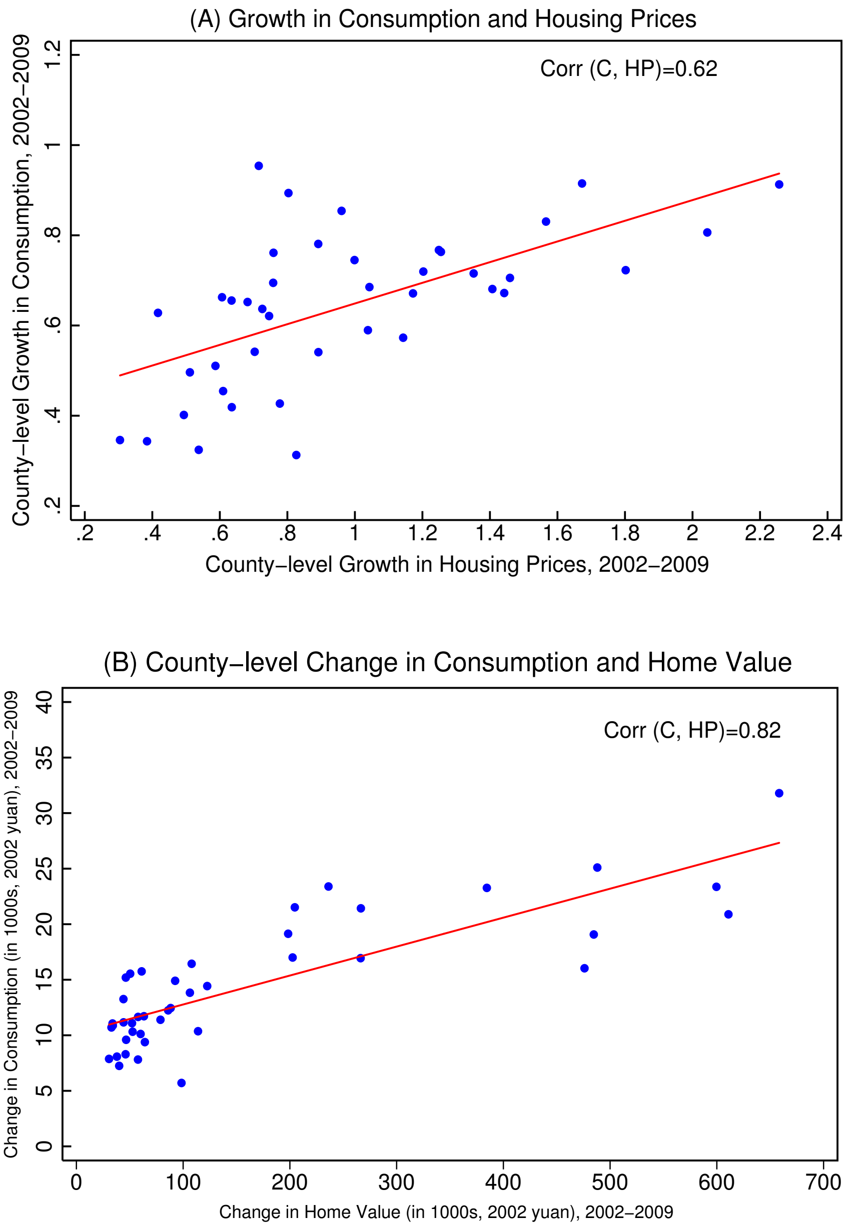


Figure A.5: County-level correlation patterns: consumption and housing

Notes. This figure plots county-level correlation between movements in consumption and housing variables. The blue solid line is the linear fitted line. "$Corr = 0.62$" and "$Corr = 0.82$" indicate a coefficient of correlation equal to 0.62 and 0.82, respectively.

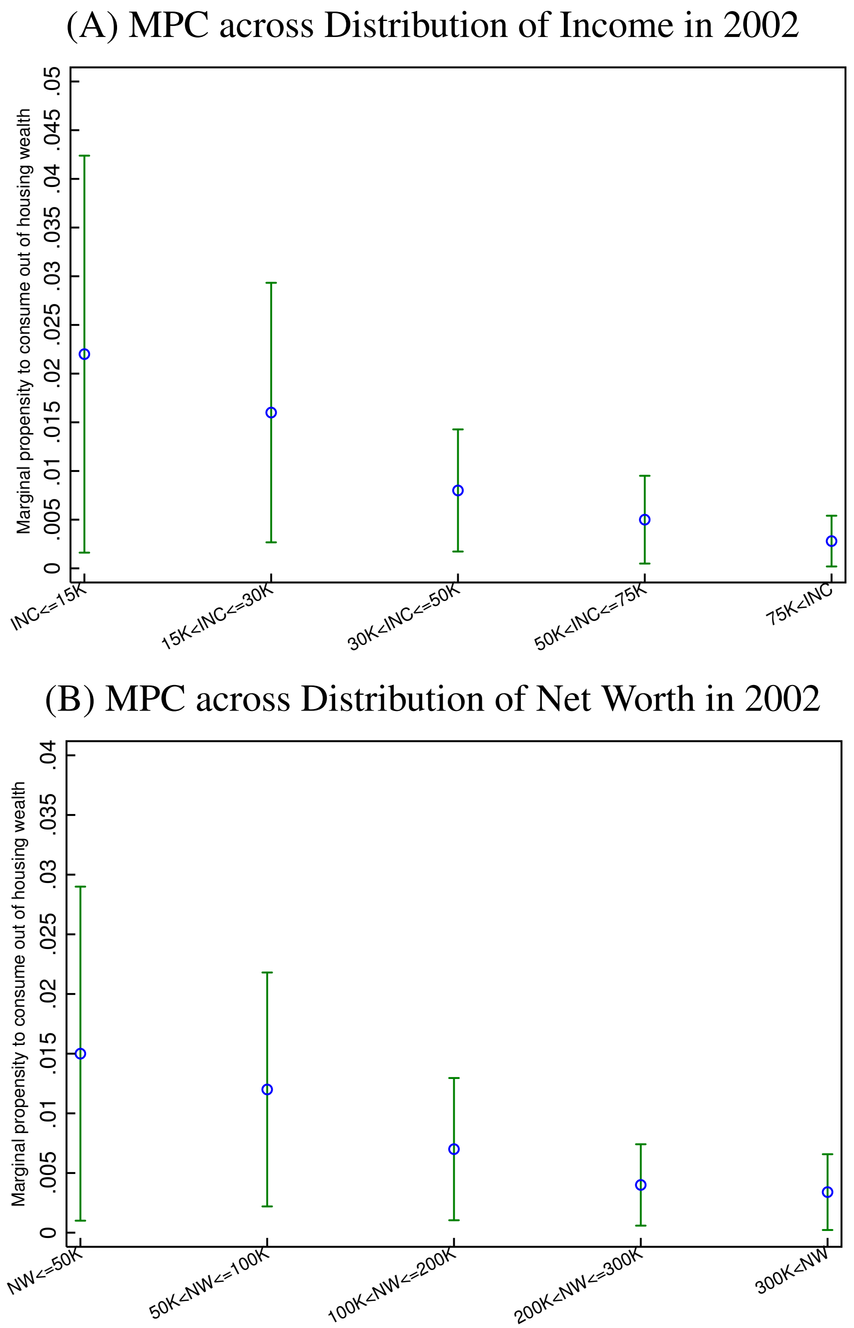


Figure A.6: Heterogeneity in MPC: income and wealth distribution

Notes. This figure plots the heterogeneity in MPC across income and wealth distribution in 2002. The distribution is divided into different intervals at the prefecture level to ensure a meaningful number of households for each interval. The blue hollow circle denotes the point estimates of MPC, and the green spikes define the 95% confidence interval. The unit for income and net worth is 2002 *yuan*. For instance, "$INC <= 15K$" means that real disposable income is no more than 15,000 *yuan* in 2002. All MPCs estimates are statistically significant at the 5% level.

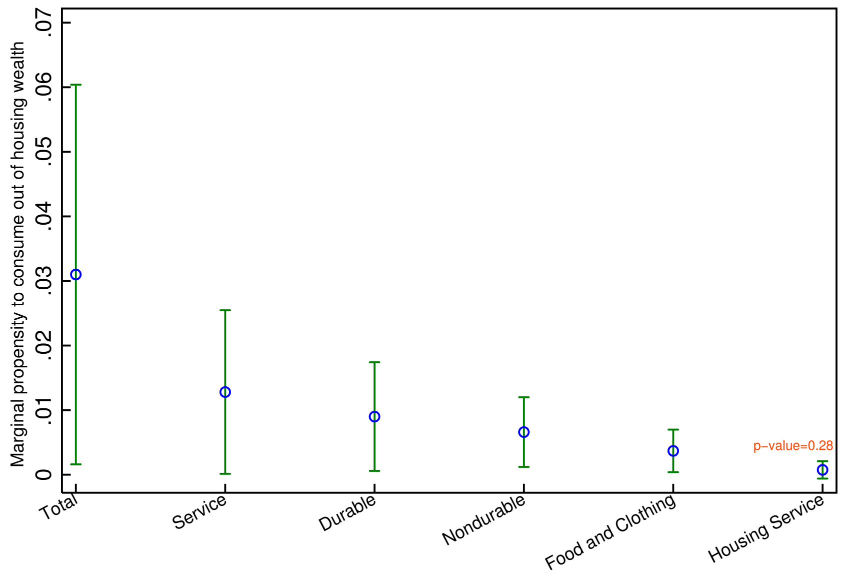


Figure A.7: MPC across various categories of consumption

Notes. This figure plots the heterogeneity in MPC across various categories of consumption. County-level regressions are implemented for each category. Total consumption and service consumption exclude consumption of housing service. Nondurable consumption includes consumption of food and clothing. The blue hollow circle denotes the point estimates of MPC, and the green spikes define the 95% confidence interval. All MPCs estimates, except for the one associated with housing service (which is statistically insignificant), are statistically significant at the 5% level.

A.1.2 **Table Appendix**

Table A.1: County-level summary statistics for benchmark UHS data

	Observations	Mean	Std. dev.	Median	IQ range
Housing net worth shock, 2002-9	42	0.526	0.534	0.514	0.569
Housing price shock, 2002-9	42	0.954	0.575	0.815	0.614
Change in home value (in 1000s, 2002 *yuan*), 2002-9	42	196.5	219.9	80.6	182.9
Total consumption growth, 2002-9	42	0.619	0.465	0.644	0.413
Change in total consumption (in 1000s, 2002 *yuan*), 2002-9	42	14.5	5.7	12.9	6.6
Change in nondurable consumption (in 1000s, 2002 *yuan*), 2002-9	42	6.4	2.0	6.0	2.5
Change in durable consumption (in 1000s, 2002 *yuan*), 2002-9	42	2.7	2.1	2.1	2.2
Change in service consumption (in 1000s, 2002 *yuan*), 2002-9	42	5.3	2.7	4.6	3.3
Employment share in SOE (%), 2002	42	53.2	16.0	55.0	19.4
Employment share in domestic private sector (%), 2002	42	38.3	14.4	37.2	21.4
Total disposable income per household (in 1000s, 2002 *yuan*), 2002	42	36.9	16.7	28.9	25.2
Net worth per household (in 1000s, 2002 *yuan*), 2002	42	252.0	219.5	138.0	233.7
Housing leverage ratio (%), 2002	42	14.3	37.1	5.8	21.7
Number of households	42	93.7	82.7	53.0	30.0

Notes. This table presents county-level summary statistics for our UHS data. The sample is restricted to 42 counties for which we have data in both 2002 and 2009. Housing net worth shock measures the growth in total net worth driven by the growth in house prices. Housing price shock is the growth of housing prices. SOE denotes the state-owned enterprises. Housing leverage ratio is the ratio between mortgage plus home equity line of credit (HELOC) and home value. All statistics have been weighted by county-level population size in 2002. IQ range is the interquartile range.

Table A.2: College enrollment expansion shock as a source of exogenous variation

Dependent variable (vertical)	College enrollment expansion shock (in 1000s)	Constant	N	R^2
(1) Housing net worth shock	0.041***	-1.596***	42	0.436
	[0.007]	[0.459]		
(2) Housing price shock	0.024***	-1.531***	42	0.597
	[0.003]	[0.335]		
(3) Change in home value, 2002-9	0.069***	-0.296***	42	0.743
(in 1000s, 2002 *yuan*)	[0.006]	[0.155]		
(4) Change in land sales share, 2002-9	0.023	0.853***	42	0.001
	[0.140]	[0.226]		
(5) Permanent shock to wage growth,	0.051	0.476	42	0.004
2002-9	[0.129]	[0.991]		
(6) Change in DPS employment share,	0.067	0.876***	42	0.001
2002-9	[0.593]	[0.310]		
(7) DPS employment share in 2002	0.112	1.297**	42	0.021
	[0.121]	[0.527]		
(8) Population growth, 2002-9	0.063***	-0.345	42	0.194
	[0.019]	[0.414]		
(9) Disposable income per household	0.030***	-1.504***	42	0.355
in 2002 (in 1000s, 2002 *yuan*)	[0.006]	[0.534]		
(10) Net worth per household in 2002	0.008**	-0.115	42	0.112
(in 1000s, 2002 *yuan*)	[0.003]	[0.442]		

Notes. This table presents coefficients from county-level univariate regressions which regress various dependent variables on college enrollment expansion shock. Each row represents a separate regression. The first three rows exhibit the first stage estimation of housing net worth shock, housing price shock, and change in home value on the instrumental variable, i.e. college enrollment expansion shock, respectively. Bootstrapped standard errors based on 1,000 repetitions are in parentheses. ***, ** indicates statistical significance at the 1% and 5% levels, respectively.

Table A.3: Household-level log housing price and log consumption

Explanatory variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(vertical)						(Home value)	(No villas)
Log housing price	0.046***	0.044***	0.053***	0.055***	0.053***	0.041***	0.052***
	[0.002]	[0.003]	[0.002]	[0.002]	[0.002]	[0.003]	[0.002]
Log non-housing net worth			0.042***	0.043***	0.039***	0.042***	0.037***
			[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Log disposable income				0.080***	0.078***	0.077***	0.079***
				[0.004]	[0.003]	[0.003]	[0.003]
Household characteristics	No	No	No	No	Yes	Yes	Yes
Home characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.28	0.30	0.32	0.47	0.57	0.56	0.57
N	113,771	96,570	96,456	96,420	96,336	96,336	95,928

Notes. This table presents coefficients from household-level regressions that regress log total consumption on log housing price and other controls. Household characteristics are household size and share of employment. Home characteristics are home age, size, architectural style, facilities and equipments. Column (6) replaces log housing price with log home value. Column (7) excludes households with homes valuing more than 2 million *yuan*. Heteroskedasticity robust standard errors clustered at the county level are in parentheses. *** indicates statistical significance at the 1% level.

Table A.4: Household-level home value and consumption

Explanatory variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(vertical)						(Housing price)	(No villas)
Home value	0.009***	0.007***	0.011***	0.014***	0.014***	0.015***	0.014***
(in 1000s, 2002 *yuan*)	[0.001]	[0.001]	[0.004]	[0.003]	[0.003]	[0.004]	[0.003]
Non-housing net worth			0.008***	0.006***	0.006***	0.008***	0.006***
(in 1000s, 2002 *yuan*)			[0.000]	[0.000]	[0.000]	[0.001]	[0.000]
Disposable income				0.057***	0.056***	0.053***	0.056***
(in 1000s, 2002 *yuan*)				[0.008]	[0.009]	[0.009]	[0.009]
Household characteristics	No	No	No	No	Yes	Yes	Yes
Home characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.17	0.18	0.18	0.27	0.45	0.44	0.46
N	113,771	96,570	96,456	96,420	96,336	96,336	95,928

Notes. This table presents coefficients from household-level regressions that regress total consumption (in 1000s, 2002 *yuan*) on home value and other controls. Household characteristics are household size and share of employment. Home characteristics are home age, size, architectural style, facilities and equipments. Column (6) replaces home value with housing price. Column (7) excludes households with homes valuing more than 2 million *yuan*. Heteroskedasticity robust standard errors clustered at the county level are in parentheses. *** indicates statistical significance at the 1% level.

Table A.5: County-level housing net worth/housing price shock and consumption growth

Explanatory variable	(1)	(2)	(3)	(4)	(5)	(6)
(vertical)	OLS	OLS	OLS	OLS	IV	IV
Housing net worth shock, 2002-9	0.134**	0.129**	-0.046	0.118**	0.149**	0.133**
	[0.063]	[0.062]	[0.032]	[0.054]	[0.066]	[0.064]
Housing price shock, 2002-9			0.093**			
			[0.038]			
Initial housing share in 2002			-0.027*			
			[0.014]			
Wage growth, 2002-9				0.088**		0.086**
				[0.043]		[0.039]
Population growth, 2002-9				0.227		0.232
				[0.223]		[0.205]
DPS employment share in 2002				0.155		0.163
				[0.121]		[0.117]
Log disposable income per household in 2002				-0.028		-0.021
				[0.027]		[0.032]
Log net worth per household in 2002				-0.084		-0.081
				[0.068]		[0.062]
Home characteristics	No	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.21	0.25	0.27	0.31	0.32	0.34
N	42	42	42	42	42	42

Notes. This table presents coefficients from county-level regressions that regress total consumption growth (2002-2009) on housing net worth shock, housing price shock, and other controls. Home characteristics are home age, size, architectural style, facilities and equipments. Column (5) and (6) instrument housing net worth shock with college enrollment expansion shock. Bootstrapped standard errors based on 1,000 repetitions are in parentheses. **, * indicates statistical significance at the 5% and 10% levels, respectively.

Table A.6: County-level average MPC out of housing wealth

Explanatory variable (vertical)	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) IV	(6) IV
Change in home value, 2002-9	0.029**	0.028**	0.035**	0.025**	0.032**	0.031**
(in 1000s, 2002 *yuan*)	[0.014]	[0.013]	[0.015]	[0.013]	[0.014]	[0.015]
(Change in home value, 2002-9)^2			-0.022**			
(in millions)			[0.011]			
DPS employment share in 2002				37.82		22.14
				[28.15]		[20.03]
Population in 2002				0.002*		0.001
(in 1000s)				[0.001]		[0.001]
Disposable income per household in 2002				0.025		0.065
(in 1000s, 2002 *yuan*)				[0.036]		[0.058]
Net worth per household in 2002				-0.008		-0.004
(in 1000s, 2002 *yuan*)				[0.013]		[0.013]
Home characteristics	No	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.36	0.43	0.47	0.55	0.56	0.60
N	42	42	42	42	42	42

Notes. This table presents coefficients from county-level regressions that regress change in consumption (2002-2009, in 1000s, 2002 *yuan*) on change in home value and other controls. Home characteristics are home age, size, architectural style, facilities and equipments. Column (5) and (6) instrument change in home value with college enrollment expansion shock. Bootstrapped standard errors based on 1,000 repetitions are in parentheses. **, * indicates statistical significance at the 5% and 10% levels, respectively.

Table A.7: County-level log housing price and household-level log consumption for renters

Explanatory variable (vertical)	(1)	(2)	(3)	(4)	(5)
County-level log housing price	0.023*	0.017*	0.006	0.001	-0.003
	[0.013]	[0.010]	[0.009]	[0.004]	[0.006]
Log non-housing net worth		0.029***	0.018***	0.012***	0.034**
		[0.006]	[0.004]	[0.003]	[0.012]
Log disposable income			0.098***	0.095***	0.073***
			[0.011]	[0.009]	[0.016]
Household characteristics	No	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE $\times$ County FE	No	No	No	No	Yes
R^2	0.07	0.31	0.38	0.43	0.46
N	10,524	10,397	10,393	9,794	9,794

Notes. This table presents coefficients from household-level regressions that regress log total consumption on county-lever log housing price and other controls. Households are renters rather than homeowners. Household characteristics are household size and share of employment. Robust standard errors clustered at the county level are in parentheses. ***, * indicates statistical significance at the 1% and 10% levels, respectively.

Table A.8: Heterogeneous average MPC out of housing wealth across housing leverage ratios

Explanatory variable	(1)	(2)	(3)	(4)	(5)	(6)
(vertical)	OLS	OLS	OLS	OLS	IV	IV
Home value	0.004***	0.003***				
(in 1000s, 2002 *yuan*)	[0.001]	[0.001]				
Change in home value, 2002-9			0.012**	0.009**	0.015**	0.013**
(in 1000s, 2002 *yuan*)			[0.005]	[0.004]	[0.005]	[0.005]
County-level housing leverage ratio, 2002	-5.184***	-5.002***	-4.133**	-3.602**	-4.585*	-4.238*
	[0.579]	[0.254]	[2.006]	[1.752]	[2.676]	[2.242]
(Home value) × (County-level housing leverage ratio in 2002)	0.034***	0.027***				
	[0.006]	[0.009]				
(Change in home value) × (County-level housing leverage ratio in 2002)			0.023**	0.013**	0.031**	0.019**
			[0.011]	[0.004]	[0.015]	[0.008]
Household-level controls	No	Yes	No	No	No	No
County-level controls	No	No	No	Yes	No	Yes
Home characteristics	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	No	No	No	No
County FE	Yes	Yes	No	No	No	No
Province FE	No	No	Yes	Yes	Yes	Yes
R^2	0.21	0.53	0.29	0.63	0.62	0.65
N	113,771	96,336	42	42	42	42

Notes. This table presents coefficients from household-level/county-level regressions that regress consumption/change in consumption on home value/change in home value (in 1000s, 2002 *yuan*), housing leverage ratio, and other controls. Household controls are non-housing wealth, disposable income, household size and share of employment. County controls include DPS employment share, population, disposable income per household, net worth per household. Home characteristics are home age, size, architectural style, facilities and equipments. In column (1) and (2), robust standard errors clustered at the county level are in parentheses. In column (3) and (4), bootstrapped standard errors based on 1,000 repetitions are in parentheses. Column (5) and (6) instrument change in home value with college enrollment expansion shock. ***, **, * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.9: Prefecture-level housing net worth shock and consumption growth

Explanatory variable	(1)	(2)	(3)	(4)	(5)	(6)
(vertical)	OLS	OLS	OLS	OLS	IV	IV
Housing net worth shock, 2002-9	0.120**	0.112**	-0.027	0.117**	0.126**	0.124**
	[0.057]	[0.052]	[0.020]	[0.059]	[0.056]	[0.057]
Housing price shock, 2002-9			0.064**			
			[0.028]			
Initial housing share in 2002			-0.106			
			[0.056]			
Wage growth, 2002-9				0.047***		0.039***
				[0.009]		[0.015]
Population growth, 2002-9				0.501		0.589
				[0.633]		[0.638]
DPS employment share in 2002				0.185		0.129
				[0.119]		[0.124]
Log disposable income per household in 2002				-0.020		-0.028
				[0.052]		[0.082]
Log net worth per household in 2002				-0.071		-0.069
				[0.072]		[0.117]
Home characteristics	No	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.29	0.38	0.41	0.52	0.63	0.65
N	59	59	59	59	59	59

Notes. This table presents coefficients from prefecture-level regressions that regress total consumption growth on housing net worth shock, housing price shock, and other controls. Home characteristics are home age, size, architectural style, facilities and equipments. Column (5) and (6) instrument housing net worth shock with college enrollment expansion shock. Bootstrapped standard errors based on 1,000 repetitions are in parentheses. ***, ** indicates statistical significance at the 1% and 5% levels, respectively.

Table A.10: Prefecture-level average marginal propensity to consume out of housing wealth

Explanatory variable (vertical)	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) IV	(6) IV
Change in home value, 2002-9	0.027**	0.023**	0.050**	0.031**	0.033**	0.032**
(in 1000s, 2002 *yuan*)	[0.012]	[0.010]	[0.021]	[0.013]	[0.016]	[0.015]
(Change in home value, 2002-9)^2			-0.053**			
(in millions)			[0.024]			
DPS employment share in 2002				31.25		20.87
				[36.79]		[13.19]
Population in 2002				0.002*		0.002**
(in 1000s)				[0.001]		[0.001]
Disposable income per household in 2002				0.012		0.036
(in 1000s, 2002 *yuan*)				[0.028]		[0.059]
Net worth per household in 2002				-0.007		-0.012
(in 1000s, 2002 *yuan*)				[0.006]		[0.018]
Home characteristics	No	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.44	0.53	0.59	0.65	0.66	0.72
N	59	59	59	59	59	59

Notes. This table presents coefficients from prefecture-level regressions that regress change in consumption (2002-2009, in 1000s, 2002 *yuan*) on change in home value and other controls. Home characteristics are home age, size, architectural style, facilities and equipments. Column (5) and (6) instrument change in home value with college enrollment expansion shock. Bootstrapped standard errors based on 1,000 repetitions are in parentheses. **, * indicates statistical significance at the 5% and 10% levels, respectively.

Table A.11: EIV estimation for county-level housing net worth shock and consumption growth

Explanatory variable	(1)	(2)	(3)	(4)
(vertical)	OLS+EIV	OLS+EIV	IV+EIV	IV+EIV
Housing net worth shock, 2002-9	0.142**	0.126**	0.157**	0.153**
	[0.072]	[0.059]	[0.078]	[0.079]
Wage growth, 2002-9		0.073**		0.098**
		[0.034]		[0.041]
Population growth, 2002-9		0.198		0.267
		[0.347]		[0.411]
DPS employment share in 2002		0.139		0.188
		[0.126]		[0.278]
Log disposable income per household in 2002		-0.044		-0.059
		[0.064]		[0.106]
Log net worth per household in 2002		-0.069		-0.093
		[0.080]		[0.114]
Home characteristics	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
R^2	0.28	0.34	0.32	0.37
N	42	42	42	42

Notes. This table presents coefficients from county-level error-in-variable regressions that regress total consumption growth on housing net worth shock and other controls. Home characteristics are home age, size, architectural style, facilities and equipments. Column (3) and (4) instrument housing net worth shock with college enrollment expansion shock. Bootstrapped standard errors based on 1,000 repetitions are in parentheses. ** indicates statistical significance at the 5% level.

Table A.12: EIV estimation for county-level average MPC out of housing wealth

Explanatory variable	(1)	(2)	(3)	(4)
(vertical)	OLS+EIV	OLS+EIV	IV+EIV	IV+EIV
Change in home value, 2002-9	0.034**	0.031**	0.038**	0.035**
(in 1000s, 2002 *yuan*)	[0.014]	[0.013]	[0.015]	[0.016]
DPS employment share in 2002		47.58		28.48
		[61.22]		[32.65]
Population in 2002		0.004*		0.003
(in 1000s)		[0.003]		[0.004]
Disposable income per household in 2002		0.055		0.075
(in 1000s, 2002 *yuan*)		[0.047]		[0.066]
Net worth per household in 2002		-0.004		-0.005
(in 1000s, 2002 *yuan*)		[0.017]		[0.021]
Home characteristics	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
R^2	0.41	0.57	0.63	0.70
N	42	42	42	42

Notes. This table presents coefficients from county-level error-in-variable regressions that regress change in consumption (in 1000s, 2002 *yuan*) on change in home value and other controls. Home characteristics are home age, size, architectural style, facilities and equipments. Column (3) and (4) instrument change in home value with college enrollment expansion shock. Bootstrapped standard errors based on 1,000 repetitions are in parentheses. **, * indicates statistical significance at the 5% and 10% levels, respectively.

Table A.13: Prefecture-level elasticity estimation with alternative housing price

Explanatory variable	(1)	(2)	(3)	(4)	(5)	(6)
(vertical)	OLS	OLS	OLS	OLS	IV	IV
Housing net worth shock, 2002-9	0.132**	0.128**	-0.036	0.130**	0.138**	0.133**
	[0.059]	[0.064]	[0.057]	[0.062]	[0.070]	[0.066]
Housing price shock, 2002-9			0.154**			
			[0.076]			
Initial housing share in 2002			-0.159			
			[0.765]			
Wage growth, 2002-9				0.039***		0.032***
				[0.011]		[0.015]
Population growth, 2002-9				0.198		0.309
				[0.503]		[0.533]
DPS employment share in 2002				0.173		0.185
				[0.181]		[0.188]
Log disposable income per household in 2002				-0.008		-0.023
				[0.009]		[0.047]
Log net worth per household in 2002				-0.129		-0.102
				[0.098]		[0.128]
Home characteristics	No	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.14	0.23	0.25	0.31	0.32	0.38
N	34	34	34	34	34	34

Notes. This table presents coefficients from prefecture-level regressions that regress total consumption growth on housing net worth shock, housing price shock, and other controls. When constructing housing net worth shock, we compute housing price growth from the hedonic housing price index constructed by Fang et al. (2015). Home characteristics are home age, size, architectural style, facilities and equipments. Column (5) and (6) instrument housing net worth shock with college enrollment expansion shock. Bootstrapped standard errors based on 1,000 repetitions are in parentheses. ***, ** indicates statistical significance at the 1% and 5% levels, respectively.

Table A.14: County-level elasticity estimation with panel regression

Explanatory variable (vertical)	(1) FE	(2) FE	(3) EIV	(4) EIV	(5) DPD	(6) DPD
Housing net worth shock	0.087***	0.069***	0.093**	0.119**	0.076**	0.062**
	[0.026]	[0.025]	[0.042]	[0.056]	[0.033]	[0.025]
Lagged consumption growth					0.332***	0.227**
					[0.084]	[0.108]
County controls	No	Yes	No	Yes	No	Yes
Home characteristics	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
N	294	294	294	294	210	210

Notes. This table presents coefficients from county-level static and dynamic panel regressions that regress total consumption growth on housing net worth shock and other controls. County controls include wage growth, population growth, DPS employment share, log disposable income per household, and log net worth per household. Home characteristics are home age, size, architectural style, facilities and equipments. Column (1) and (2) employ the standard fixed-effect estimation for panel data. Column (3) and (4) adjust the estimation for error in variables. Column (5) and (6) introduce habit formation in consumption and utilize the Arellano-Bond estimation for dynamic panel data models. Robust standard errors clustered at the province level are in parentheses. ***, ** indicates statistical significance at the 1% and 5% levels, respectively.

Table A.15: County-level average MPC out of housing wealth with panel regression

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)
(vertical)	FE	FE	EIV	EIV	DPD	DPD
Change in home value	0.019***	0.022***	0.035**	0.031**	0.014**	0.017**
(in 1000s, 2002 *yuan*)	[0.004]	[0.007]	[0.016]	[0.014]	[0.006]	[0.008]
Lagged change in consumption					0.362***	0.297***
(in 1000s, 2002 *yuan*)					[0.105]	[0.079]
County controls	No	Yes	No	Yes	No	Yes
Home characteristics	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
N	294	294	294	294	210	210

Notes. This table presents county-level static and dynamic panel regressions that regress change in consumption (in 1000s, 2002 *yuan*) on change in home value and other controls. County controls include DPS employment share, population, disposable income per household, net worth per household. Home characteristics are home age, size, architectural style, facilities and equipments. Column (1) and (2) employ the standard fixed-effect estimation for panel data. Column (3) and (4) adjust the estimation for error in variables. Column (5) and (6) introduce habit formation in consumption and utilize the Arellano-Bond estimation for dynamic panel data models. Robust standard errors clustered at the province level are in parentheses. ***, ** indicates statistical significance at the 1% and 5% levels, respectively.

A.2 Appendices for Chapter 2

A.2.1 Figure Appendix

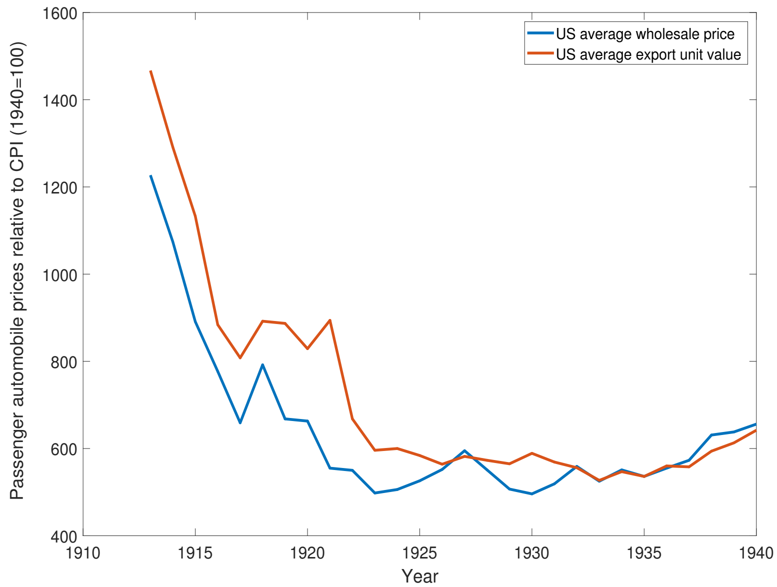


Figure A.8: Domestic prices and export unit values of U.S. passenger automobiles

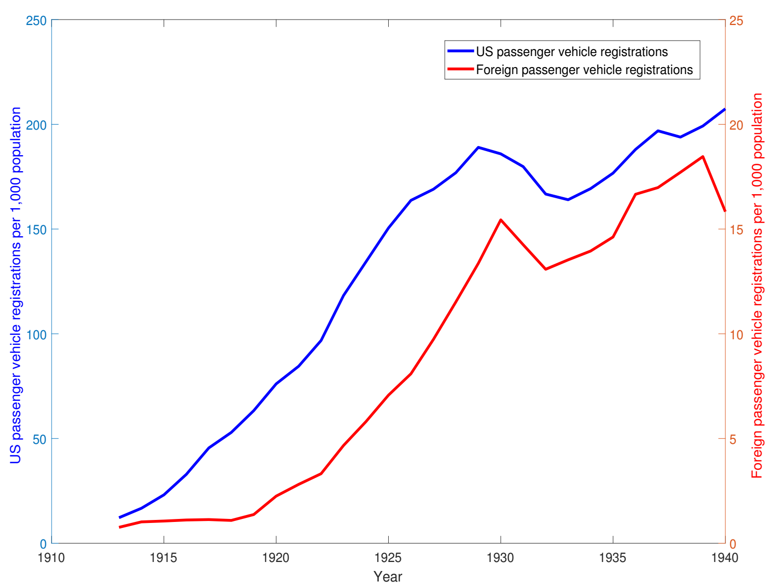


Figure A.9: U.S.and rest-of-the-world passenger automobile registrations per 1,000 persons

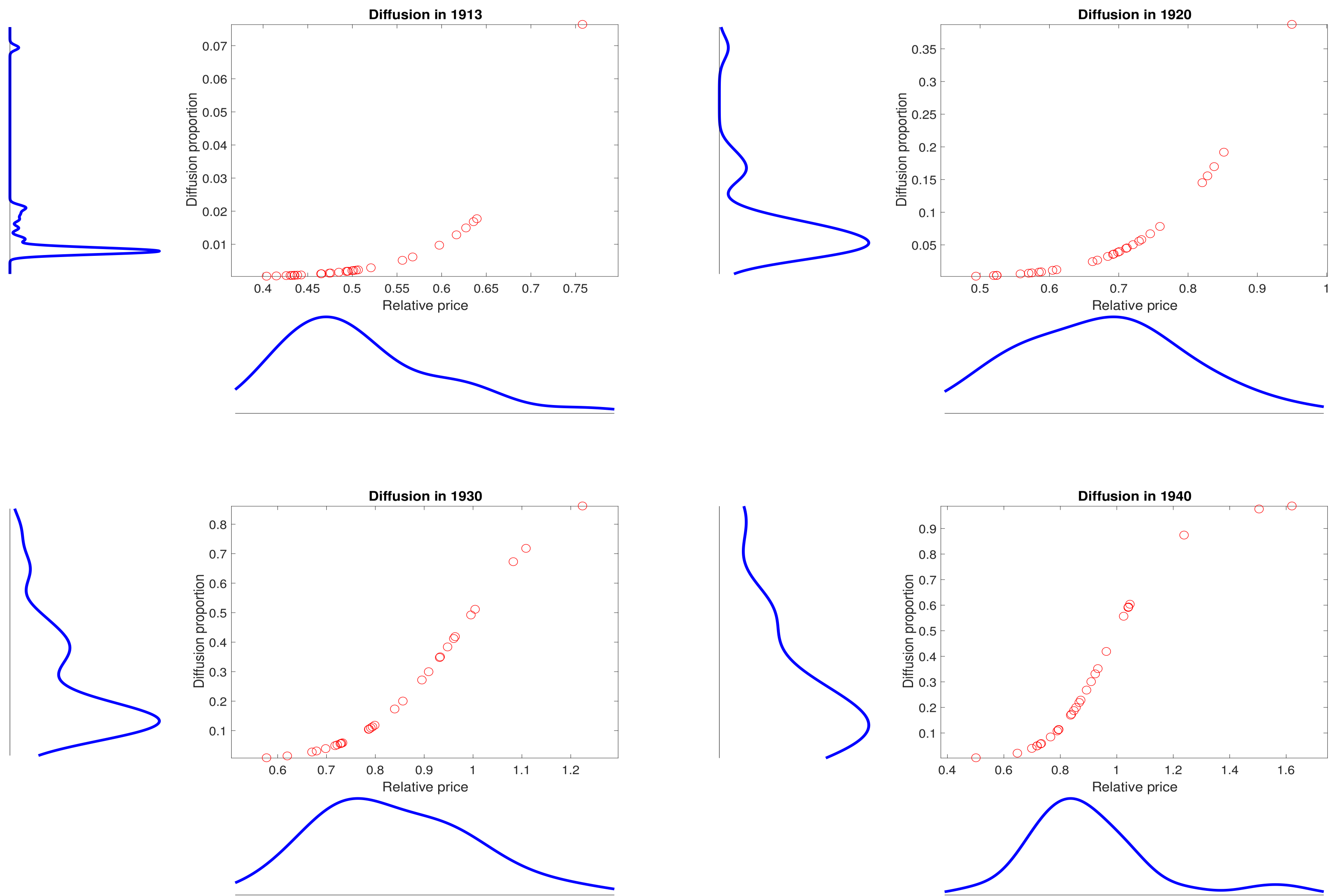


Figure A.10: Simulated diffusion using U.S. export unit values

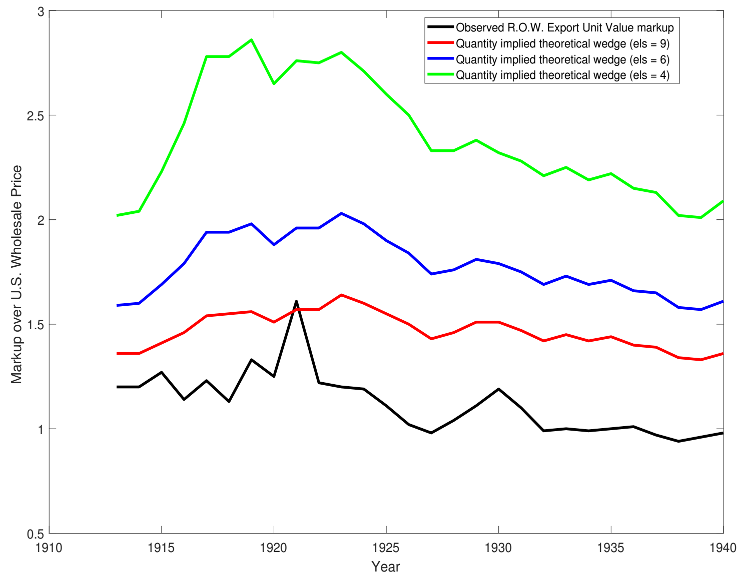


Figure A.11: Theoretical wedges and markup of U.S. EUV over U.S. domestic prices

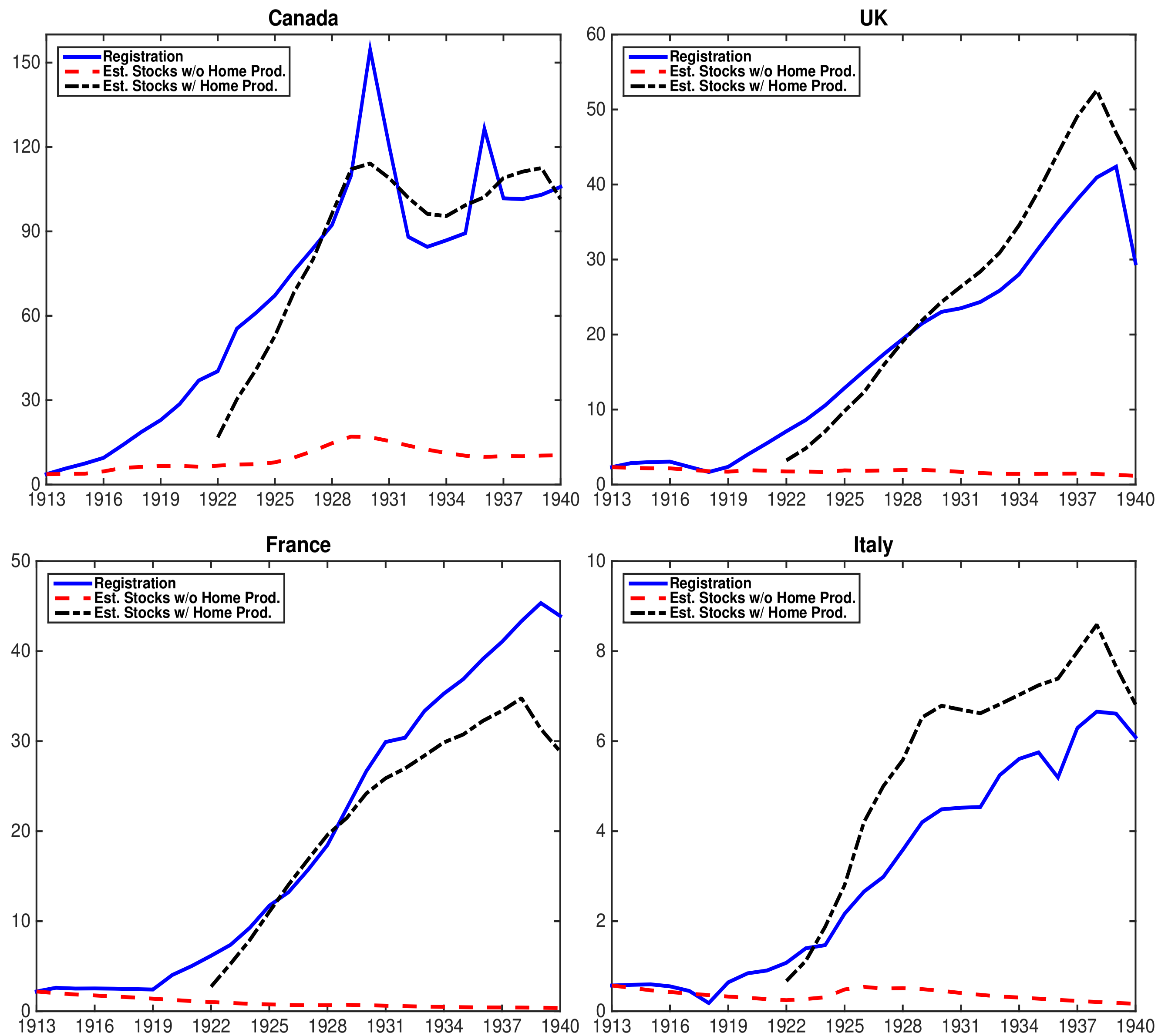


Figure A.12: Automobile stock estimates and their components

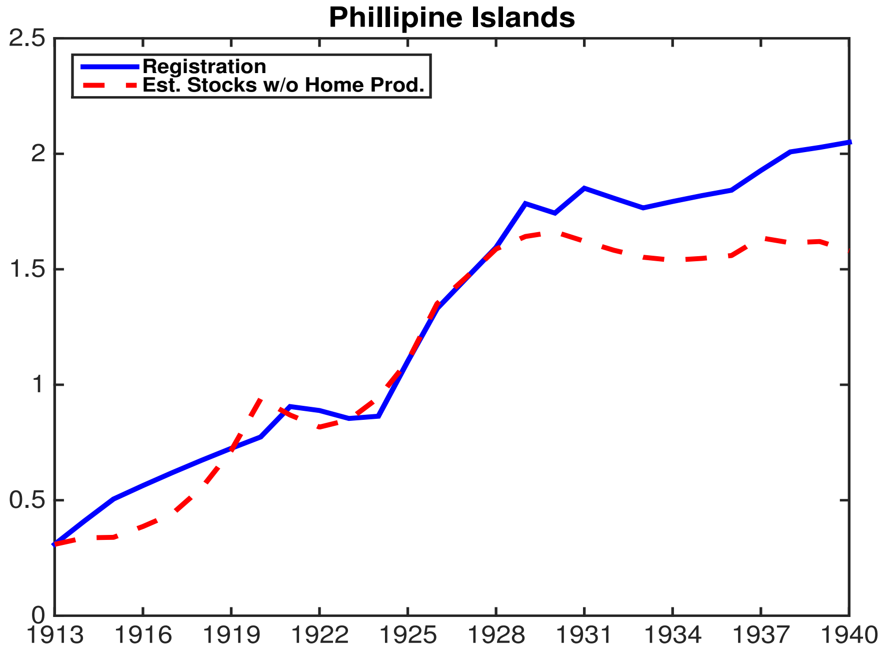


Figure A.13: Philippine Islands automobile stock estimates

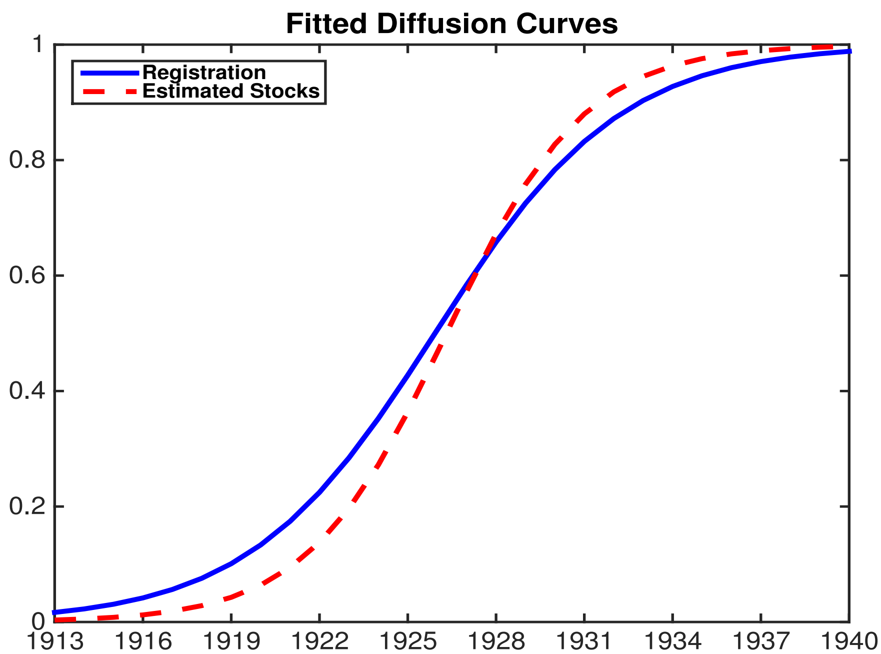


Figure A.14: Estimated diffusion using CHAT registration measure and stock-flow estimate

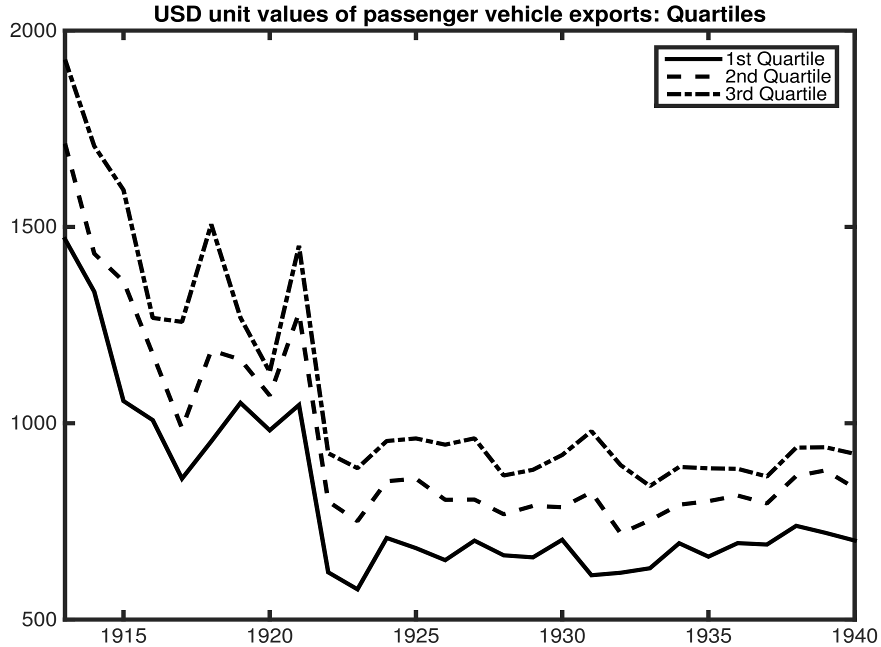


Figure A.15: Distribution of EUV deflated by the U.S. CPI (1940=100)

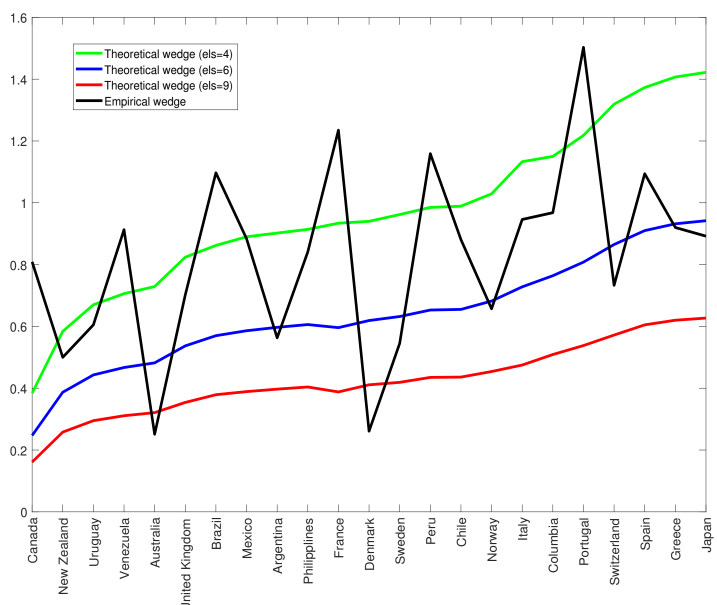


Figure A.16: Cross-country distribution of theoretical and empirical wedges

A.2.2 **Table Appendix**

Table A.16: Estimated rest-of-the-world diffusion curves with logistic function

Measure	Long-run $\widehat{\alpha}$	Phase shift $\widehat{\tau}$	Diffusion rate $\widehat{\beta}$	R^2
Registrations	14.5	6.08	0.425	0.99
	(0.447)	(0.365)	(0.030)	
Stock-flow	16.9	4.49	0.316	0.99
	(0.605)	(0.307)	(0.029)	

Notes. Estimation by NLS. Standard errors are in parentheses.

Table A.17: Estimated diffusion curves by country with logistic function

Country	$\widehat{\alpha}$	$\widehat{\tau}$	$\widehat{\beta}$	R^2	Country	$\widehat{\alpha}$	$\widehat{\tau}$	$\widehat{\beta}$	R^2
Argentina	11.79	11.54	0.897	0.97	NZL	26.25	3.34	0.376	0.98
	(0.46)	(1.92)	(0.152)			(1.01)	(0.40)	(0.053)	
Australia	28.29	11.69	1.009	0.97	Norway	3.42	4.34	0.572	0.99
	(1.15)	(1.70)	(0.151)			(0.11)	(0.46)	(0.073)	
Brazil	1.93	10.43	0.833	0.97	Peru	1.65	3.39	0.281	0.98
	(0.08)	(2.33)	(0.185)			(0.09)	(0.35)	(0.036)	
Canada	114.10	8.11	0.623	1.00	Philippines	1.88	3.01	0.285	0.99
	(2.15)	(0.65)	(0.053)			(0.02)	(0.22)	(0.021)	
Chile	3.07	3.08	0.323	0.95	Portugal	1.00	5.41	0.400	0.99
	(0.17)	(0.66)	(0.073)			(0.02)	(0.76)	(0.060)	
Colombia	2.06	3.23	0.163	0.96	Spain	1.33	8.04	0.733	0.92
	(0.49)	(0.23)	(0.033)			(0.11)	(1.73)	(0.169)	
Denmark	12.05	14.06	1.037	0.98	Sweden	17.47	4.36	0.228	0.99
	(0.47)	(1.98)	(0.151)			(1.56)	(0.26)	(0.024)	
France	31.88	6.41	0.425	1.00	Switzerland	4.48	7.03	0.452	1.00
	(1.00)	(0.44)	(0.033)			(0.09)	(0.46)	(0.031)	
Greece	0.72	7.34	0.589	0.93	UK	52.96	5.37	0.285	0.99
	(0.05)	(2.33)	(0.200)			(4.71)	(0.37)	(0.030)	
Italy	7.66	8.05	0.570	0.99	Uruguay	14.19	5.25	0.525	0.95
	(0.22)	(0.51)	(0.040)			(0.90)	(0.88)	(0.094)	
Japan	2.40	4.67	0.177	0.98	Venezuela	6.41	3.90	0.235	0.99
	(1.14)	(0.23)	(0.029)			(0.43)	(0.25)	(0.024)	
Mexico	3.46	6.08	0.575	0.99	**Median**	4.48	5.41	0.452	0.98
	(0.090	(0.38)	(0.040)			(0.43)	(0.46)	(0.053)	

Notes. Estimation by NLS. Standard errors are in parentheses.

Table A.18: Estimated inflection points and long-run adoption levels

Country	Inflection year	LR Adoption Level
Norway	1921	3
New Zealand	1922	26
Chile, Uruguay	1923	3, 14
Mexico, Philippine Islands, Spain	1924	4, 2, 1
Australia, Greece, Peru	1925	28, 1, 2
Argentina, Brazil, Canada	1926	12, 2, 114
Denmark, Italy, Portugal	1927	12, 8, 1
France	1928	32
Switzerland	1929	5
Venezuela	1930	6
Sweden, United Kingdom	1932	18, 53
Colombia	1933	2
Japan	1939	2
Median	**1926**	**4.5**
United States	**1922**	**200+**

Table A.19: Reduced form price dynamics

Country	$\widehat{\mu}_j$	$\widehat{\delta}_j$	Country	$\widehat{\mu}_j$	$\widehat{\delta}_j$
Argentina	1.04	0.2	Mexico	1.39	0.3
Australia	1.01	0.2	Netherlands	1.08	0.1
Austria	1.59	2.2	New Zealand	1.10	0.2
Belgium	1.07	0.2	Norway	1.01	0.1
Brazil	1.20	0.4	Peru	1.01	0.1
Canada	1.39	0.3	Philippine Islands	1.33	0.3
Chile	1.48	0.5	Portugal	1.11	0.1
Colombia	1.53	0.6	Spain	1.77	2.7
Denmark	1.02	0.2	Sweden	1.01	0.1
Finland	1.51	0.8	Switzerland	1.05	0.1
France	1.97	3.3	United Kingdom	1.01	0.1
Germany	1.19	0.1	Uruguay	1.19	0.4
Greece	1.07	0.1	Venezuela	1.30	0.5
Italy	1.47	1.0	**Mean**	**1.25**	**0.6**
Japan	1.14	0.2	**Median**	**1.17**	**0.2**

Notes. $Q^A_{j,t} = (\overline{Q}\exp(-\delta_j t) + \underline{Q})\mu_j + \varepsilon_{j,t}$.

Table A.20: Ad valorem equivalent tariffs on passenger automobiles

Country	Average	1920	1921
Argentina	0.320	0.320	0.320
Australia	0.322	0.100	0.543
Brazil	0.207	0.158	0.257
Canada	0.388	0.425	0.350
Chile	0.239	0.238	0.239
Colombia	0.012	0.012	0.011
Denmark	0.076	0.029	0.123
Finland	0.450	0.450	0.450
Greece	0.120	0.090	0.150
Italy	0.373	0.154	0.591
Japan	0.425	0.350	0.500
Mexico	0.179	0.000	0.358
New Zealand	0.200	0.200	0.200
Norway	0.150	0.120	0.180
Peru	0.246	0.110	0.382
Philippine Islands	0.000	0.000	0.000
Portugal	0.768	0.196	1.339
Spain	0.470	0.077	0.863
Sweden	0.150	0.150	0.150
Switzerland	0.084	0.084	0.084
United Kingdom	0.333	0.333	0.333
Uruguay	0.265	0.240	0.290
Venezuela	0.017	0.017	0.016
Mean	**0.252**	**0.168**	**0.336**
Variance	**0.033**	**0.018**	**0.091**

Table A.21: Theoretical and empirical wedges and their components with elasticity = 4

	Theory wedge	Emprical wedge	Markup	Tariff	Penn Effect	Trade Costs	Residual
Cross-country mean	0.97	0.82	0.17	0.21	0.29	0.15	0.15
Ratios	1.00	0.85	0.17	0.22	0.30	0.15	0.15
Ratios		1.00	0.20	0.26	0.36	0.18	
Cross-country std. dev.	0.27	0.30	0.16	0.14	0.20	0.00	0.31
Var. decomposition	1.00	0.43	0.20	0.05	0.18	0.00	0.57
Var. decomposition		1.00	0.38	0.19	0.43	0.00	
Cross-country correlation	1.00	0.39	0.33	0.10	0.24	0.00	0.48

A.2.3 **Data Appendix**

There are three distinct sets of data used in our analysis. The trade data come from the U.S. Foreign Commerce and Navigation of the United States, an annual serial volume that records line item imports and exports. From this volume we collected export quantities and value exported by destination market. The macroeconomic data consists of GDP, aggregate consumption, exchange rates and price indices. The manufacturing units values and other automobile production data are from the Census of US Manufacturers. We provide some details on each source below.

A.2.3.1 **Trade Data**

The automobile export panel spans the period 1913 to 1940 and records the value and number of passenger vehicles exported to as many as 81 destination markets. While the source does not disaggregate by make and model, it does break the passenger vehicle exports into price ranges. Thus, we have some confidence that unit values, computed as the ratio of value to quantity exported is a reasonably accurate estimate of destination unit prices of comparable passenger vehicles. The data appendix describes the original source data and how the panels used in subsequent analysis were constructed. Canada is the top export destination with about 42,000 units in 1929 valued at almost 34 million USD ($809 per vehicle). As surprising as it may seem for a small country to be such an important destination for US exports it is not alone: the five next largest destination markets are Australia, Argentina, Brazil and Mexico. The United Kingdom is the top destination among the largest industrialized nations, but despite being almost 10 times the size of Mexico, it accounts for a smaller export volume.

A.2.3.2 **Macroeconomic Data**

The passenger vehicle data are supplemented with the annual per capita GDP and population data complied by Angus Maddison. The per capita income data are presented in common base period units (1990 International Geary-Khamis dollars). In order to have a consistency between the trade data and the income data, we normalize the unit prices and values of U.S. automobile exports by the U.S. CPI obtained from the Bureau of Labor Statistics.

A.2.3.3 **Census of Manufacturers Data**

The Census of Manufacturing data was originally collected by Bresnahan and Raff (1991).[44] These plant-level data are available at two year-intervals: 1929, 1931, 1933 and 1935. The two series produce comparable markups of export unit values over domestic selling prices for the years in which both are available. One advantage of the Census data is the availability of data by make and model.

[44] These data were further augmented by Lee (2016) and incorporated into an archive of similar data spanning a selection of U.S. industries being developed by Vickers and Ziebarth (2017).

A.2.4 **Estimation Appendix**

A.2.4.1 **Trade Costs**

Two estimates of trade costs were considered. The first was to draw from the gravity literature relating distance and trade costs along with our Euler equation approach. The second was to draw on available contemporary measures of actual freight and insurance costs for automobiles. We discuss each one in turn.

One of the most robust empirical relationships in the trade literature is the fact the bilateral trade between bilateral pairs of nations is declining in the distance between them. This 'gravity' model of trade also has argued that the volume of bilateral trade is roughly proportional to the product of output. As our emphasis is on the stock of automobiles with trade facilitating the accumulation, the focus in this section is the distance. Unlike the contemporary period when most conventional tariff barriers have been eliminated (at least between large industrialized nations and members of various bilateral and multilateral trade agreements), tariffs on automobiles (and other products) were high, differed across countries and varied substantially over time.

Trade costs are estimates in two steps. First, a standard gravity equation is estimated by regressing the logarithm of the ratio $\widetilde{\omega}_{j,t}/\widetilde{\omega}_{u,t}$ (the inverse of the ratio of our Euler equation) on a constant and the logarithm of distance (between capital cities):

$$z_{jt} = \ln \widetilde{\omega}_{j,t} - \ln \widetilde{\omega}_{u,t} = \underset{(0.73)}{0.52} - \underset{(0.08)}{0.44} \ln(d_j) + \varepsilon_{j,t} \ .$$

Note that all of the time series variation is attributed to the residual as income effects are assumed to be proportional to the stock and distance is time invariant. The coefficient on distance is -0.44, which is quite close to the average reported in the meta analysis of the gravity literature (Disdier and Head, 2008).

As is well-known from the theoretical gravity literature, the reduced form coefficient on distance is the product of a substitution elasticity and the slope coefficient linking distance to trade costs (typically: $\ln(1+\tau) = \beta \ln d_j$). To disentangle these two margins, we substitute the predictions of the gravity equation for quantities, $\widehat{\omega}_{u,t}/\widehat{\omega}_{j,t}$ $=\exp(-\widehat{z}_{jt}) = \exp(-\widehat{\alpha} - \widehat{\theta}\ln(d_j))$ into our Euler equation with the U.S. domestic relative price set to unity, to generate a cross-section of trade cost wedges.

$$(1+\widehat{\tau}_{j,t}) = \left(2(\frac{\omega_{u,t}}{\omega_{j,t}}) - 1\right)^{\frac{1}{\sigma}} \ .$$

Canada is a geographic outlier given its proximity to the United States and, not surprisingly is predicted to have the lowest trade cost wedge (1.37) by far. The cross–country median trade cost wedge is 1.55, Australia and New Zealand are at the top end of the distribution, 1.62.

Hummels (2001) is a seminal contribution to contemporary estimation of trade costs, tariffs. The paper also contains industry level import demand elasticity estimation which we discuss in the section on demand elasticity estimation.

A.2.5 **Historical Appendix**

The Ford Motor Company was established in 1903 and plays a central role in the domestic growth and globalization of this budding industry. The industry had humble beginnings competing with the horse and carriage for short-haul trips by passengers and with limited cargo. Production numbers of the horse and carriage are difficult to come by, but the number of carriage companies operating in the United States fell from about 4,600 in 1914 to 150 in 1925 and then to less than 90 by 1929. Keeping in mind that aggregate real output grew quickly during this period, the decline is remarkable and it seems reasonable to attribute it to the rapid diffusion of the automobile as a replacement for the horse and carriage.

In 1907, Ford production had reached a meagre 14,887 units and the next largest manufacturer, Buick, produced just 4,641. While Ford's factories were initially assembly plants with chassis and running gears supplied from the Dodge brothers and bodies from the C.R. Wilson Carriage Company, by 1907 Ford was making almost all major components (except tires which were supplied by Firestone). In other words, Ford decided to *in-source* his inputs. This decision gave Ford more control over the development in his production process and was likely instrumental in facilitating a number of complementary technological changes that propelled Ford and the United States automobile industry to world-wide dominance.

The rapid expansion specific to the Ford Company was in part due to Ford's insistence that the company focus on a four cylinder model with very little in the way of product variety, lending the process to assembly line production. Ford believed this would lead both to greater production efficiency and a mass market. His insistence on this focus and the desire of partners to move into the six cylinder market, lead to the dissolution of earlier (i.e., pre-1903) partnerships. Ford dropped the six cylinder model in 1907 and would not produce another one until 1941 (except through the acquisition of Lincoln in 1922). In October of 1907, Ford introduced the famous Model T and it would dominate his production plans and world markets for the next two decades.

In 1914 Henry Ford introduced a full assembly-line production of the Model T chassis, reducing the hours required for production from 12.5 to 1.5, a factor of more than 8 (Baldwin et al., 1987). This was a timely decision on the eve of World War I, placing the company in a position to satisfy war-time production demands along with the capacity to supply vehicles to the increasingly prosperous citizenry in the United States as Europe struggled to recover from the devastation of the war. Foreign producers are much more modest in production scale compared to Ford, which thus created a large world market into which Ford quickly expanded as one of the first truly multinational U.S. companies. By 1920 it is estimated that half of the stock of automobiles in the world were Model Ts. By 1929, Ford production peaked at 1.7 million units, more than 100-fold increase from 1907. And then, of course, the industry would come to be dominated by the veracity of Great Depression.[45]

According to Wikipedia (https://en.wikipedia.org/wiki/Ford_Model_T), the Ford model runs and descriptions of products are as follows:

Model T1 (1909-1914) Characterized by a nearly straight, five-sided hood, with a flat top containing a center hinge and two side sloping sections containing the folding hinges. The firewall was flat from the windshield down with no distinct cowl.

Model T2 (1915-1916) The hood design was nearly the same five sided design with the only obvious change being the addition of louvers to the vertical sides. There was a significant change to the cowl area with the windshield relocated significantly behind the firewall and joined with a compound contoured cowl panel.

Model T3 (1917–1923) The hood design was changed to a tapered design with a curved top. the folding hinges were now located at the joint between the flat sides and the curved top. This is sometime referred to as the low hood to distinguish if from the later hoods. The back edge of the hood now met the front edge of the cowl panel so that no part of the flat firewall was visible outside of the hood. This design was used the longest and during the highest production years accounting for about half of the total number of Model T's built.

[45] It is surprising that the macroeconomics literature has not paid more attention to this industry. Check for references beyond the broad-brushed durable goods story (which were probably much less concentrated in production).

Model T4 (1923–1925) This change was made during the 1923 calendar year so models built earlier in the year have the older design while later vehicles have the newer design. The taper of the hood was increased and the rear section at the firewall is about an inch taller and several inches wider than the previous design. While this is a relatively minor change, the parts between the third and fourth generation are not interchangeable.

Model T5 (1926–1927) This design change made the greatest difference in the appearance of the car. The hood was again enlarged with the cowl panel no longer a compound curve and blended much more with the line of the hood. The distance between the firewall and the windshield was also increased significantly. This style is sometimes referred to as the high hood.

A.3 **Appendices for Chapter 3**

A.3.1 **Figure Appendix**

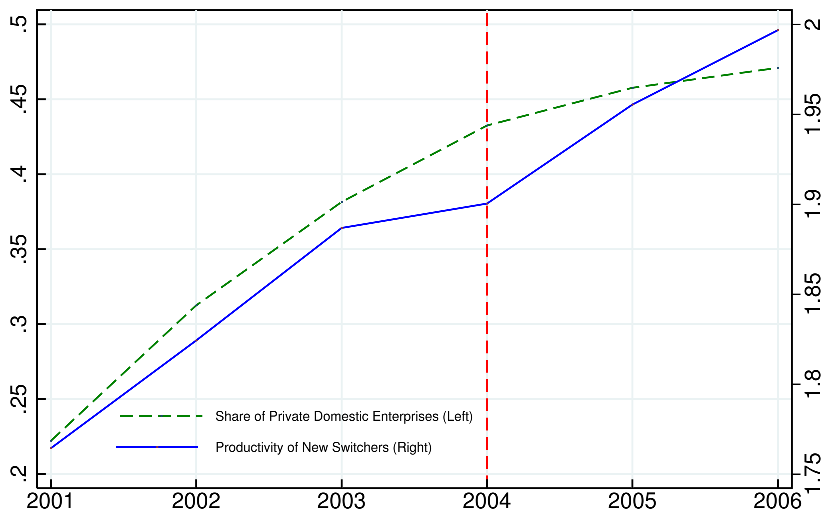


Figure A.17: Share of PDEs and average productivity of new switchers

Notes. Share of PDEs is the share of PDEs direct exporters in the pool of all direct exporters. Productivity of new switchers is the average productivity of firms that newly switch from indirect to direct exporting. The red dash line confines the period when China fully lifted its regulation on direct exporting rights, that is, 2004.

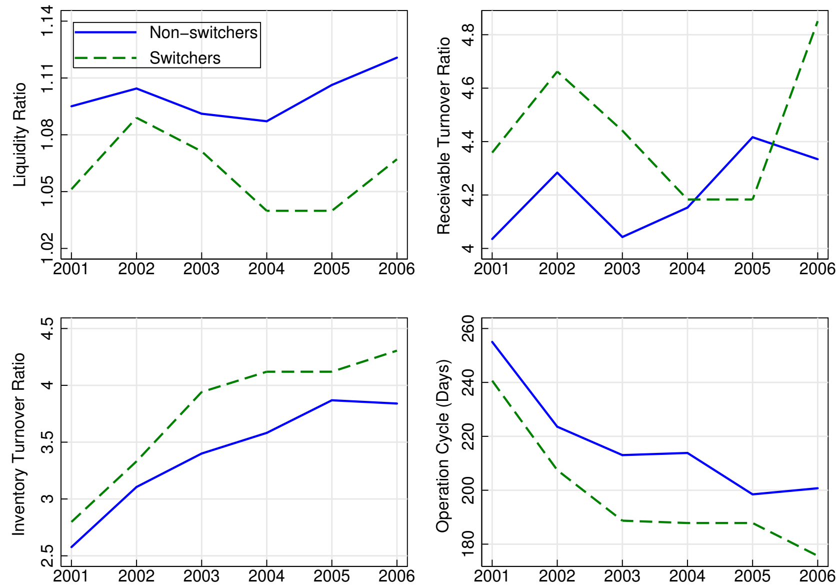


Figure A.18: Four measures of firms' efficiency in utilizing finance

Notes. Non-switchers are indirect exporters in both previous and current periods. Switchers are firms switching from indirect exporters in the previous period to direct exporters in the current period. Since the construction of receivable turnover ratio and inventory turnover ratio requires lagged variables, the four financial variables start from 2001.

A.3.2 **Table Appendix**

Table A.22: Basic statistical summary of the ASIP dataset

Year	Number of Firms	Number of Exporters	TFPR	TFPR of Exporters	Value Added	Value Added of Exporters	Employment Value	Capital Stock	Intermediate Input
2000	146,898	36,759	1.46	1.62	14,105	28,573	354	25,247	39,597
2001	153,958	39,997	1.55	1.71	14,833	28,992	296	24,348	41,570
2002	165,491	44,886	1.64	1.77	16,600	31,738	287	24,274	45,893
2003	180,696	50,534	1.73	1.83	19,410	37,006	276	24,294	55,254
2004	258,390	76,482	1.79	1.88	17,235	31,645	224	20,400	49,465
2005	250,467	74,250	1.85	1.91	21,492	38,993	240	24,123	59,697
2006	278,014	78,052	1.9	1.95	24,101	45,515	229	25,227	65,822

Notes. As in Hsieh and Klenow (2009), TFPR is dimensionless; value added is measured in thousand yuan; labor is measured in persons; capital and intermediate inputs are measured in thousand yuan.

Table A.23: Basic statistical summary of the customs dataset

Year	Number of Observations	Number of Firms	Export Value	Total Destinations	Average Destinations	Number of Products
2000	1,882,359	62,746	29,6791.4	213	6.9	30
2001	2,121,515	68,487	286,292.2	222	7.3	30.9
2002	2,613,005	78,612	270,810.7	222	7.5	33.2
2003	3,243,538	95,686	276,459.1	220	7.8	33.9
2004	4,029,789	120,590	297,836.6	220	8.3	33.4
2005	5,103,048	144,030	298,019.1	221	8.3	35.4
2006	6,187,856	171,144	301,018.7	220	8.1	36.2

Notes. Export value is measured in thousand yuan.

Table A.24: Three types of firms in the matched dataset

Year	Exporting Mode	Number of Firms	Mean TFPR	Custom Export Value	Average Destinations
2000	Direct	15,639	1.63	55,120.52	6.46
	Indirect	21,120	1.47	26,580.81	
	Non-exporters	106,994	1.37		
2001	Direct	17,957	1.71	55,482.69	7.00
	Indirect	22,040	1.53	26,678.49	
	Non-exporters	110,188	1.48		
2002	Direct	21,157	1.77	60,235.41	7.66
	Indirect	23,729	1.65	29,911.51	
	Non-exporters	115,891	1.57		
2003	Direct	25,392	1.85	68,748.30	8.27
	Indirect	25,142	1.74	37,509.51	
	Non-exporters	124,233	1.66		
2004	Direct	41,392	1.88	64,746.70	8.09
	Indirect	37,431	1.81	37,237.03	
	Non-exporters	174,321	1.73		
2005	Direct	38,683	1.93	78,127.19	9.21
	Indirect	35,567	1.85	47,413.39	
	Non-exporters	166,285	1.78		
2006	Direct	41,944	1.97	90,630.63	9.81
	Indirect	36,109	1.91	61,387.64	
	Non-exporters	188,714	1.84		

Notes. As in Hsieh and Klenow (2009), TFPR is dimensionless; custom export value is measured in thousand yuan.

Table A.25: DID estimation for export value with internal finance

Dependent Variable (horizontal)	Export Value	Export Value	Export Value	Export Value
	(1)	(2)	(3)	(4)
dExportingmode ×	0.1096***	0.1097***		
Internalfinance	[0.0376]	[0.0376]		
dExportingmode_IV ×			0.4261**	0.4332**
Internalfinance			[0.2109]	[0.2168]
dExportingmode	0.2072***	0.2016***		
	[0.0294]	[0.0279]		
dExportingmode_IV			0.1978***	0.1985***
			[0.0218]	[0.0221]
Internalfinance	0.1479***	0.1486***	0.1438***	0.1452***
	[0.0535]	[0.0538]	[0.0498]	[0.0512]
Age		0.0046*		0.0045*
		[0.0024]		[0.0024]
Size		0.0000		0.0000
		[0.0002]		[0.0002]
Year Fixed Effect	YES	YES	YES	YES
R Squared	0.14	0.14	0.14	0.14
Number of Observations	25,728	25,721	25,576	25,569
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV			0.5542***	0.5601***
			[0.0039]	[0.0042]
rk Wald F Statistic			413.01	412.42

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.26: DID estimation for export value with external finance

Dependent Variable (horizontal)	Export Value (1)	Export Value (2)	Export Value (3)	Export Value (4)
dExportingmode ×	0.1728***	0.1723***		
Externalfinance	[0.0291]	[0.0291]		
dExportingmode_IV ×			0.2892**	0.2931**
Externalfinance			[0.1358]	[0.1439]
dExportingmode	0.1994***	0.1948***		
	[0.0118]	[0.0114]		
dExportingmode_IV			0.1826***	0.1801***
			[0.0104]	[0.0097]
Externalfinance	0.1106***	0.1078***	0.0988***	0.0923***
	[0.0048]	[0.0046]	[0.0041]	[0.0039]
Age		0.0051**		0.0052**
		[0.0024]		[0.0024]
Size		0.0000		0.0000
		[0.0001]		[0.0001]
Year Fixed Effect	YES	YES	YES	YES
R Squared	0.15	0.15	0.15	0.15
Number of Observations	25,602	25,594	25,476	25,468
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV			0.5731***	0.5724***
			[0.0039]	[0.0038]
rk Wald F Statistic			563.25	563.19

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.27: DID estimation for TFPR with internal finance

Dependent Variable (horizontal)	TFPR	TFPR	TFPR	TFPR
	(1)	(2)	(3)	(4)
dExportingmode ×	0.0144***	0.0142***		
Internalfinance	[0.0047]	[0.0047]		
dExportingmode_IV ×			0.0778***	0.0783***
Internalfinance			[0.0084]	[0.0084]
dExportingmode	0.0243***	0.0239***		
	[0.0054]	[0.0052]		
dExportingmode_IV			0.0264***	0.0257***
			[0.0058]	[0.0056]
Internalfinance	0.0137***	0.0132***	0.0118***	0.0112***
	[0.0024]	[0.0023]	[0.0015]	[0.0014]
Age		-0.0001		-0.0001
		[0.0001]		[0.0001]
Size		-0.0000***		-0.0000***
		[0.0000]		[0.0000]
Year Fixed Effect	YES	YES	YES	YES
R Squared	0.03	0.03	0.02	0.03
Number of Observations	37,630	37,618	37,438	37,426
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV			0.5842***	0.5785***
			[0.0012]	[0.0012]
rk Wald F Statistic			12165.74	12162.61

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.28: DID estimation for TFPR with external finance

Dependent Variable (horizontal)	TFPR	TFPR	TFPR	TFPR
	(1)	(2)	(3)	(4)
dExportingmode ×	0.0064*	0.0064*		
Externalfinance	[0.0037]	[0.0037]		
dExportingmode_IV ×			0.0655***	0.0659***
Externalfinance			[0.0072]	[0.0072]
dExportingmode	0.0231***	0.0216***		
	[0.0076]	[0.0069]		
dExportingmode_IV			0.0207***	0.0196***
			[0.0046]	[0.0044]
externalfinance	0.0067***	0.0064***	0.0073***	0.0068***
	[0.0022]	[0.0021]	[0.0024]	[0.0023]
Age		-0.0002*		0.0002*
		[0.0001]		[0.0001]
Size		-0.0000***		-0.0000***
		[0.0000]		[0.0000]
Year Fixed Effect	YES	YES	YES	YES
R Squared	0.03	0.03	0.02	0.02
Number of Observations	37,460	37,447	37,274	37,261
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV			0.5906***	0.5893***
			[0.0015]	[0.0014]
rk Wald F Statistic			9200.25	9196.48

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.29: DDD estimation for export value with internal finance

Dependent Variable (horizontal)	Export Value (2002)	Export Value (2002)	Export Value (2003)	Export Value (2003)	Export Value (2004)	Export Value (2004)
dExportingmode_IV ×	0.3897***	0.3888***	0.0984***	0.0972***	0.0618**	0.0616**
Internalfinance × *dPost*	[0.0776]	[0.0776]	[0.0341]	[0.0341]	[0.0273]	[0.0273]
dExportingmode_IV	0.1527***	0.1493***	0.1532***	0.1517***	0.1524***	0.1502***
	[0.0176]	[0.0172]	[0.0179]	[0.0172]	[0.0175]	[0.0173]
Internalfinance	0.1075***	0.0982***	0.1103***	0.1084***	0.1093***	0.1078***
	[0.0329]	[0.0296]	[0.0331]	[0.0307]	[0.0334]	[0.0327]
dExportingmode_IV × *dPost*	0.0631*	0.0577*	0.0454*	0.0442*	0.0308*	0.0297*
	[0.0337]	[0.0324]	[0.0248]	[0.0245]	[0.0170]	[0.0167]
dExportingmode_IV × *Internalfinance*	0.2154**	0.2078**	0.3826**	0.3764**	0.4174**	0.4083**
	[0.1031]	[0.1023]	[0.1663]	[0.1587]	[0.1739]	[0.1767]
dPost × *Internalfinance*	0.0423**	0.0394**	0.0337**	0.0314**	0.0148**	0.0142**
	[0.0177]	[0.0171]	[0.0139]	[0.0136]	[0.0072]	[0.0066]
Age		0.0026		0.0037		0.0032
		[0.0031]		[0.0029]		[0.0023]
Size		0.0000**		0.0000*		0.0000
		[0.0000]		[0.0000]		[0.0000]
Year Fixed Effect	YES	YES	YES	YES	YES	YES
R Squared	0.18	0.18	0.16	0.16	0.18	0.18
Number of Observations	25,593	25,586	25,593	25,586	25,593	25,586
First stage estimation (dependent variable is *dExportingmode*)						
dExportingmode_IV	0.5628***	0.5652***	0.5643***	0.5647***	0.5721***	0.5734***
	[0.0032]	[0.0033]	[0.0035]	[0.0037]	[0.0042]	[0.0042]
rk Wald F Statistic	503.33	403.10	1328.02	1063.10	2302.88	1842.76

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. *dPost* is a dummy variable which takes value 1 if the year is in post-WTO accession period. We consider three cases of post-WTO accession period, column (2002) means that the post-WTO accession period is 2002-2006. The same logic applies to column (2003) and (2004). rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.30: DDD estimation for export value with external finance

Dependent Variable (horizontal)	Export Value (2002)	Export Value (2002)	Export Value (2003)	Export Value (2003)	Export Value (2004)	Export Value (2004)
dExportingmode_IV×	0.8663***	0.8592***	0.2769**	0.2721**	0.2641*	0.2605*
Externalfinance× *dPost*	[0.3261]	[0.3185]	[0.1185]	[0.1172]	[0.1463]	[0.1455]
dExportingmode_IV	0.1746***	0.1705***	0.1734***	0.1698***	0.1752***	0.1712***
	[0.0184]	[0.0178]	[0.0182]	[0.0176]	[0.0189]	[0.0181]
Externalfinance	0.0675***	0.0623***	0.0697***	0.0642***	0.0683***	0.0658***
	[0.0071]	[0.0068]	[0.00751]	[0.0072]	[0.0074]	[0.0070]
dExportingmode_IV × *dPost*	0.0893**	0.0865**	0.0614**	0.0598**	0.0547**	0.0521**
	[0.0446]	[0.0432]	[0.0267]	[0.0254]	[0.0219]	[0.0212]
dExportingmode_IV × *Externalfinance*	0.1018**	0.1006**	0.1872**	0.1799**	0.2253**	0.2198**
	[0.0443]	[0.0435]	[0.0749]	[0.0726]	[0.0959]	[0.0912]
dPost × *Externalfinance*	0.0923**	0.0915**	0.0587**	0.0562**	0.0553**	0.0541**
	[0.0402]	[0.0401]	[0.0269]	[0.0262]	[0.0232]	[0.0218]
Age		0.0101*		0.0059**		0.0036
		[0.0059]		[0.0030]		[0.0029]
Size		0.0000		0.0000*		0.0000
		[0.0000]		[0.0000]		[0.0000]
Year Fixed Effect	YES	YES	YES	YES	YES	YES
R Squared	0.22	0.21	0.23	0.22	0.24	0.24
Number of Observations	25,576	25,568	25,576	25,568	25,576	25,568
First stage estimation (dependent variable is *dExportingmode*)						
dExportingmode_IV	0.5486***	0.5427***	0.5519***	0.5501***	0.5492***	0.5476***
	[0.0043]	[0.0042]	[0.0046]	[0.0044]	[0.0044]	[0.0039]
rk Wald F Statistic	90.51	91.13	173.08	174.73	90.80	99.23

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. *dPost* is a dummy variable which takes value 1 if the year is in post-WTO accession period. We consider three cases of post-WTO accession period, column (2002) means that the post-WTO accession period is 2002-2006. The same logic applies to column (2003) and (2004). rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.31: DID estimation for efficiency of finance usage

Dependent Variable (horizontal)	Liquidity (1)	Receivable Turnover (2)	Inventory Turnover (3)	Operation Cycle (4)
dExportingmode	-0.0218**	0.0026	0.0534***	-0.0322***
	[0.0107]	[0.0130]	[0.0127]	[0.0094]
Export Share	-0.0002	-0.0006***	-0.0008***	0.0007***
	[0.0002]	[0.0002]	[0.0002]	[0.0001]
Constant	0.1516***	1.6130***	1.2711***	5.3167***
	[0.0149]	[0.0181]	[0.0177]	[0.0132]
R Squared	0.16	0.09	1.14	0.08
Number of Observations	9,830	9,853	9,853	9,853

Notes. Export share is the share of exports in firms' total sales, included to control for the level of involvement in international markets after the entry into direct exporting. Age, size, and year fixed effect are controlled for all regressions.

Table A.32: DID estimation for export value with internal finance and alternative fixed effects

Dependent Variable (horizontal)	Export Value	Export Value	Export Value	Export Value
	(1)	(2)	(3)	(4)
dExportingmode ×	0.1081***	0.1084***		
Internalfinance	[0.0364]	[0.0367]		
dExportingmode_IV ×			0.4223**	0.4421**
Internalfinance			[0.2123]	[0.2134]
dExportingmode	0.1941***	0.1943***		
	[0.0218]	[0.0219]		
dExportingmode_IV			0.1863***	0.1872***
			[0.0206]	[0.0214]
Internalfinance	0.1521***	0.1529***	0.1482***	0.1503***
	[0.0557]	[0.0559]	[0.0580]	[0.0592]
Age		0.0038*		0.0035*
		[0.0021]		[0.0018]
Size		0.0000		0.0000
		[0.0003]		[0.0004]
Province-Year Fixed Effect	YES	YES	YES	YES
Sector-Year Fixed Effect	YES	YES	YES	YES
R Squared	0.15	0.15	0.14	0.14
Number of Observations	24,782	24,775	24,690	24,683
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV			0.5525***	0.5549***
			[0.0038]	[0.0038]
rk Wald F Statistic			408.24	406.79

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.33: DID estimation for export value with external finance and alternative fixed effects

Dependent Variable (horizontal)	Export Value	Export Value	Export Value	Export Value
	(1)	(2)	(3)	(4)
dExportingmode ×	0.1701***	0.1698***		
Externalfinance	[0.0212]	[0.0214]		
dExportingmode_IV ×			0.2565**	0.2946**
Externalfinance			[0.1164]	[0.1426]
dExportingmode	0.1903***	0.1897***		
	[0.0122]	[0.0105]		
dExportingmode_IV			0.1794***	0.1718***
			[0.0089]	[0.0088]
Externalfinance	0.0952***	0.0918***	0.0925***	0.0896***
	[0.0042]	[0.0037]	[0.0039]	[0.0031]
Age		0.0048**		0.0053**
		[0.0022]		[0.0027]
Size		0.0000		0.0000
		[0.0001]		[0.0001]
Province-Year Fixed Effect	YES	YES	YES	YES
Sector-Year Fixed Effect	YES	YES	YES	YES
R Squared	0.15	0.15	0.15	0.15
Number of Observations	24,584	24,576	24,393	24,385
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV			0.56771***	0.5659***
			[0.0039]	[0.0038]
rk Wald F Statistic			561.77	564.32

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.34: DID estimation for TFPR with internal finance and alternative fixed effects

Dependent Variable (horizontal)	Export Value	Export Value	Export Value	Export Value
	(1)	(2)	(3)	(4)
dExportingmode ×	0.0117***	0.0109***		
Internalfinance	[0.0026]	[0.0023]		
dExportingmode_IV ×			0.0819***	0.0898***
Internalfinance			[0.0057]	[0.0061]
dExportingmode	0.0268***	0.0232***		
	[0.0068]	[0.0057]		
dExportingmode_IV			0.0256***	0.0241***
			[0.0049]	[0.0046]
Internalfinance	0.0124***	0.0116***	0.0126***	0.0123***
	[0.0018]	[0.0015]	[0.0016]	[0.0015]
Age		-0.0006*		-0.0008*
		[0.0003]		[0.0004]
Size		-0.0000***		-0.0000***
		[0.0000]		[0.0000]
Province-Year Fixed Effect	YES	YES	YES	YES
Sector-Year Fixed Effect	YES	YES	YES	YES
R Squared	0.03	0.03	0.03	0.03
Number of Observations	36,267	36,255	36,143	36,131
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV			0.5636***	0.5591***
			[0.0011]	[0.0011]
rk Wald F Statistic			12078.24	12069.79

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.35: DID estimation for TFPR with external finance and alternative fixed effects

Dependent Variable (horizontal)	Export Value (1)	Export Value (2)	Export Value (3)	Export Value (4)
dExportingmode × *Externalfinance*	0.0061**	0.0067**		
	[0.0031]	[0.0032]		
dExportingmode_IV × *Externalfinance*			0.0683***	0.0713***
			[0.0064]	[0.0065]
dExportingmode	0.0217***	0.0242***		
	[0.0062]	[0.0064]		
dExportingmode_IV			0.0220***	0.0208***
			[0.0048]	[0.0045]
Externalfinance	0.0073***	0.0081***	0.0086***	0.0084***
	[0.0025]	[0.0027]	[0.0029]	[0.0028]
Age		-0.0004*		0.0001
		[0.0002]		[0.0001]
Size		-0.0000***		-0.0000***
		[0.0000]		[0.0000]
Province-Year Fixed Effect	YES	YES	YES	YES
Sector-Year Fixed Effect	YES	YES	YES	YES
R Squared	0.03	0.03	0.03	0.03
Number of Observations	35,218	35,206	35,096	35,084
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV			0.5731***	0.5722***
			[0.0014]	[0.0012]
rk Wald F Statistic			9216.78	9155.76

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.36: DDD estimation for export value with internal finance and alternative fixed effects

Dependent Variable (Horizontal)	Export Value (2002)	Export Value (2002)	Export Value (2003)	Export Value (2003)	Export Value (2004)	Export Value (2004)
dExportingmode_IV ×	0.3197***	0.3095***	0.0874***	0.0871***	0.0607**	0.0598**
Internalfinance × *dPost*	[0.0782]	[0.0780]	[0.0372]	[0.0310]	[0.0259]	[0.0248]
dExportingmode_IV	0.1433***	0.1417***	0.1468***	0.1451***	0.1462***	0.1445***
	[0.0158]	[0.0156]	[0.0162]	[0.0159]	[0.0161]	[0.0160]
Internalfinance	0.0946***	0.0927***	0.0968***	0.0945***	0.0937***	0.0918***
	[0.0313]	[0.0304]	[0.0325]	[0.0319]	[0.0308]	[0.0306]
dExportingmode_IV × *dPost*	0.0517*	0.0498*	0.0328*	0.0312*	0.0254*	0.0233*
	[0.0305]	[0.0298]	[0.0186]	[0.0184]	[0.0153]	[0.0150]
dExportingmode_IV × *Internalfinance*	0.1972**	0.1894**	0.4069**	0.3923**	0.4143**	0.4127**
	[0.0944]	[0.0931]	[0.1628]	[0.1604]	[0.1677]	[0.1616]
dPost × *Internalfinance*	0.0327*	0.0316*	0.0284*	0.0269*	0.0225*	0.0213*
	[0.0179]	[0.0174]	[0.0151]	[0.0144]	[0.0126]	[0.0122]
Age		0.0061*		0.0046		0.0038
		[0.0035]		[0.0031]		[0.0024]
Size		0.0000***		0.0000**		0.0000**
		[0.0000]		[0.0000]		[0.0000]
Province-year Fixed Effect	YES	YES	YES	YES	YES	YES
Sector-year Fixed Effect	YES	YES	YES	YES	YES	YES
R Squared	0.19	0.19	0.16	0.16	0.19	0.19
Number of Observations	24,636	24,609	24,634	24,609	24,634	24,609
First stage estimation (dependent variable is *dExportingmode*)						
dExportingmode_IV	0.5645***	0.5672***	0.5683***	0.5694***	0.5812***	0.5826***
	[0.0033]	[0.0035]	[0.0039]	[0.0040]	[0.0043]	[0.0044]
rk Wald F Statistic	514.78	409.34	1379.46	1109.87	2469.45	2107.37

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. *dPost* is a dummy variable which takes value 1 if the year is in post-WTO accession period. We consider three cases of post-WTO accession period, column (2002) means that the post-WTO accession period is 2002-2006. The same logic applies to column (2003) and (2004). rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.37: DDD estimation for export value with external finance and alternative fixed effects

Dependent Variable (horizontal)	Export Value (2002)	Export Value (2002)	Export Value (2003)	Export Value (2003)	Export Value (2004)	Export Value (2004)
dExportingmode_IV ×	0.8473***	0.8439***	0.2265**	0.2208**	0.2176*	0.2098*
Externalfinance × *dPost*	[0.0311]	[0.3107]	[0.0960]	[0.0947]	[0.1127]	[0.1120]
dExportingmode_IV	0.1628***	0.1614***	0.1578***	0.1563***	0.1608***	0.1597***
	[0.0172]	[0.0169]	[0.0165]	[0.0161]	[0.0168]	[0.0166]
Exnternalfinance	0.0614***	0.0602***	0.0577***	0.0563***	0.0594***	0.0587***
	[0.0065]	[0.0062]	[0.0056]	[0.0054]	[0.0061]	[0.0059]
dExportingmode_IV × *dPost*	0.0738*	0.0716*	0.0513*	0.0496*	0.0357*	0.0326*
	[0.0397]	[0.0392]	[0.0282]	[0.0279]	[0.0192]	[0.0190]
dExportingmode_IV × *Externalfinance*	0.0956**	0.0941**	0.1327**	0.1313**	0.1409**	0.1401**
	[0.0379]	[0.0362]	[0.0533]	[0.0521]	[0.0595]	[0.0591]
dPost × *Externalfinance*	0.1072*	0.1033*	0.0824*	0.0817*	0.0619*	0.0606*
	[0.0583]	[0.0580]	[0.0431]	[0.0429]	[0.0321]	[0.0314]
Age		0.0105**		0.0063**		0.0042
		[0.0056]		[0.0031]		[0.0030]
Size		0.0000*		0.0000*		0.0000*
		[0.0000]		[0.0000]		[0.0000]
Province-year Fixed Effect	YES	YES	YES	YES	YES	YES
Sector-year Fixed Effect	YES	YES	YES	YES	YES	YES
R Squared	0.22	0.21	0.23	0.22	0.24	0.24
Number of Observations	24,578	24,569	24,578	24,569	24,578	24,569
First stage estimation (dependent variable is *dExportingmode*)						
dExportingmode_IV	0.5507***	0.5494***	0.5502***	0.5513***	0.5497***	0.5486***
	[0.0039]	[0.0037]	[0.0039]	[0.0041]	[0.0037]	[0.0036]
rk Wald F Statistic	92.78	94.84	184.78	186.13	94.87	101.06

Notes. Robust standard errors in bracket. Size is measured by firms' capital stock. *dExportingmode_IV* is constructed as the product of firm-level base-year productivity and province-level aggregate capital supply shock. *dPost* is a dummy variable which takes value 1 if the year is in post-WTO accession period. We consider three cases of post-WTO accession period, column (2002) means that the post-WTO accession period is 2002-2006. The same logic applies to column (2003) and (2004). rk Wald F Statistic is reported for the weak identification test of our instrumental variable. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table A.38: DID estimation with an alternative IV for switching

Dependent Variable (horizontal)	Export Value	Export Value	TFPR	TFPR
	(1)	(2)	(3)	(4)
dExportingmode_IV2 ×	0.2694**		0.0472**	
Internalfinance	[0.1242]		[0.0193]	
dExportingmode_IV2 ×		0.1748**		0.0361***
Externalfinance		[0.0828]		[0.0174]
Age	NO	YES	NO	YES
Size	NO	YES	NO	YES
Province-Year Fixed Effect	YES	YES	YES	YES
Sector-Year Fixed Effect	YES	YES	YES	YES
R Squared	0.11	0.13	0.03	0.03
Number of Observations	24,367	24,078	35,785	34,792
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV2	0.1819***	0.1880***	0.1811***	0.1832***
	[0.0124]	[0.0128]	[0.0126]	[0.0127]
rk Wald F Statistic	217.88	214.06	416.81	412.95

Notes. *dExportingmode_IV2* is an alternative instrumental variable for switching in exporting mode, it is the product of sector-level share of non-SOE firms and province-level lagged aggregate capital supply shock.

Table A.39: DDD Estimation with an alternative IV for switching

Dependent Variable (horizontal)	Export Value [2002]	Export Value [2003]	Export Value [2004]	Export Value [2002]	Export Value [2003]	Export Value [2004]
dExportingmode_IV2 × *dPost*	0.3061***	0.0976***	0.0379***			
× *Internalfinance*	[0.1250]	[0.0454]	[0.0192]			
dExportingmode_IV2 × *dPost*				0.5139***	0.1764**	0.1105**
× *Externalfinance*				[0.2459]	[0.0761]	[0.0555]
Age	NO	YES	NO	YES	YES	YES
Size	NO	YES	NO	YES	YES	YES
Province-Year Fixed Effect	YES	YES	YES	YES	YES	YES
Sector-Year Fixed Effect	YES	YES	YES	YES	YES	YES
R Squared	0.15	0.13	0.16	0.18	0.17	0.18
Number of Observations	23,793	23,793	23,793	23,468	23,468	23,468
First stage estimation (dependent variable is dExportingmode)						
dExportingmode_IV2	0.1806***	0.1817***	0.1867***	0.1896***	0.1846***	0.1852***
	[0.0123]	[0.0126]	[0.0131]	[0.0135]	[0.0132]	[0.0133]
rk Wald F Statistic	173.43	183.87	452.97	154.28	165.36	149.63

Notes. *dExportingmode_IV2* is an alternative instrumental variable for switching in exporting mode, it is the product of sector-level share of non-SOE firms and province-level lagged aggregate capital supply shock

Table A.40: DID estimation for export value with finance proxy

Dependent Variable (horizontal)	Export Value	Export Value	TFPR	TFPR
	(1)	(2)	(3)	(4)
dExportingmode ×	0.3862***		0.0782***	
Internalfinance_proxy	[0.1893]		[0.0076]	
dExportingmode_IV ×		0.2573***		0.0605***
Externalfinance_proxy		[0.1191]		[0.0064]
Age	NO	YES	NO	YES
Size	NO	YES	NO	YES
Province-Year Fixed Effect	YES	YES	YES	YES
Sector-Year Fixed Effect	YES	YES	YES	YES
R Squared	0.13	0.14	0.03	0.03
Number of Observations	24,683	24,385	36,131	35,084
First stage estimation (dependent variable is *dExportingmode*)				
dExportingmode_IV	0.5467***	0.5502***	0.5627***	0.5683***
	[0.0036]	[0.0019]	[0.0012]	[0.0013]
rk Wald F Statistic	379.38	502.78	11749.65	8763.48

Notes. *Internalfinance_proxy* and *Externalfinance_proxy* are sector-level internal and external finance, respectively.

Table A.41: DDD estimation for export value with finance proxy

Dependent Variable (horizontal)	Export Value	Export Value	Export Value	Export Value	Export Value	Export Value
	[2002]	[2003]	[2004]	[2002]	[2003]	[2004]
dExportingmode_IV × *dPost*	0.4184***	0.1074***	0.0685**			
× *Internalfinance_proxy*	[0.0831]	[0.0377]	[0.0304]			
dExportingmode_IV × *dPost*				0.7554***	0.2856**	0.2237*
× *Externalfinance_proxy*				[0.2368]	[0.1216]	[0.1157]
Age	YES	YES	YES	YES	YES	YES
Size	YES	YES	YES	YES	YES	YES
Province-Year Fixed Effect	YES	YES	YES	YES	YES	YES
Sector-Year Fixed Effect	YES	YES	YES	YES	YES	YES
R Squared	0.18	0.15	0.18	0.22	0.22	0.23
Number of Observations	24,537	24,537	24,537	24,498	24,498	24,498
First stage estimation (dependent variable is *dExportingmode*)						
dExportingmode_IV	0.5736***	0.5812***	0.5843***	0.5413***	0.5492***	0.5427***
	[0.0038]	[0.0042]	[0.0046]	[0.0036]	[0.0040]	[0.0035]
rk Wald F statistic	417.34	979.18	1879.82	92.61	176.94	97.76

Notes. *Internal finance_proxy* and *External finance_proxy* are sector-level internal and external finance, respectively.

A.3.3 Data Appendix

A.3.3.1 Matching Procedure for Manufacturing and Customs Data

We match Chinese manufacturing survey data (ASIP) and Customs data using the following procedure. This algorithm produces highly comparable results to the existing studies, like Manova and Yu (2016).

`Step 1.` Given the potential existence of typographical errors in both data sets, we clean the data sets using a conservative approach. In the Customs data set, we use the non-missing modes (i.e. the most frequent value) of party_id, zip code, and telephone number of the monthly data as the annual value for our matching purpose. In both annual data sets, if the identifier or "concatenation of zip code and telephone number" exists more than once, we discard all the observations to avoid the case that an identifier in one data set might link to multiple identifiers in the other data set. Less than 0.01% of the observations are dropped each year due to these typographical errors.

`Step 2.` To get the identifier concordance, we first match firms' Chinese name of the two data sets if the same names appear in both data sets in the same year. This provides the most reliable matching results. Then we add concordances if the same name shows up in different years of the two data sets, which might be due to delays in information updating. If the second match generates a different identifier concordance from the first match, we dropped the second matched result.

`Step 3.` We follow the same procedure in `Step 2` for the "concatenation of zip code and telephone" for the two data sets. Again we think that the matches from the same year are more reliable than matches from different years.

`Step 4.` The order of confidence in the concordance is: same names in the same year, same telephone number and zip code in the same year, same names in different years, and same telephone number and zip code in the different years. Every time the latter matches generate a different identifier concordance from the earlier matches, we use the earlier matched results.