DEPARTMENT
OF ECONOMICS

Essays on Trade, Climate Change, and Household Finance

Chloe Larkou

Thesis submitted for assessment with a view to
obtaining the degree of Doctor of Economics
of the European University Institute

Florence, 15 March 2024

European University Institute
Department of Economics

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Researcher declaration to accompany the submission of written work
Department Economics - Doctoral Programme

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I confirm that chapter 3 was jointly co-authored with Mr Russell Cooper and I contributed 50% of the work.

Statement of language correction:
This thesis has been corrected for linguistic and stylistic errors. I certify that I have checked and approved all language corrections, and that these have not affected the content of this work.

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31.08.2023

To my mum, Rena

Abstract

This thesis uses applied econometric methods to answer policy-relevant questions. Using unexploited big micro datasets, this thesis investigates the economic implications of shocks, such as trade policy shocks and climate change shocks.

The first chapter (joint with Alica Ida Bonk) investigates the effects of trade policy shocks on the US economy between 2007 and 2019. We identify unanticipated trade policy shocks by analyzing the stock price reactions of trade-exposed and non-trade exposed firms around the release of official trade policy statements. Using local projections, we explore asymmetries and non-linearities in the effects of these shocks, including whether a policy is protectionist or liberalizing, whether the shock was originated by the US or a trade partner, and whether the statement refers to an implementation of a policy change or a mere announcement.

The second chapter explores the impact of physical risk from climate change on residential real estate prices. I identify climate change shocks using temporal and spatial variation of natural events in Germany between 2013-2021. Using geo-coded property-level data, I uncover that real estate prices drop significantly after climate change shocks, not only in directly affected areas but also in unaffected areas of similar risk or geographically close to the affected areas. This study also investigates differential property risk and the media's role in spreading local climate shocks.

The third chapter of this thesis (joint with Russell Cooper) turns to a different topic and methodology. This paper examines the dependence of a household's marginal propensity to consume on its homeownership status. This is relevant for assessing arguments that policy innovations impact spending through the relatively large consumption response to income variations of homeowners with mortgages. In contrast with existing claims, we do not find robust evidence that the

MPC of agents with mortgages exceeds that of outright homeowners and renters.

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In reflecting on this journey, I am deeply grateful for the diverse contributions and unwavering support that have enriched my path to completing this PhD thesis.

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A very special thanks to my esteemed co-authors, Prof. Russell Cooper and Alica Ida Bonk, for their invaluable contributions to this thesis. Collaborating with you has been a privilege, and I am extremely grateful for your expertise, dedication, and support. I have learned a lot from you.

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Finally, I owe the biggest "thank you" to my family and friends for supporting me throughout this PhD and life in general. I am profoundly grateful to my brother, Frixos, for always supporting and believing in me, especially in those moments when I doubted myself. I know you never did. To my father, Philippos, who has accompanied me during the beginning of this PhD journey and every new beginning. Thank you for your support and your belief in me. Your pride in me gives me the strength and confidence to strive to continue to get better. Moreover, I am deeply thankful to my cherished grandparents for their constant love, guidance, and support in every aspect of my life.

I would like to express my deepest appreciation and heartfelt thanks to my incredible partner, Mattia, for his unwavering presence and support throughout the last five years, during the completion of this thesis. From spending the weekends in the library with me, offering a shoulder to cry on during the many difficult moments throughout this journey, and engaging in fruitful discussions about my ideas, his contribution has been instrumental to the development of this thesis. I am forever grateful for your unwavering belief in me. I couldn't have done this without you.

Last but certainly not least, I am eternally thankful to my exceptional mother, Rena. She has been an unparalleled exemplar of strength and independence, ceaselessly inspiring, encouraging, and supporting me to follow my dreams. Despite her physical absence, her unwavering belief in my abilities and her unconditional love have been a constant and enduring source of strength throughout this journey. I am forever grateful for the values she instilled in me and all her sacrifices. *This thesis is dedicated to her.*

Contents

Chapter 1

The macroeconomic impact of trade policy: A new identification approach

1.1 Motivation

The long-term benefits of free trade are widely acknowledged by economists, yet there remains less consensus regarding its short-term implications and the consequences of moving towards a more protectionist stance. This lack of consensus can be partially attributed to the scarcity of post-war protectionist actions. Additionally, challenges arise related to the identification of the unanticipated component of trade policy changes due to the multiple rounds of negotiations and prior threats that typically precede implementation.

This paper aims to address these challenges and answer the question: What are the short- and medium-term implications of both liberalizing and protectionist trade policies on macroeconomic variables? To achieve this, we introduce a novel data set comprising daily official trade policy statements issued by the United States and its trade partners between 2007 and 2019, covering various *types* of reforms. We argue that this narrative approach can be combined with stock market data to identify unanticipated trade policy shocks.

We find that industrial production, exports, imports, and commercial loans increase following a liberalizing shock and similarly decline after a protectionist shock. The gains from trade liberalizations and the damage from protectionism are of equal magnitude in absolute terms, with no non-linearities observed along this dimension.

On the other hand, responses vary depending on the type of trade policy shock. We find that implementation tends to elicit a stronger, more significant response than announcements. Being uncertain about whether policymakers will follow through with planned trade policy changes, firms and households seem to adopt a "wait and see" approach. Moreover, a trade shock initiated by the US has less significant effects compared to one caused by trade partners. Our results point to potentially detrimental effects of foreign retaliations on firm investment, employment, and consumption. We estimate that the implementation of retaliatory tariffs, such as by the EU in June 2018, decreased US firm investment by -0.9% to -4.6% on impact. Finally, our analysis of non-linearities reveals that this could be a conservative estimate since firm investment is affected disproportionately by large trade policy shocks.

These results are highly robust to different monthly shock aggregations, to grouping stock prices based on different definitions of trade exposure (i.e. import dependency), and to controlling for other macroeconomic news. In an extension, we use Trump's tweets instead of official trade policy statements to identify trade policy shocks. We find that the response of most macroeconomic variables is more muted compared to our baseline shock, possibly due to the tweets' lack of credibility and specificity.

Our contribution to the existing literature is threefold. First, we present a new data set containing daily official trade policy statements by the United States and its trade partners covering periods of striving towards trade liberalization as well as the abrupt shift towards protectionism in 2018/19. This data set provides richer information than previously used sources such as Google Trends (Amiti et al. (2020)). Thus, we can distinguish between pure announcements and implementations and account for differential effects depending on the initiating country. Moreover, we capture changes in non-tariff barriers, an important component of trade agreements.

Second, we propose a novel identification strategy that complements the data set with information contained in stock prices. More specifically, on days with trade-related statements, we observe price movements of two stock baskets that differentiate firms by exposure to trade policy based on their propensity to export. Apart from allowing for a more accurate categorization of liberalizing and protectionist shocks, this high-frequency approach enables us to extract and quantify the unanticipated and exogenous component of trade policy actions.

Our identification strategy brings together three strands of the literature: research using a narrative approach to identify macroeconomic shocks, papers pointing at policy news as a major source of equity market movements (e.g., Baker et al. (2019), Moser and Rose (2014)), and analyses exploiting firm-level differences in policy exposure (e.g. Fisher and Peters (2010), Baker et al. (2016)).

Trade policy shocks are identified whenever a trade policy statement was issued by a US or foreign government entity, and two additional conditions hold. Both the stock price of trade-exposed firms and the ratio of trade-exposed and non-trade-exposed firms need to move in the same direction. Hence, we require that internationally active firms are relatively more affected, which is a well-documented fact (see Greenland et al. (2019) and Huang et al. (2019)), that we validate ex-ante using our data set. Both the stock price of trade-exposed firms and the ratio of trade-exposed to non-trade-exposed firms must move in the same direction. To minimize subjectivity, we adopt an ex-ante agnostic stance regarding the direction of policy changes, relying instead on stock price movements to determine the sign of the shock. Our analysis demonstrates that stock prices effectively capture significant trade policy shifts, enabling us to quantify the magnitude of shocks. Furthermore, we provide robust evidence that the identified shocks remain uncontaminated by other macroeconomic shocks, as well as the current, past, or expected future economic conditions or uncertainties. Notably, our shocks exhibit considerable predictive power for the trade policy uncertainty index developed by Baker et al. (2016). Moreover, we find that combining stock price movements with a narrative approach is crucial for ensuring the exogeneity of the shocks.

Our last contribution is based on using the local projections strategy proposed by Jordà (2005), which allows us to analyze asymmetric responses of monthly output, investment, and trade to liberalizing and protectionist shocks. In addition, we can detect non-linearities in shock size and compare responses to shocks caused by the US and its trade partners. By examining the effect of different trade policy shocks using the local projections framework, we are able to obtain comparable estimates for the different shocks. This is not the case when comparing estimated effects under different studies, which make use of diverse datasets, sample periods, and methodologies. In this way, our findings provide a more comprehensive understanding of the heterogeneous effects of trade policies.

The rest of the paper is structured as follows. Section 1.2 provides an overview of the existing literature, while Section 1.3 introduces the data. Subsequently, Section 1.4 describes our empirical strategy and Section 1.5 the results. We present sensitivity analysis and extensions in Section 1.6. Finally, Section 1.7 concludes.

1.2 Literature

Our work relates to three strands of the literature: research investigating the effects of trade policy shocks, studies highlighting the implications of policy news on equity markets, and analyses exploiting firm-level differences in policy exposure.

Within the literature on trade policy shocks, a number of studies have motivated our work. Barattieri et al. (2021), for example, investigate the effects of tariffs on short-term macroeconomic fluctuations using anti-dumping investigations. Complementing this identification approach with input-output tables, Barattieri and Cacciatore (2023) find ambiguous employment effects of anti-dumping and countervailing duties. This motivates our further study of the macroeconomic consequences of trade policies, aiming to capture both tariff and non-tariff barriers in order to assess the comprehensive impact of reforms.

Furthermore, Waugh (2020) explores the impact of trade shocks by focusing on US counties' exposure to Chinese retaliatory tariffs during the 2017-2018 trade war. He finds significant employment effects already before tariffs are implemented. Based on this observation, our identification strategy distinguishes between announcements and implementations and analyzes both domestically-originated and foreign-induced trade shocks.

Several other papers investigate the effect of the 2018 trade war on traded goods' quantities and prices. For example, using an event study, Fajgelbaum et al. (2020) find a significant decrease in targeted imports and exports and a full pass-through to import prices after a tariff increase. Similarly, Amiti et al. (2019) analyze firm-level customs data and find an immediate increase in US prices due to tariffs, primarily affecting US consumers rather than foreign exporters.

Trade policies may also have indirect effects through uncertainty. Caldara et al. (2020) show that increased uncertainty reduces investment and activity both at micro and aggregate levels. Moreover, Handley and Limão (2017) find that China's WTO accession in 2001, by reducing the threat of a US-China trade war, led to increased US imports from China, lower prices, and higher consumer incomes. However, Alessandria et al. (2019) suggest that pure uncertainty has minimal impact, but expectations of future tariff increases encourage front-loading of trade. Their study emphasizes the significant influence of anticipation effects and motivates our focus on isolating unexpected policy changes. Unlike our paper, Alessandria et al. (2019) do not specifically examine unanticipated policy shocks, as the vote to renew China's MFN status represented a potential tariff change whose size and timing were known. Similarly, Metiu (2021) study the effect of U.S. trade policy announcement shocks. They find that announced but not yet imposed trade restrictions lead to output and investment contractions in major trading partners, further emphasizing the role of expectations in trade policy.

Our identification approach is related to the literature on policy news and its impact on the stock market. For example, Moser and Rose (2014) demonstrate the significant influence of news related to regional trade agreements on national stock market indices. Additionally, Baker et al. (2019) construct an Equity-Market-Volatility index based on newspaper data, showing that policy news, including trade-related news, drives stock market fluctuations. Furthermore, Egger and Zhu (2019) analyze stock market reactions during the US-China trade war, finding effects on stock prices in both target and home countries. The research by Pástor and Veronesi (2013) confirms that government policies impact risk premia and stock prices, providing a basis for utilizing stock prices as indicators of reactions to trade news.

Our approach also builds on studies that analyze how policy news affects firms with different levels of policy exposure. For example, according to Huang et al. (2019), firms highly reliant on trade with China experienced lower stock returns and increased default risks following President Trump's proposed tariffs. Similar conclusions are reached using Google Trends to identify key trade war events Amiti et al. (2020). Baker et al. (2016) demonstrate that firms with greater exposure to government purchases have higher stock price volatility during periods of fiscal policy uncertainty. Wagner et al. (2018), Davis and Seminario (2019), Hassan et al. (2019) and Hassan et al. (2020) provide additional evidence on the firm-specific impact of policy risk. These papers provide the underpinning for our shock identification which relies on the fact that differences in firms' trade exposure are reflected in stock prices.

Our paper contributes to the existing literature by examining the short- and medium-term effects of trade policy shocks on the macroeconomy. We combine a narrative approach with stock returns to identify unanticipated trade policy shocks, capturing both tariff and non-tariff barriers. Our novel data set allows us to shed light on a wide range of previously overlooked asymmetries. In particular, we differentiate between the effects of protectionist and liberalizing trade policy shocks, as well as those initiated by the US versus its trading partners. We also distinguish between policy announcements and their subsequent implementation. In addition, we uncover non-linearities in the magnitude of shocks and highlight differences across trade policy communication channels. By examining the effect of trade policy shocks using the local projections framework, we are able to obtain estimates that are comparable and can help us identify or reject the presence of asymmetric effects. Thus, our findings provide a more comprehensive understanding of the heterogeneous effects of trade policies.

1.3 Data

Our paper relies mainly on three types of data: trade policy statements, stock price data, and macroeconomic variables. This section describes each of these in turn and Appendix A.1 summarizes our sources.

1.3.1 Official trade policy statements

To identify trade policy shocks, we construct a new data set using official trade policy statements issued by the US and its trade partners. Statements are recorded for every day starting on 1 January 2007 and ending on 31 December 2019. We end the estimation sample before the Covid-19 pandemic due to the potential distortions it may introduce, making it challenging to separate the effects of trade policies from those of the pandemic. Our primary source of information on trade policy is the Office of the US Trade Representative (USTR), the government agency entrusted with the development of trade policies, advising the president, and overseeing trade negotiations. USTR statements are complemented by publications of other US Executive Branches, such as the White

House, the Department of Commerce, and the International Trade Commission.[1] Additionally, newspaper articles from Bloomberg, the Financial Times, and Reuters are consulted in case they precede official statements or provide complementary information.[2] Information on trade partners' policy actions towards the US is mostly taken from press releases of the USTR but is supplemented with the respective national sources (e.g. the Chinese Ministry of Commerce).

Statements are classified into "major" and "minor" based on the importance of the information released. The former category is assigned if **either** one of the major US trade partners (Canada, Mexico, China, the EU, Korea, Japan or the UK) **or** a group of at least five trade partners is involved **and** if a large amount of goods is affected by a drastic change in trade policy, for example through tariffs or a trade agreement. Although the data set provides information on whether the announced policy is presumably trade liberalizing (i.e. implying lower barriers to trade) or protectionist, we do not make use of this classification when constructing our baseline shock. Data entries are further categorized into "announcements", notifying the public of potential future policy changes, and "implementations", marking the day on which policies are formally approved (e.g. signing of trade agreements) or go into effect. Moreover, we record whether or not the policy was initiated by a trade partner (e.g. tariff retaliations). This detailed categorization allows us to study the effect of different types of trade policy shocks and explore the relevance of uncertainty that is inherent in pure announcements. Out of the 3262 working days in our sample, trade policy statements have occurred on 848 days. On 104 of these days, "major" proclamations were made, which we will use in this paper. 16 of these days contain statements issued unilaterally by trade partners without any dissemination of trade policy by the US. Moreover, 30% of the major entries refer to policy implementations, and the rest to announcements.

Figure 1.1 depicts the number of major protectionist and liberalizing statements per month, while the figure in Appendix A.2.1 also shows minor statements. The shift from a relatively liberal trade policy stance under Bush and Obama towards a protectionist stance under Trump is clearly

[1] Additional sources include the Departments of State and Agriculture, the US Customs and Border Protection, the Federal Register and the Department of the Treasury

[2] Statements made during Trump's presidency are also cross-checked with piie.com and shenglufashion.com

visible. A timeline of selected major trade policy statements can be found in Appendix A.3.

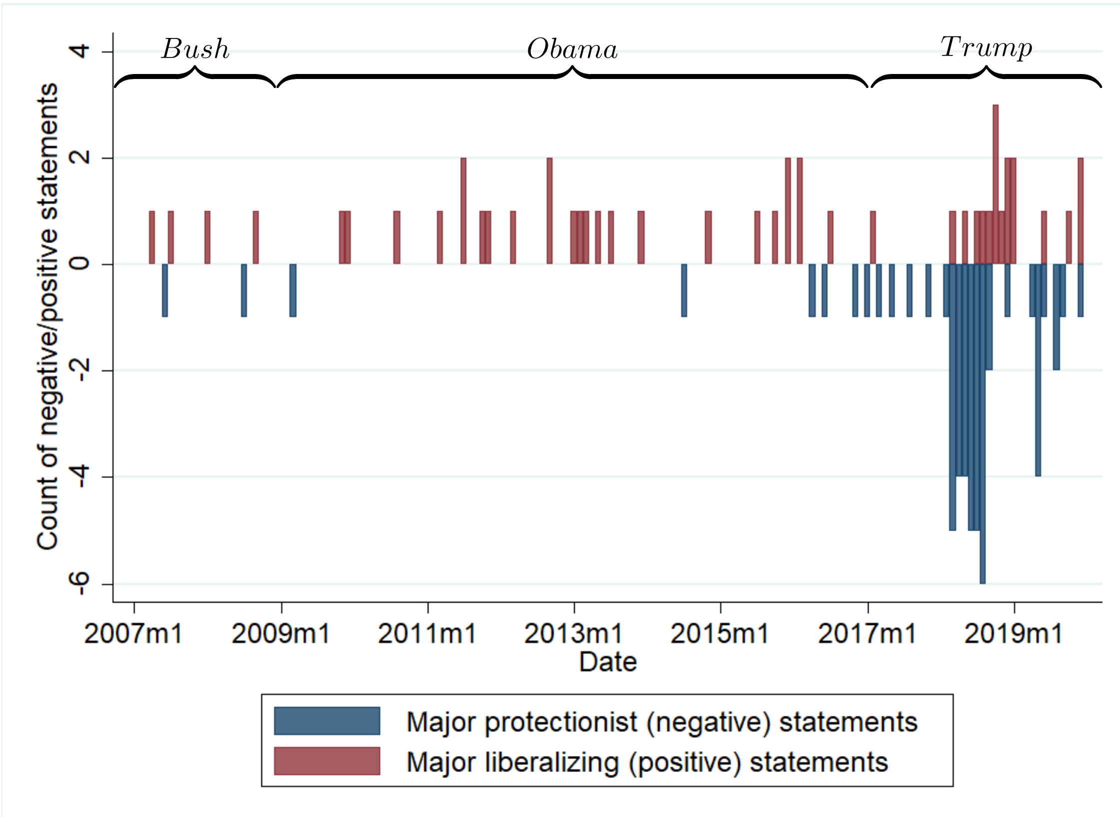


Figure 1.1: Number of **major** trade policy statements by sign, aggregated to the monthly frequency.

1.3.2 Stock market data

The second building block of our analysis is daily stock market data.[3] Specifically, we use two stock baskets constructed by Goldman Sachs and provided by Bloomberg. On the one hand, we use their "International Sales basket", which constructs a stock market index based on the 50 S&P 500 companies with the highest international sales share (henceforth "exporters"). These firms should be particularly affected by trade policy changes. According to Bloomberg, the international

[3]To verify robustness, we would have liked to use a more high-frequency identification scheme but such stock market data was not accessible to us. Besides, we only know the day but not the exact time at which trade policy statements were issued. Furthermore, Wagner et al. (2018) have documented the time lag with which the stock market responds to trade policy changes. This stands in contrast to monetary policy decisions for which the planned time of announcement is roughly known ex-ante and hence investors may react more quickly to policy changes.

portfolio contains companies from 11 different sectors, encompassing both manufacturing and services.[4] The median firm derives 71% of its revenues from abroad compared with 27% for the median S&P 500 company. On the other hand, we rely on the "Domestic sales basket", based on the 50 S&P 500 stocks with the highest domestic revenue exposure, which should be least affected by trade policy. The median firm in this basket generates 100% of its revenues domestically, whereas, for the median S&P 500 company, the share is 73%. Appendix A.4 reports examples of firms that make up each basket. The evolution of both stock price indices as well as their ratio, is depicted in Figure A.1.

To ensure that our results also hold when using an alternative definition of trade exposure, we build our own importer stock price index. This is done by using the Hoberg-Moon Offshoring data set (Hoberg and Moon (2017)), which records the frequency of firms mentioning the purchase of inputs from abroad in their 10-K financial statements.[5] For each year, we pick the top 50 importers.[6] These firms are matched to their daily stock prices taken from the Center for Research in Security Prices (CRSP). The stock price index is then built by weighting each firm's stock price by its import intensity.[7] Finally, we build the ratio of importers to non-importers, with the latter referring to companies that had never imported while they were part of the sample.

1.3.3 Macroeconomic data

The third data type used pertains to macroeconomic variables. The dependent variables we are particularly interested in include industrial production, the consumer price index, the producer price index, consumption, consumer and commercial loans as proxies for investment, imports, and exports. Furthermore, the effect of trade policy on labor market variables such as the unemploy-

[4]While manufacturing firms bear the brunt of tariffs on goods, service sector companies are also influenced by fluctuating input costs, non-tariff trade barriers, and potential reputation damage during trade conflicts.

[5]A 10-K financial statement is a detailed financial report that public companies have to submit to the US Securities and Exchange Commission (SEC).

[6]Given that 2015 is the last year in the Hoberg-Moon dataset, we assume that the import intensities for 2016-2019 are equal to the average of the last two years in the sample.

[7]We checked that our results are robust to using simple averages and weighing stock prices by firms' market capitalization.

ment rate and the hiring rate is analyzed. To further refine the analysis, industrial production and consumption by sector are used. The exchange rate (broad dollar index), a measure of uncertainty (VIX), the S&P 500, commodity prices, and the federal funds rate are included as controls since they may be correlated with both the dependent variables and the shock measure described in Section 1.4.1 and capture the state of the business cycle. Most of these variables are obtained from the FRED, the Bureau of Labor Statistics (BLS) and the US Census Bureau. Whenever appropriate, the data is deflated by the CPI and expressed in log per capita terms. More detail on the sources and variable transformations can be found in Appendix A.1.

1.4 Empirical approach

This section discusses our estimation approach and highlights the benefits of combining trade policy statements with stock market data. The latter help to reveal each statement's surprise element, assess whether announcements are perceived as trade liberalizing or protectionist, and quantify the magnitude of trade policy shocks. However, relying solely on stock market movements carries the risk of misidentifying shocks caused by factors such as exchange rate fluctuations or business cycle fluctuations. To ensure that trade policy shocks are exogenous, it is necessary to combine stock prices with a narrative approach.

1.4.1 Identifying trade policy shocks

We identify trade policy shocks between January 2007 and December 2019 based on official statements and the stock price data for exporters and non-exporters described above. Firms with a high revenue generated abroad are relatively more responsive to innovations in trade policy, as shown in Huang et al. (2019) and Bonk (2020), since their expected future sales depend on tariff and non-tariff barriers. In addition to being directly affected by trade partners' announcements and implementations, stock prices of internationally exposed firms may also react to US statements either in anticipation of foreign retaliation or because exporting firms also tend to be importers of input factors (Bernard et al. (2009)).

A trade policy shock is identified for a given day whenever a trade-related statement occurs, and both the stock price of trade exposed firms as well as the ratio of trade exposed and non-trade exposed firms move in the same direction.[8] Hence, domestically operating firms are allowed to respond to a trade policy statement since they may be influenced through downstream suppliers.[9] However, we require internationally operating firms ("exporters") to be relatively more affected.[10]

Combining official trade policy statements with stock market data to identify trade shocks is useful for four reasons. First, using statements alone, does not eliminate anticipatory effects which weaken the causal identification of the effect of trade shocks on the macroeconomy. As Appendix A.3 shows, many trade policy changes have been announced several times before they were implemented and hence consumers and investors may have already incorporated these into their consumption and investment behavior. Instead, changes in stock prices reveal the surprise component of each statement.[11] Second, from reading official statements alone it is hard to assess whether announcements will be judged positively (as trade liberalizing) or negatively (as protectionist) by the market. Examples of statements that cannot be classified unequivocally are provided in Appendix A.5. Third, using stock prices allows us to gauge the size of trade shocks which otherwise is a challenging undertaking, especially for trade agreements which involve eliminating both tariff and non-tariff barriers, the latter of which is difficult to quantify.[12] Finally, by using movements in the two stock baskets alone, we would run into the risk of identifying shocks that are not due to changes in trade policy but that may reflect exchange rate movements or

[8] If a trade policy statement is issued during the weekend or a public holiday, we consider the nearest day on which stock markets re-open, as the day of the announcement.

[9] Bonk (2020) shows that on average, the returns on exporting and non-exporting firms are pushed in opposite directions by trade policy statements which has favourable implications for our identification strategy.

[10] For example, after a surprise protectionist shock caused by US policymakers, the price ratio is likely to fall if exporting firms expect retaliation, if exporters have a higher tendency to import than non-exporters which is well-established or if domestically oriented firms are competing with foreign producers such that they benefit from market protection. From Appendix A.4 it becomes clear that almost half of the example companies in the domestic stock basket provide services which do not rely on foreign inputs making these firms not only less exposed to US trade policy through their buyers but also suppliers.

[11] As Baker et al. (2019) point out, according to the efficient markets hypothesis, stock price movements "reflect genuine news that alter rationally grounded forecasts of future earnings and discount factors".

[12] Examples of non-tariff barriers include import bans, restrictive licenses and lengthy customs procedures.

business cycle fluctuations.[13]

Ex-ante we are agnostic about whether an announcement is protectionist or liberalizing because, as mentioned above, there are several instances in which objective categorization is not possible. Instead, we determine the sign of the shock based on the stock price movements they induce. We define cumulative stock returns (CR) over a two-day window [0,1] on the basket of trade exposed firms as the sum of the returns on the day of an announcement ($t = 0$) and the subsequent day ($t = 1$), i.e.

$$CR^{EX}[0, +1] = \sum_{t=0}^{1} ret_t^{exporter} = \sum_{t=0}^{1} \frac{P_t^{exporter} - P_{t-1}^{exporter}}{P_{t-1}^{exporter}}. \tag{1.1}$$

where $P_x^{exporter}$ is the stock index of exporting firms. Similarly, the cumulative change in the ratio of trade exposed and domestically focused firms is given by:

$$CR^{Ratio}[0, +1] = \sum_{x=t}^{t+1} ret_x^{Ratio} = \sum_{x=t}^{t+1} \frac{\frac{P_x^{exporter}}{P_x^{non-exp.}} - \frac{P_{x-1}^{exporter}}{P_{x-1}^{non-exp.}}}{\frac{P_{x-1}^{exporter}}{P_{x-1}^{non-exp.}}}. \tag{1.2}$$

where $P_x^{non-exp.}$ is the stock index of non-exporting (domestically oriented) firms.

A *protectionist (liberalizing)* shock is identified for a given day, whenever the following conditions are satisfied:

(a) a trade policy statement was issued, and

(b) both the cumulative change in the stock price ratio of exporting and non-exporting firms (see Equation (1.2)) **and** the cumulative returns on the exporters' stock basket (see Equation (1.1)) are *negative (positive)*.

To avoid misclassification, both conditions in (b) are necessary. Neglecting the former could lead to misidentifying a trade policy shock when it is actually caused by an unrelated event affecting the stock market as a whole. Neglecting the latter could lead to misclassifying a statement as protectionist, ignoring the possibility that the stock price index of non-exporting firms outperforms

[13] A sudden appreciation of the US dollar for example is likely to cause drops in foreign demand and therefore in the stock price index of trade exposed firms. Similarly, a global economic downturn as in 2007-2009 harms exporting firms most, leading to a disproportionate fall in their stock prices.

that of exporters and causes the ratio to fall. The approach allows for domestically operating firms to respond to a trade policy statement since they may be influenced through upstream suppliers. However, we require internationally operating firms to be relatively more affected.[14] The reason for cumulating returns over a two-day window [0,1], i.e. the day of an announcement and the subsequent day, is to account for lags in investors' decision making.[15] The size of trade policy shocks is captured by Equation (1.2), i.e. the shock series takes the value of the cumulative change in the two baskets' ratio. This ratio better reflects the impact of trade policy compared with the cumulative returns on the exporters' basket, which may follow general market movements caused by news unrelated to trade, as mentioned above. On days without trade policy-relevant news, the shock series equals 0.

Figure 1.2 shows the resulting shock series at the daily frequency and highlights the largest protectionist and liberalizing shocks in the sample.[16] Three observations confirm the plausibility of our shock series: First, trade policy shocks became larger, more frequent, and more protectionist after President Trump took office. Second, 75% of the daily shocks have the expected sign based on our ex-ante subjective classification of statements. Third, the shocks seem to accurately reflect momentous trade policy changes, i.e. those that are particularly surprising, involve a large share of traded goods or substantial tariff changes.

[14]For example, after a surprise protectionist shock caused by US policymakers, the price ratio is likely to fall if exporting firms expect retaliation, if exporters have a higher tendency to import than non-exporters which is well-established or if domestically oriented firms are competing with foreign producers such that they benefit from market protection. From Appendix A.4 it becomes clear that almost half of the example companies in the domestic stock basket provide services which do not rely on foreign inputs making these firms not only less exposed to US trade policy through their buyers but also suppliers.

[15]If a trade policy statement is issued during the weekend or a public holiday, we consider the nearest day on which stock markets re-open, as the day of the announcement.

[16]Figure A.4 plots the distribution of daily shock sizes and compares them to the average cumulative change in the stock price ratio of exporters to non-exporters for the whole sample period.

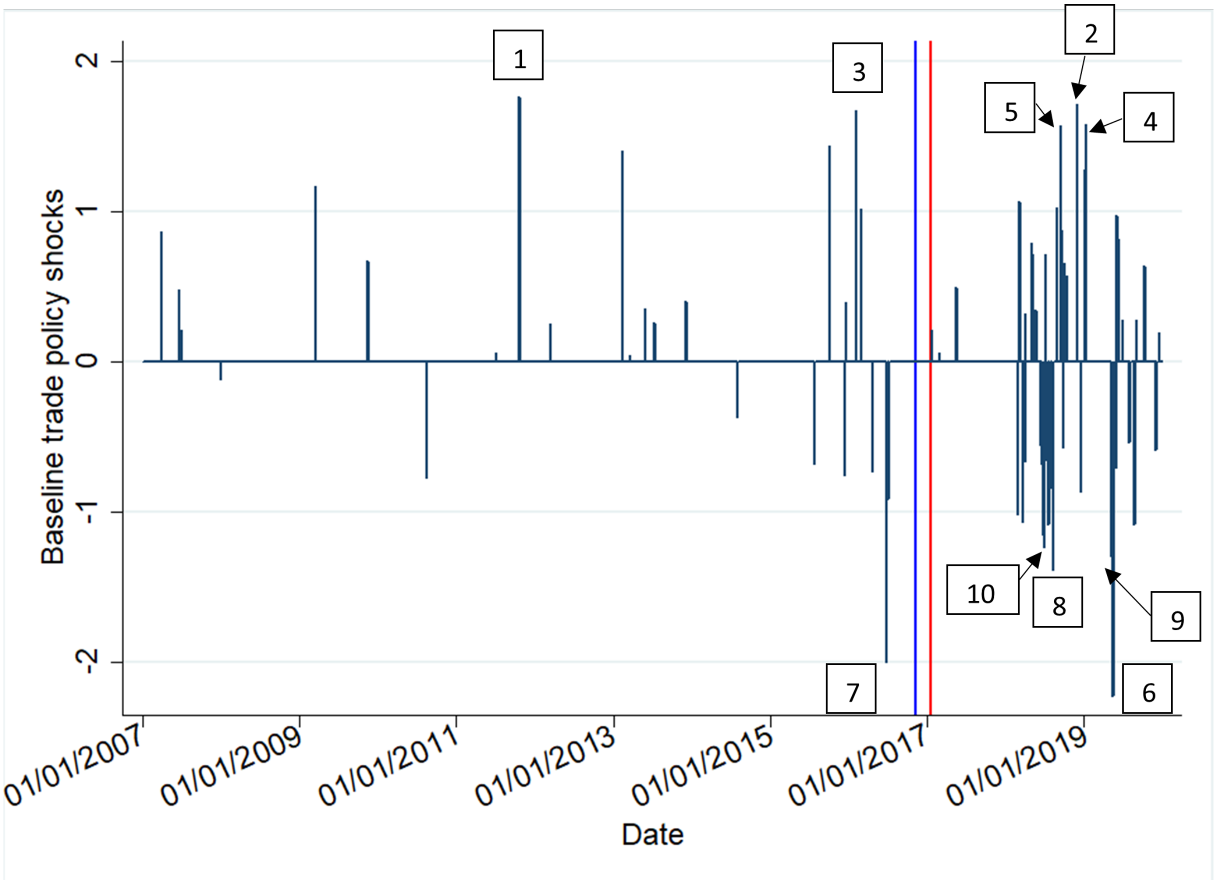


Figure 1.2: **Daily** baseline trade policy shocks. The vertical lines represent the day on which the 2016 election results were announced (blue) and President Trump took office (red).

The largest **liberalizing** shocks (2007-2019):
1- Mexico lifts tariffs on dozens of US imports. (21/10/2011)
2- US-Mexico-Canada FTA signed; Trump announces to maintain tariffs at 10% on $200 bn worth of Chinese products, without increasing them to 25%. (30/11/2018 & 01/12/2018)
3- Trans-Pacific Partnership agreement signed (03/02/2016)
4- Talks between the US and China to de-escalate tariff war. (09/01/2019)
5- Trump signs law reducing or eliminating import tariffs on over 1,660 items, including half made in China. (13/09/2018)

The largest **protectionist** shocks (2007-2019):
6- The Trump administration claims certain cars and car parts threaten US national security; will urge trade partners to limit auto exports. (17/05/2019)
7- UK voted to leave the EU (with implications for future UK-US tariff rates). (23/06/2016)
8- Trump announces raising steel tariff on Turkey from 25% to 50%. (10/08/2018)
9- Trump plans to raise 10% section 301 tariffs on $200 billion of Chinese imports to 25% and threatens to impose tariffs on all Chinese imports. (05/05/2019)
10- Canada imposes tariffs on US products totaling $12.8 bn, half of these affect steel and aluminum (25% tariff rate). (01/07/2018)

The "most liberalizing" day occurred in October 2011 on which Mexico announced to suspend all its retaliatory tariffs it had imposed in response to the US blocking Mexican trucks from entering the country for several years. This event resulted in a 1.77% cumulative increase in the stock price ratio which represents 2.4 standard deviations. The second most beneficial day for US exporters marked the signing of the revised NAFTA (now: US-Mexico-Canada Trade Agreement) in 2018. Furthermore, negotiations of the Trans-Pacific Partnership (TPP) Agreement (from which President Trump later withdrew) is among the most liberalizing events. This seems reasonable considering that TPP would have been the largest new trade agreement of the last decade (member countries account for 40% of global GDP), which was estimated to increase US incomes by 0.5% of GDP and US exports by 9.1% (Peterson Institute, 2016). The remaining large positive shocks relate to a de-escalation of the tariff war between the US and China between 2018 and 2019. Since China, Mexico, and Canada are among the largest trade partners of the US and combined account for almost half of US imports and exports, the results above are in line with prior expectations.

The "most protectionist" day occurred in May 2019, when Trump announced that imports of automobiles and parts from the EU, Japan, and others pose a threat to national security and would potentially be restricted. The UK's vote to leave the EU, implying higher future barriers to trade with the US, resulted in the second largest protectionist shock. The fact that both decisions caught the public by surprise explains at least parts of the large stock price response.[17] The remaining key protectionist events refer to tariff threats by the Trump administration towards Turkey and China, as well as Canada's implementation of retaliatory tariffs on US steel and aluminium exports. Overall, the identified shocks largely align with events that would be considered crucial based on common sense. Hence, augmenting a narrative identification with stock market data allows us to gauge the size of trade shocks. This approach is particularly valuable for policy changes that encompass both tariff and non-tariff barriers, with the latter being challenging to quantify.[18]

Since we are interested in the macroeconomic impact of trade policy shocks, we aggregate the

[17] After all, the UK only accounts for approx. 3% or US trade.

[18] Examples of non-tariff barriers include import bans, restrictive licenses and lengthy customs procedures.

daily shocks into a monthly shock series.[19] The latter is depicted in Figure A.3 and will be utilized throughout the remainder of the paper. Between January 2007 and December 2019, we identify 29 trade liberalizing and 17 protectionist shocks at the monthly frequency.[20]

In addition to our baseline shocks, we build a series distinguishing between statements released by the US administration and its trade partners, and we differentiate between announcements and implementations. The insights obtained from such a differentiated analysis are valuable because they gauge the role of uncertainty and policy makers' credibility as well as the true cost or benefit of US trade actions which may depend on trade partners' responses.

1.4.2 Validating the exogeneity of trade policy shocks

In the following, we provide evidence that the identified trade policy shocks are exogenous and that augmenting stock market data with a narrative approach is crucial for exogeneity.

For our results to be valid, trade policy decisions must be unrelated to business cycle conditions. In the US, at least within the time period under consideration, the key driver of trade policy seems to be the president's political views rather than the economic cycle. This is evident in the Obama administration, which encompassed both the aftermath of the 2007-2008 crisis and subsequent expansion, yet predominantly proposed trade liberalizing measures (see Figure A.3). In contrast, the Trump administration, which began during an expansion, predominantly advocated protectionist measures. This challenges the conventional belief that protectionism is countercyclical (see, for example, Bohara and Kaempfer (2016)). Rose (2013) further underpins our observation by showing that trade policy has been acyclical since World War II. Despite this evidence, we still control for the state of the business cycle within our estimation in order to ensure that the effect we are capturing is indeed exogenous.

Moreover, we provide additional formal evidence that our shocks are not contaminated by: (a) other macroeconomic shocks, (b) the current and past economic state, (c) expectations about

[19] In some months more than one statement has been made. In Section 1.6.1 we verify the robustness of our results to different ways of aggregating shocks across the month.

[20] As shown in Figure A.5, the magnitude of more than 80% of the shocks falls between -2 and +2. Stated differently, the cumulative change in the ratio of exporting to non-exporting firms' stock index around trade policy events tends to be between -2% and 2% within a month.

future economic conditions, and (d) uncertainty. Following Caldara et al. (2020), we first look at the correlation between our identified trade policy shocks and conventional macroeconomic shocks and then test for Granger causality. Potentially confounding shocks include those related to technology, monetary policy, oil prices, and terms of trade. In line with Caldara et al. (2020), we extract technology shocks by estimating an AR(1) model of the log-difference in total factor productivity (TFP) adjusted for utilization (Fernald (2014)) and store the residuals. Terms of trade shocks are constructed in a similar way using the ratio of export and import prices. Moreover, oil price shocks are taken from Hamilton (2003). Since conventional time series of monetary policy shocks (e.g. Romer and Romer (2004)) are unavailable for our sample period, we revert to estimating a Taylor rule for the pre-sample period and calculate predicted interest rates. Deviations of the realized federal funds rate from predicted values represent monetary policy surprises and serve as shocks. To verify robustness, we also calculate predicted interest rates using equal weights for the output and inflation gaps as originally suggested by Taylor (1993). More details on how the Taylor rule is estimated and the data used can be found in Appendix A.6.1. As shown in Table A.4, our baseline trade policy shock is not Granger caused by any of the macroeconomic shocks considered, and none of the correlations is significant at the 5% level.

Furthermore, our shock is contemporaneously uncorrelated and exogenous to past economic conditions as measured by the unemployment rate, growth in industrial production, and four different indices measuring the state of the business cycle. These indicators include the recession probability from Chauvet and Piger (2020), the Coincident Economic Activity Index, the Purchasing Manager's Index (PMI), and the Chicago Fed National Activity Index.[21]

In addition, forward-looking variables such as measures of consumer and business confidence, and the real oil price do not predict our shock series.[22] Hence, policymakers do not seem to implement protectionist trade policy changes in anticipation of a recession. Only the S&P 500 return Granger causes our shock series. This is unsurprising given that we use the cumulative change in the 50 most and least export-dependent companies in the S&P 500 basket. Exporting

[21]Details on these indices can be found in Appendix A.1.

[22]Real oil prices are used in addition to oil shocks since only the former are available at the same frequency as our original trade policy shocks (i.e. monthly).

firms seem to co-move more strongly with the overall equity market than domestically focused firms. To account for this, we control for the S&P 500 in our estimations, and we also build an alternative shock series that calculates the excess return on the exporters' basket over the S&P 500.[23]

Finally, macroeconomic and financial uncertainty, as well as economic policy uncertainty (EPU), do not Granger cause our trade policy shocks. The F-test for uncertainty caused by trade policy (EPU Trade) is also insignificant at the 5% level. However, vice versa the opposite holds: our trade policy shock has substantial predictive power for EPU Trade. This index, described in Baker et al. (2016), relies on counting the number of US newspaper articles that mention economic policy uncertainty and trade policy matters. Since the identified trade policy shock is based on official policy statements, which should precede the newspaper coverage, this result is unsurprising and confirms that our shock is unanticipated. The correlation coefficient (-0.2837) for the two series has the expected sign, implying that protectionist trade policies create uncertainty while liberalization policies reduce it. However, the correlation coefficient indicates that our shock is an imperfect measure of trade policy uncertainty. Instead, it captures information beyond second-moment effects and allows for the possibility that macroeconomic effects of trade policy are driven by (expected) changes in the first moments.

Table A.5 displays the results from using the stock price ratio of exporters to non-exporters alone, disregarding trade policy statements, instead of the identified trade policy shock. This series is Granger caused by monetary policy shocks, unemployment, business confidence, and financial uncertainty, and it also correlates with terms of trade shocks. Furthermore, the stock market ratio no longer causes trade policy-induced uncertainty, reflecting that its movements may be driven by other events that are unrelated to trade.

Overall, there is strong evidence that the identified trade policy shocks are exogenous to other sources of macroeconomic fluctuations. This is not true when using stock market data alone, which underlines the merit of combining it with a narrative approach.

[23]This alternative shock series is not Granger caused by the S&P 500.

1.4.3 Estimating impulse responses by local projections

We use the local projections method (LPs) developed by Jordà (2005) to quantify the impact of trade policy shocks. Unlike the standard VAR approach, the impulse response functions (IRFs) from local projections are estimated in a series of regressions for each prediction horizon h and for each dependent variable of interest (Jordà (2005)). Among the advantages of this method is that LPs allow us to distinguish between the impact of protectionist and trade liberalizing measures. Second, they help us to detect non-linear effects depending on the size of the tariff change. These advantages, however, come at the cost of often less precise and more erratic impulse response functions and serial correlation in the error terms. The latter issue is addressed using Newey-West standard errors, which also correct for heteroscedasticity (Newey and West (1987)).

Following the notation of Ramey and Zubairy (2018), for each dependent variable x and horizon h, we estimate the following equation:

$$x_{t+h} = \alpha_h + \beta_h shock_t + \psi_h(L) z_{t-1} + \phi_h \text{trend}_t + \epsilon_{t+h} \quad \text{for} \quad h = 0, 1, 2, ...H \tag{1.3}$$

where x is one of the dependent variables of interest. These include industrial production, manufacturing sales, aggregate price indices (PPI and CPI), exports, and imports. Since we are also interested in the effect of trade policies on households and the labor market, we consider consumption, the unemployment rate, and hours worked. Furthermore, loans to consumers and businesses are proxies for investment. Where appropriate, the dependent variables are in logs and in per capita terms. The variable *shock* refers to our identified exogenous trade policy shock, described in the previous section. We include a vector of lagged control variables z, aiming to capture the state of the business cycle and other potentially confounding influences. These include the unemployment rate, the federal funds rate (FFR), the global commodity price index, the VIX as a proxy for uncertainty, S&P 500 returns, and the broad trade-weighted US Dollar Index. The latter two are important to purge our shock measure of general stock market movements and the influence of exchange rate fluctuations which may affect trade-exposed firms disproportionately. We also control for lags of both the dependent variable and the shock variable in order to capture any possible serial correlation in the trade policy variable. $\psi_h(L)$ is a polynomial of order one in the lag operator, determined using the usual optimal lag criteria. We determine the optimal lag-length in the 1-period ahead estimation ($h = 1$) and use this for the estimation of the rest of the horizons. This method, according to Brugnolini (2018), is superior to selecting a different lag-length for each horizon. A linear time trend is included where appropriate to account for the fact that the majority of the dependent variables grow at a constant rate.[24] The coefficient of interest is β_h which gives the response of x at time $t+h$ to the shock at time t. The impulse response functions are then constructed as a sequence of the β_h's estimated in a series of single regressions for each of the 20 horizons plotted.

In order to investigate whether protectionist or liberalizing shocks have asymmetric effects, we can allow the coefficient of interest to vary depending on the sign of the shock. In particular, we estimate the following equation for each dependent variable x and horizon h:

$$\begin{aligned} x_{t+h} = I_t(\alpha_h^P + \beta_h^P shock_t + \psi_h^P(L)z_{t-1}) + (1-I_t)(\alpha_h^L + \beta_h^L shock_t + \psi_h^L(L)z_{t-1}) \\ +\phi_h \text{trend}_t + \epsilon_{t+h} \quad \text{for} \quad h = 0, 1, 2, ...H \end{aligned} \tag{1.4}$$

where I_t is a dummy variable that equals 1 or 0 whenever the shock at time t is protectionist or liberalizing, respectively. Therefore β_h^P captures the impact of a protectionist trade policy shock, whilst β_h^L captures the effect of a liberalizing measure. If the difference between β_h^P and β_h^L is statistically different from zero, then the effects between a trade liberalizing and a protectionist shock are asymmetric. An analogous equation is estimated to contrast the effects of trade policy announcements and implementations and to distinguish between shocks initiated by the US and its trade partners.

Finally, in order to study non-linear effects depending on the size of the shock, we augment

[24]A time trend is included for all dependent variables except for the unemployment rate, hiring rate, hours worked, business and consumer confidence. Most results are robust to the inclusion of a quadratic trend.

Equation (1.4) to include a quadratic term of the trade policy shock, i.e.

$$\begin{aligned} x_{t+h} = \alpha_h + \beta_h shock_t + \beta_s (shock_t)^2 + \psi_h(L) z_{t-1} \\ + \phi_h \text{trend}_t + \epsilon_{t+h} \quad \text{for} \quad h = 0, 1, 2, ...H \end{aligned} \tag{1.5}$$

The effects of a trade policy shock are size-dependent if β_s is significant and if a likelihood-ratio test indicates a better fit for the augmented model than the linear-only nested model.

1.5 Results

In this section, we first describe the results from the baseline shock, which assumes that no asymmetries exist in the effects of protectionist and liberalizing shocks. We then relax and test this assumption in Section 1.5.2, demonstrating that non-linearities do not arise in the sign of the shock. We also find that the cause of the shock seems does not matter since we find no statistically significantly different effects following trade policy implementations compared to initial announcements in Section 1.5.3. However, Section 1.5.4 documents more significant responses to shocks caused by trade partners rather than by the US. Finally, Section 1.5.5 points to non-linearities in the responses of investment and trade, implying that large trade policy shocks have disproportionate effects on these variables.

1.5.1 Baseline shock

In Equation (1.3), we do not distinguish between positive and negative shocks but implicitly assume that their effects are symmetric. Hence, the following results should be interpreted as being driven by a trade liberalizing shock. Figure 1.3 shows the plots of the IRFs of the main variables of interest along with the 68% and 90% confidence intervals.

The baseline liberalizing shock leads to an increase in industrial production after seven months, and hence, the opposite should hold for a protectionist shock (Figure 1.3a). To provide an interpretation of the magnitude of the response: a liberalizing trade policy statement that triggers a cumulative change (for t_0 and t_1) in the ratio of exporters to non-exporters of 1%, increases industrial production by 0.75% at its peak, 16 months after the shock. Signing the Trans-Pacific Partnership (TPP) agreement, for example, led to an increase of 1.67% in the cumulative return on the stock price ratio. This in turn resulted in a 0.76% increase in industrial production at its peak in month 16.

The response of industrial production is predominantly driven by increased production of manufacturing goods (Figure 1.3g) rather than by other sectors such as materials or consumer durables production.[25] This result is confirmed by an increase in manufacturing sales (figure 1.3h). Considering that machinery, motor vehicles and parts, as well as chemical products, account for half of US exports (UN Comtrade (2017)) and are all part of the manufacturing sector, this result seems to point to higher foreign demand. Indeed, Figure 1.3c shows a spike in exports ten months after the shock that remains positive before dying out 16 months after the shock. Imports display similar dynamics, confirming that lower trade barriers indeed increase trade flows (Figure 1.3d).[26] Net exports increase during the first five months after the shock before showing erratic behavior (figure 1.3i). Hence, at least partial evidence exists against President Trump's claim that tariffs will improve the US trade balance.

Firms seem to boost their investment, proxied by commercial loans (Figure 1.3b), already before industrial production and manufacturing sales increase. Hence, they seem to anticipate future opportunities for exporting.[27]

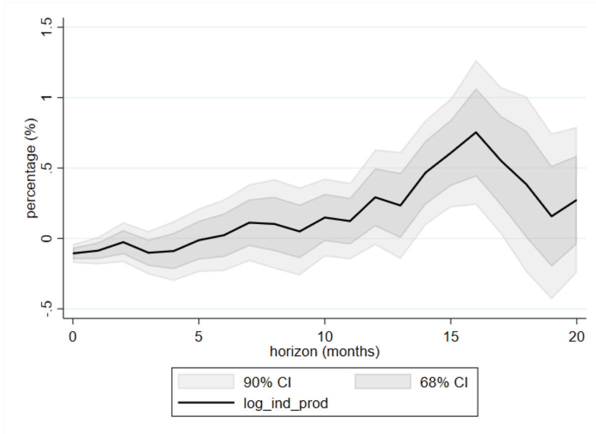
Furthermore, introducing trade liberalizing policies reduces unemployment (Figure 1.3e) and slightly increases the hire rate (Figure 1.3j). These contribute to the increase in the consumption of goods, already 4 months after the liberalizing trade policy shock, mainly driven by non-durables (Figure 1.3f). Furthermore, there is also an increase in the consumption of services. However, the response is much more muted since the majority is non-tradable.

We find no statistically significant effect of trade policy shocks on consumer and producer price levels (Figures 1.3k to 1.3l).

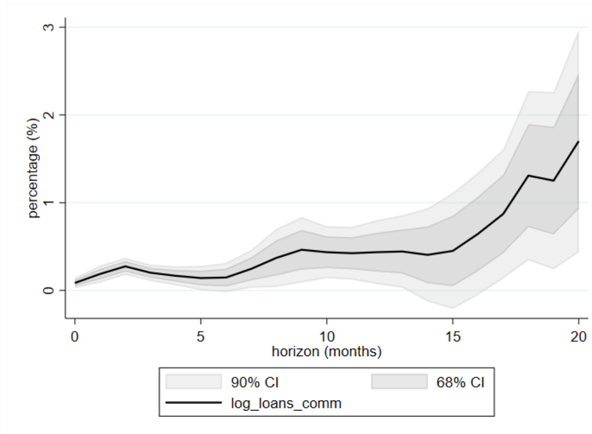
[25] Plots for non-responsive sectors are not displayed but are available upon request.

[26] The same holds when expressing imports and exports as a share of GDP as well as for trade openness (defined as the sum of exports and imports, as a percentage of GDP).

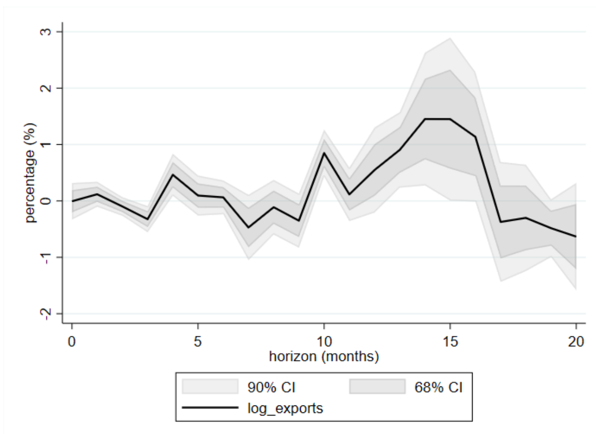
[27] The response is shown up to 20 months after the shock, which corresponds to the peak response of commercial loans to the baseline shock. The response declines afterwards.



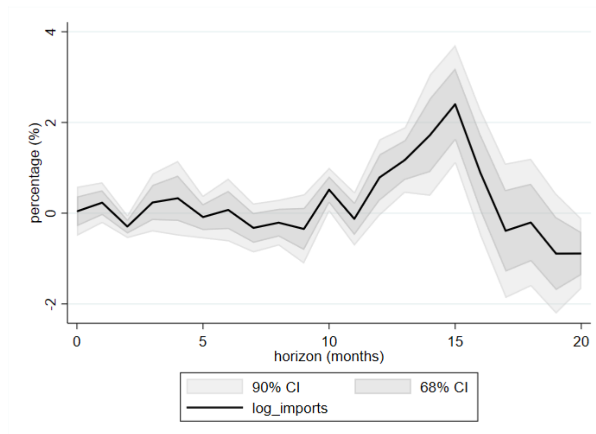
(a) Industrial production (in logs)



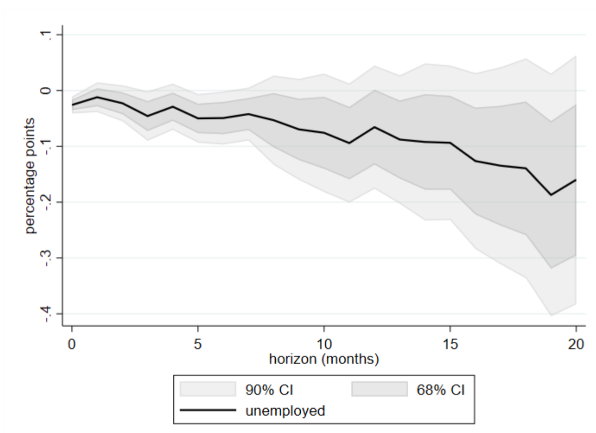
(b) Commercial loans (in logs)



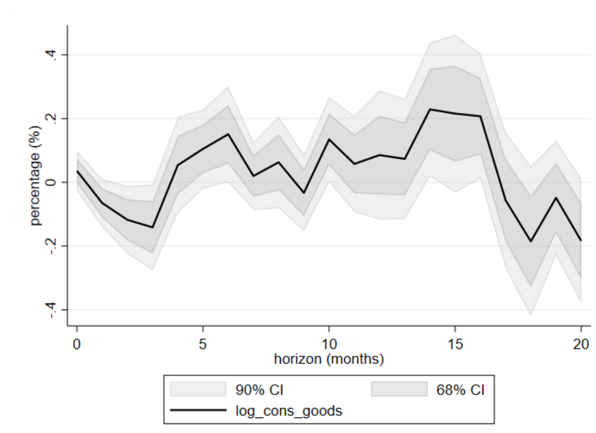
(c) Exports (in logs)



(d) Imports (in logs)

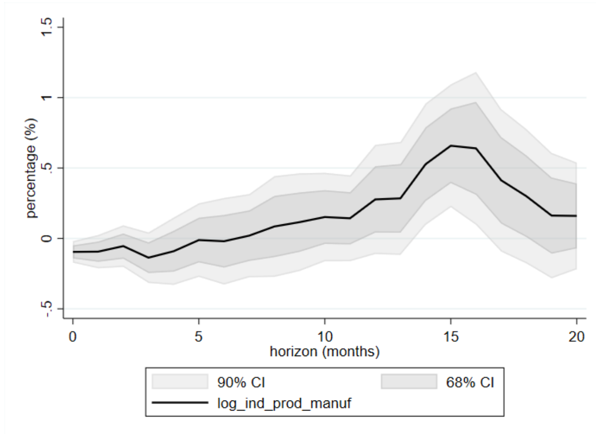


(e) Unemployment rate

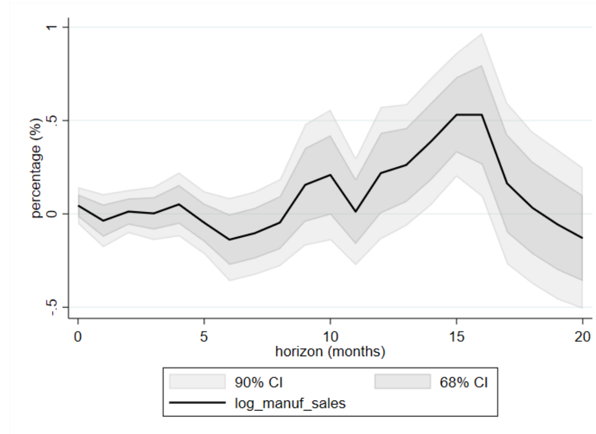


(f) Consumption of goods (in logs)

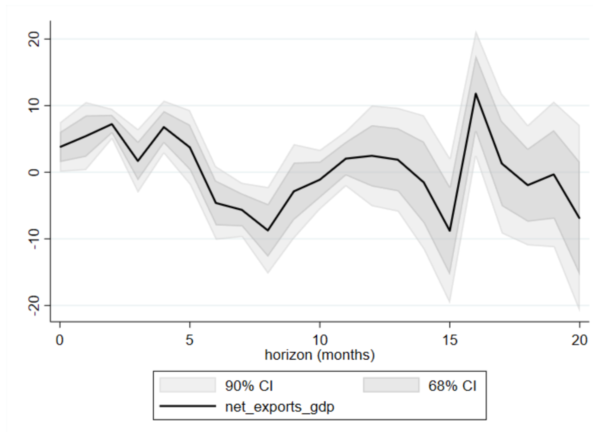
Figure 1.3: IRFs for a baseline trade policy shock



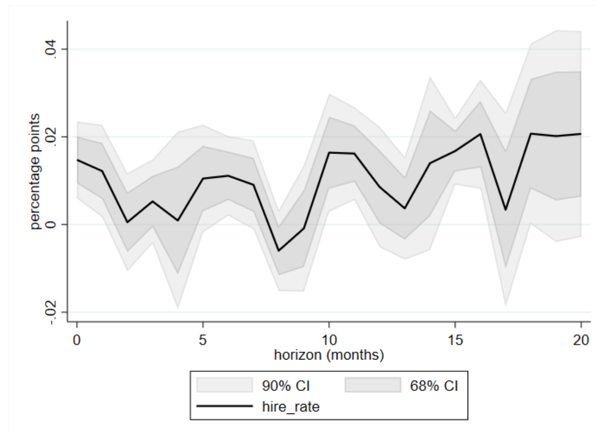
(g) Ind. prod.: Manufacturing (in logs)



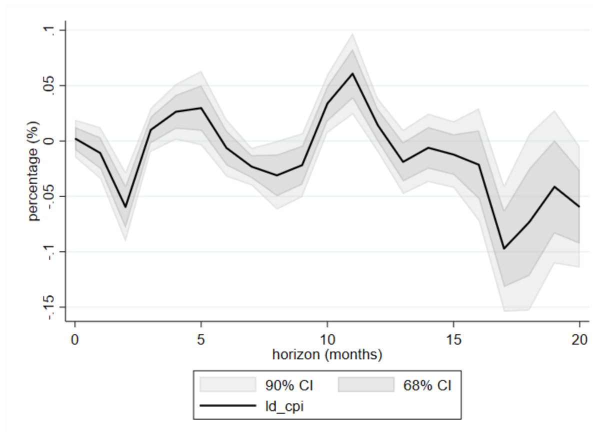
(h) Manufacturing sales (in logs)



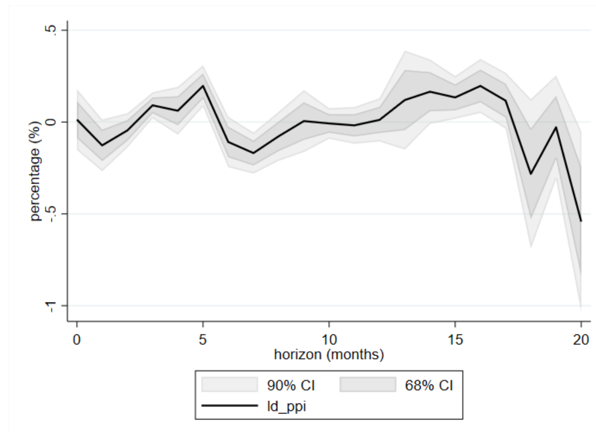
(i) Net exports (% of GDP)



(j) Hire rate



(k) Inflation rate (Consumer price index)



(l) Inflation rate (Producer price index)

Figure 1.3: IRFs for a baseline trade policy shock

1.5.2 Protectionist versus liberalizing shocks

Our identification strategy allows us to examine the presence of asymmetries in the effects of trade policy shocks with different signs by estimating Equation (1.4). The plots in Appendix A.7.1 compare the results for a liberalizing shock (first column) and a protectionist trade policy shock (middle column).[28] The right column visualizes the t-statistic from testing the null hypothesis that their response is equal.[29]

As expected, industrial production, exports, and imports increase after a liberalizing shock and decline after a protectionist shock. However, we find no statistically significant differences in the response of macroeconomic variables to liberalizing and protectionist shocks. This implies that the gains from trade liberalizations and the damage from protectionism are of equal magnitude in absolute terms, with no non-linearities observed along this dimension.

1.5.3 Announcements versus implementations

Our approach to constructing trade policy shocks also allows us to compare the effect of announcements of future policy changes and their implementations.[30] Such insights help to gauge the role of uncertainty at different stages of the policy-making process and may be useful for political leaders aiming to optimize their communication strategy.

The IRFs in Appendix A.7.2 contrast both types of shocks, and we again display the results from testing for statistically different coefficients. As for the baseline, we assume that shocks of different signs have symmetric effects, therefore the IRFs should be interpreted as responses to a liberalizing shock. This is a plausible assumption given the findings of the previous section 1.5.2, where we find that there are no non-linearities in the sign of the shock.

[28]Note that in this and the following sections, we no longer display the full set of IRFs as in the baseline. However, all plots are available upon request.

[29]Here we are interested in comparing absolute magnitudes to answer the question of whether protectionist shocks are more harmful than liberalizations are beneficial which does not seem to be the case.

[30]It is useful to keep in mind that by definition, the identified shocks associated with implementations are unanticipated, otherwise they would have been incorporated in market expectations and would not have resulted in stock price movements. Indeed, some implementations are not identified as shocks because they were anticipated. Hence, the difference between the results obtained from announcements and implementations should not be due to anticipation effects.

We find that for the majority of variables, the responses to implementations and announcements are not statistically different (Figures A.13 to A.16). However, industrial production appears to move in opposite directions following the two shocks - it increases after a liberalizing implementation but falls after a pure announcement leading us to reject the null hypothesis of equal coefficients (figure 1.4).

Implementation **Announcement** **t-statistics**

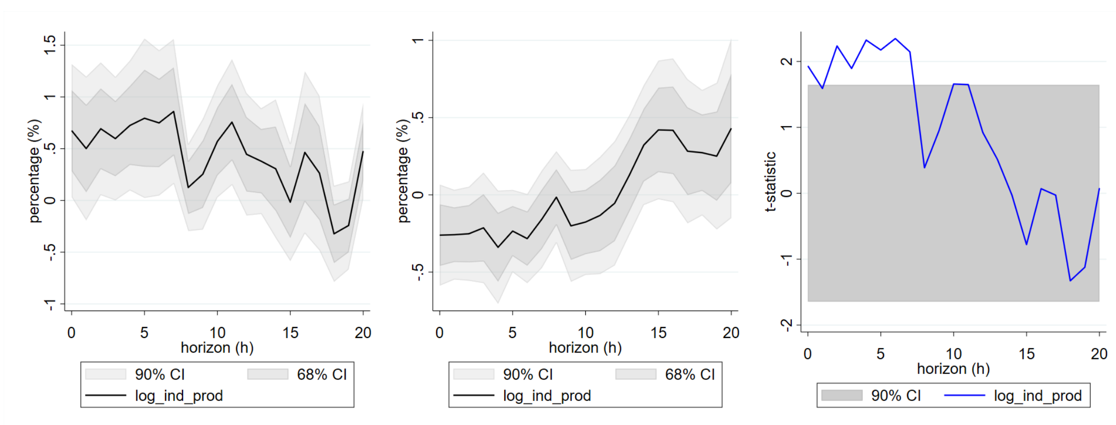


Figure 1.4: Industrial production (in logs)

The difference in responses is statistically significant for the first 6 months after the shock, which suggests that after that the response to announcements captures the effect of a potential implementation.[31]

1.5.4 Trade policy shocks initiated by the US versus its trade partners

Comparing the effects of a trade shock initiated by the US to one caused by trade partners, it appears that the latter has more significant effects on the US economy.[32] This is most evident

[31] To exclude this possibility, we compare the effects of implementations and announcements that were never implemented. We find that agents are not able to distinguish ex-ante between announcements that would later be implemented and those that would not.

[32] The estimation of a model including only a linear trend (like in the rest of the paper) resulted in anomalous-shaped IRFs, suggesting that such model is misspecified. We therefore augment the model to include a quadratic trend in addition to a linear one. Note that "US initiated" shocks also include those caused by multilateral agreements since the US is often the leading force behind such negotiations.

for commercial loans, which show a stronger positive response for twelve months after another country lowers trade barriers towards the US (Figure A.20). This seems plausible since firms may need to borrow to enter the foreign market or expand existing export capacities. Expanding import activities because of lower US trade barriers may not require the same amount of external funding for firms. To provide a rough estimate of the quantitative importance of this result, let us consider a major unilateral trade liberalization in our sample. Mexico's decision to cut tariffs by half on a wide range of US products in July 2011, resulted in a cumulative return on the ratio of exporters to non-exporters of 1.15%. Consequently, commercial borrowing may have increased by approximately 10.1% at its peak, 15 months after the shock.[33]

Furthermore, unemployment falls significantly only after a positive foreign shock (Figure A.21). Similarly, the overall consumption of goods increases following a foreign shock, before declining after 15 months (Figure A.18). In contrast, the response of consumption to a US trade shock is positive on impact but no longer significant at the 90% level after 1 month. This seems counter-intuitive as lower US import barriers should lead to cheaper consumer goods. However, the response may be driven by the employment-boosting effect of foreign trade liberalizations which fails to materialize after US liberalizations.

Since, by assumption, protectionist shocks should have the opposite effect, the above observations hint at the detrimental impact of foreign retaliations during the recent tariff war on firm investment, unemployment, and consumption. Unfortunately, when allowing for asymmetries across both initiating party and shock sign (protectionist and liberalizing), none of the responses were significant. This outcome can be attributed to the scarcity of unilateral statements, particularly those pertaining to liberalizing measures, as trade partners often make joint declarations. However, based on the insights from the previous sections, we can provide a rough estimate. For instance, the implementation of retaliatory tariffs such as by the EU on $3.2 billion of US products in June 2018 points to a decrease in US firm investment by -0.9% to -4.6% on impact.[34] It is likely that comparable declines took place following the implementation of the most potent retaliatory mea-

[33]This assumes that the effect across foreign-induced positive and negative shocks is the same.

[34]This range is based on a fall of cumulative returns of -1.15% combined with the point estimates for protectionist shocks, implementations and foreign trade policy changes.

sures by Canada and China in 2018/19. However, as the next section shows, these estimates may be further exacerbated due to non-linearities.

1.5.5 Non-linearity in shock size

The last element of our inquiry into heterogeneous trade policy effects consists of non-linearities depending on the size of the shock. Following the estimation of Equation (1.5), we plot the coefficient for the quadratic term for each horizon (first column) and the associated t-statistics together with the 90% acceptance interval (± 1.645) (middle column) in Appendix A.7.4. The third column shows the p-values at each horizon from a likelihood-ratio test of whether the linear-only nested model fares better than the model with the squared shock term.

We find evidence of size dependence on impact for all macroeconomic variables we consider in our analysis. Especially, firm investment (proxied by commercial loans) seems to be boosted disproportionately strongly for large liberalizing shocks and, assuming symmetry, would be harmed more by large protectionist shocks. The effect for most variables, including commercial loans, industrial production, exports, and imports, is long-lived since the squared shock term is significant even after twelve months. The non-linear response observed for commercial loans may be due to the limited resources available to firms, which may only be sufficient to cope with smaller trade liberalization shocks. However, in the face of more significant shocks, binding financial constraints may emerge, necessitating borrowing to expand production and trading capacity. Subsequently, borrowing may allow firms to expand their capacity disproportionately. The existence of fixed costs for engaging in international trade may accentuate the non-linear response to trade policy shocks, creating threshold effects. Future research should investigate the level of the threshold above which trade policy changes become particularly effective.

1.6 Sensitivity analysis and extensions

The following section examines the robustness of our results to different ways of constructing the trade policy shock series (Section 1.6.1) and a different definition of trade exposure (i.e. import dependency) used in our shock identification (Section 1.6.2). Furthermore, we evaluate whether the effects are different under President Trump compared to previous presidents (Section 1.6.3). Finally, we use tweets as an alternative source of trade news (Section 1.6.4).

1.6.1 Constructing alternative baseline shock

To verify the robustness of our results, we propose several alternative methods to obtain trade policy shock series. All of these yield very similar IRFs to our baseline. In Appendix A.8.1, we only plot the results for industrial production, commercial loans, consumption of goods, and unemployment since these were most significant in Section 1.5.1.

First, we generate monthly shocks from the original daily series in two alternative ways. Instead of adding up the cumulative change in the stock price ratio of exporters to non-exporters caused by statements, we take the average over the month. Figure A.26 shows that after a trade liberalizing shock, industrial production still increases with a small lag. Demand for commercial loans rises shortly after impact and stays elevated. After an initial decline, the consumption of goods picks up, and unemployment follows a persistent downward trajectory. Next, if there are several shocks within a month, we only use the larger one in absolute terms. Again, the results are robust (Figure A.27).

Furthermore, we build a shock series for which we are no longer ex-ante agnostic about the sign of the shock but only select those that are correctly identified based on a subjective classification of statements.[35] Figure A.28 confirms that the macroeconomic impact is the same as for the agnostic approach.

Moreover, instead of using the ratio of the international and domestic sales baskets, we calculate the excess return of the exporters' basket over the S&P 500 index based on a standard market model.[36] This approach ensures that none of the variation in the exporters' stock price index is

[35]Examples of protectionist, liberalizing and unclassified statements can be found in Appendix A.6. Although trade policy statements were classified into protectionist and liberalizing only if such classification could be made without doubt, a sentiment analysis based on machine-learning should be performed in the future to eliminate subjectivity.

[36]We estimate

$$ret_t^{EX} = \alpha + \beta ret_t^{S\&P500} + e_t \quad \text{for 2006}$$

caused by general market movements. Figure A.29 shows almost no deviations from the baseline.

To avoid capturing the effect of other macroeconomic news, which may lead to fluctuations in the two stock baskets, we discard announcements that coincided with monetary policy decisions by the FOMC or the publication of CPI, PPI, or labor market statistics by the BLS.[37] Our conclusions remain the same (Figure A.30).

In addition, the time window for cumulating returns is extended from two to three days, i.e. t_0, t_1 and t_2. Once again, the baseline results are confirmed.

Lastly, we perform a "placebo test" by selecting large movements in the stock price ratio of exporters to non-exporters in both directions, regardless of whether a trade policy statement was issued or not.[38] Hence, these movements may have been caused by other shocks (e.g., exchange rate or oil price shocks) that have differential effects on exporters and non-exporters. Substituting this series for our shock yields a lower point estimate and a less significant increase in industrial production and commercial loans (Figure A.31). In addition, the fall in unemployment is muted, and its downward trajectory is less persistent.

All in all, our results are highly robust to different shock aggregations, to controlling for other macroeconomic news, and to only selecting shocks with a subjectively correct sign.

1.6.2 Identifying shocks based on importers' stock price index

Our trade policy shock is based on the heterogeneous stock market reaction of exporting relative to non-exporting firms. To ensure that our results also hold when using an alternative definition of trade exposure, we use our own importer stock price index (as described in Section 1.3.2). Importing firms should be impacted by trade policy changes (e.g. tariffs) initiated by the US, jointly agreed-upon trade agreements, as well as trade partners' unilateral actions that are expected

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and then obtain excess returns from

$$\widehat{excess}_t = ret_t^{EX} - \hat{\alpha} - \hat{\beta} ret_t^{S\&P500} \quad \text{for 2007-2019.}$$

[37] This data was downloaded from: fraser.stlouisfed.org and Haver Analytics.

[38] We use stock price movements above the 99th percentile or within the 1st percentile. The former are large increases in the ratio and the latter present large falls.

to elicit retaliation by the US.

We construct the trade policy shock in the same way as described in Section 1.4.1, but substitute the importer for the exporter basket and the non-importer for the non-exporter basket. The correlation of the two shocks is high (0.82), which is partly because many exporting firms also import.

Selected IRFs obtained by using importers' relative stock prices to identify shocks can be found in Appendix A.8.2. The results are very similar to the baseline based on exporters. Hence, our results do not hinge on the particular measure of firms' trade exposure used for shock identification.

1.6.3 Analyzing trade policy shocks under different presidents

Our sample covers the time period of an abrupt departure from previous decades of striving towards trade liberalization due to the change of government in 2017. In an escalation of protectionist actions, the US government raised tariffs on approximately 16% of imports in 2018 and 2019 (Congressional Research Service (2020)). These triggered forceful retaliatory responses from trade partners resulting in the largest return to protectionism since the 1930s. We are therefore interested in exploring whether the shift towards protectionism has impacted the transmission of trade policy shocks onto the economy. To investigate this, we introduce a dummy that equals 1 between January 2017 and the end of the sample capturing the period of President Trump's administration and 0 otherwise. We interact this dummy with the baseline shock variable and include this as an additional regressor in our baseline Equation (1.3). If this interaction term is statistically significant, the effect of trade policy shocks is different for the period of the protectionist stance compared to the trade liberalizing period. We plot the t-statistics from the null hypothesis that the interaction term is not statistically different from zero, together with the 90% acceptance interval (± 1.645) in Figure A.33. If the t-statistic is positive and significant, this indicates that the effects are larger under the Trump administration compared to the other regimes.

The results reveal that for most variables, including the unemployment rate and consumption, the new administration does not alter the effect of trade policy shocks, as shown in Figure A.33. This does not hold for commercial loans, the response of which is lower during the Trump administration for the medium-term forecasting horizons considered, compared to the rest of the time period.

There are two potential explanations for the above results. First, commercial loans, which is a proxy for firm investment, is a forward-looking variable in the sense that it encompasses expectations about future economic conditions. Thus, it is highly affected by uncertainty about whether trade policy changes will be implemented and maintained. The capricious nature of announcements under Trump, with frequent amendments and revocations, may have muted their response as firms adopted a "wait and see" approach. Second, the relationship between equity price movements and the macroeconomy under Trump may have weakened due to stock market overreactions. For example, the truce during the trade war with China in January 2019 led to a similar stock market response as signing the Transpacific Partnership agreement. The former, however, simply maintains the status quo - potentially not boosting firm investment - whereas the latter implies major tariff changes.

1.6.4 Using Trump's tweets instead of official statements

We are interested in whether trade policy-relevant information disseminated through alternative communication channels has similar macroeconomic effects as official statements. To this end, we use tweets published by Donald J. Trump. In particular, we classify tweets mentioning at least one of the words "trade", "tariff", "China", or "NAFTA", and that were re-tweeted at least 20 thousand times into protectionist and liberalizing tweets.[39]

For consistency with the baseline, we start by being ex-ante agnostic about the sign of the shock. Hence, we identify a protectionist shock whenever a tweet triggers a negative return on the exporters' stock price ratio, as well as a fall in the ratio of exporters to non-exporters. A liberalizing shock is identified if the opposite stock market reactions are observed. We again aggregate the series to the monthly level and estimate the macroeconomic effects using local projections. Figure A.34 shows the IRFs that should be interpreted as being caused by a liberalizing shock. Overall we

[39] A large number of retweets ensures that the announcement received sufficient public attention and may also reflect that the information contained in the tweet caught the public by surprise, which would aid our identification of unanticipated trade news shocks.

find that the response of most variables to these shocks is more muted compared to our baseline trade policy shocks. Using only "correctly" identified shocks, i.e., tweets resulting in stock price movements that are consistent with their ex-ante classification into liberalizing and protectionist, does not change this.[40] This is because, as shown in A.8.5, Trump's tweets do not provide added, trade policy-relevant information to the market. They are usually vague and do not mention concrete trade policy changes. Instead, President Trump frequently used Twitter as a platform to threaten and complain about trade partners. As a result, once official statements are controlled for, tweets have no significant effect on the stock return of trade-exposed firms.

1.7 Conclusion

In this paper, we use daily official trade policy statements by the US and its trade partners since 2007 to uncover their effects on the macroeconomy. Exploiting the fact that trade-exposed firms react more to these statements than non-trade-exposed firms, we are able to identify and quantify exogenous trade policy shocks. Furthermore, by using the local projections method introduced by Jordà (2005), we can distinguish between the effects of trade liberalizing and protectionist measures. We also look at other asymmetries, such as the difference between announcements and implementations, shocks initiated by the US or its trade partners, and finally, non-linearities in shock size.

We find that following a trade liberalizing shock (based on the baseline shock, which treats both liberalizing and protectionist measures symmetrically), firm investment, industrial production, and manufacturing sales increase. Whilst unemployment falls after the shock, the only slightly elevated hiring rate and constant hours worked, along with just a small increase in personal income, translate into a muted response of consumption in the short-term. Unlike previous studies, such as Fajgelbaum et al. (2020) and Amiti et al. (2019), we do not find a significant effect on prices.

By using a new, comprehensive dataset and the local projections methodology, this paper

[40] Instead of using stock prices, we also included a shock series that takes a value of -1 (on days with protectionist tweets), +1 (for liberalizing tweets) and 0 if no trade-related tweet occurred. However, the results do not change.

estimates and compares the effects of different trade policy shocks. This allows us to shed light on the heterogeneous effects of trade policies, which have not been previously studied by the literature. When we distinguish between trade liberalizing and protectionist shocks, we find no statistically significant differences in the response of macroeconomic variables to the two shocks. This suggests that any gains from trade liberalizations are of equal magnitude to potential damage from protectionism. Comparing the responses to implementations and announcements, we find that there are differences in the credibility of the two types of shocks. Even after planned trade policy changes are announced, agents seem uncertain whether policymakers will follow through since most of the responses are muted. On the other hand, once policies have been implemented, uncertainty is dissolved, and firms and households react to these. Finally, it appears that the US economy responds more significantly to shocks initiated by its trade partners compared to domestically originated shocks.

Overall, the macroeconomic impact of trade policy seems multilayered and is subject to substantial heterogeneity depending on the type and origin of the policy change. Augmenting a traditional narrative shock identification with stock market data may help to isolate the unanticipated component and provides a simple way to quantify the size of shocks.

However, it is important to acknowledge that our shock identification method may not capture shocks with perfect precision. This is due to its reliance on the top 50 companies in the S&P 500 index with the highest and lowest international revenue share. These companies may not accurately represent the average size of US businesses, which tend to be smaller. As a result, the effect of trade policy statements on firms in the overall economy may only be captured imperfectly. Furthermore, different industries are impacted by different trade agreements or tariffs at varying times. While the firms in our international stock basket come from diverse sectors, capturing every round of policy changes with equal precision cannot be guaranteed. In addition, using the export or import propensity as a measure of trade exposure may not fully account for the intricate positions of firms within global value chains, potentially distorting the magnitude of trade shocks.

Nevertheless, our findings can be valuable for policymakers seeking to make informed decisions about the short and medium-term impact of trade policies on the macroeconomy. For instance, as the Biden administration conducts a comprehensive review of the Section 301 tariff actions imposed on China during the Trump era, it must carefully consider the costs and benefits involved. Based on a survey that yielded over 1,400 submissions from industry, experts, and other stakeholders, nearly 80% support the removal of these tariffs, while the remaining respondents advocate for maintaining them. This majority viewpoint aligns with the overall impact on the US economy found in our paper, as protectionist measures appear to negatively affect industrial production, trade, firm investment, and lead to increased unemployment.

Chapter 2

When it rains it pours - Climate change risks in housing markets

2.1 Motivation

The distribution of natural events such as storms, floods, and wildfires is shifting, with the frequency and severity of events already increasing due to climate change (Masson-Delmotte et al. (2021)). Real estate is particularly prone to this risk because many natural events lead to direct property destruction. Previous literature has mostly focused on estimating discount rates that capture long-term damages, given the long-lived nature of real estate assets and the previously thought long delay before the climate change risks materialize. However, there is evidence that many of the extreme natural events of the last decade, including the catastrophic floods in Germany in 2021 and the recent drought in Europe during the summer of 2022, have been made more likely because of human intervention (Seneviratne et al. (2021)). This paper, therefore, focuses on a novel channel, also explored by Correa et al. (2022), which examines whether climate change could already affect economic outcomes today due to the increasing frequency and severity of natural events.

Many studies in the literature focus on the impact of single natural events, such as hurricanes and floods, in disaster-prone areas. For example, previous studies explored how flood events such as Hurricane Katrina in New Orleans or Hurricane Sandy in New York affected real estate prices in those areas (Vigdor (2008); Ortega and Tașpınar (2018); Addoum et al. (2021)). Similarly, other studies focus on the responses of real estate prices to future flood risks from sea-level rise (Bernstein et al. (2019); Baldauf et al. (2020); Keys and Mulder (2020); Giglio et al. (2021); Bakkensen and Barrage (2021)). However, the literature has yet to reach a consensus on the effect and impact of these risks, with different studies often getting contradicting conclusions. Moreover, previous studies tend to focus on the *local* impact of physical risk of climate change on affected areas and how differential property exposure affects real estate prices.

However, as agents experience or become aware of more frequent and severe natural events, such as floods, heatwaves, or wildfires, compared to even a decade ago, they understand that the distribution of natural disasters is changing. For example, media attention on climate change has heightened considerably over the last decade, raising agents' awareness (Boykoff et al. (2022)). Changing expectations about future disasters feeds into the pricing of such disasters in present terms. This does not only imply that climate change could already affect economic outcomes today but also could give rise to *indirect effects* in other unaffected, but *at-risk* areas. For example, an increasing frequency and severity of floods on the left bank of a river could still influence the real estate prices $50km$ upstream on the right bank of the river, which is exposed to the same type of physical risk.

This paper investigates the presence and magnitude of *direct* and *indirect* effects of natural disasters attributed to climate change on residential real estate prices. The key contribution of this paper is the focus on the presence and magnitude of indirect, spillover effects that have not been previously studied in the literature. The research question has important policy implications, especially regarding financial stability, given the role of residential real estate as collateral for banks.

The identification strategy relies on locations ex-ante exposed to climate change-related natural events but not directly affected by the event. This paper exploits detailed post-code level data on residential real estate prices from the largest online platform for real estate listings in Germany and post-code level data on the ex-ante physical risk to different natural events, such as flooding and wind storms. The paper focuses on Germany because climate change awareness is much higher in Germany compared to the United States. In 2021, only 8% of German people believed that climate change is not a serious problem, compared to around 30% of people in the U.S. who believed that global warming is not real (Leiserowitz et al. (2021); European Commission (2021)). Agents' awareness of climate change is an important transmission mechanism in generating indirect or spillover effects.

Climate change shocks are identified using exogenous temporal and spatial variation in the occurrence of natural events, such as floods and wind storms, in Germany from 2013 to 2021. As a second step, I estimate the static effects of climate change shocks on residential real estate prices in *directly affected* areas and in areas that are not directly affected by climate change shocks but are *close* to the affected areas. Closeness is defined in two ways: "geographical closeness", which refers to locations being close in distance, and "geological closeness", which refers to locations sharing a similar risk profile to a certain type of natural disaster. The empirical analysis sheds light on two types of spillover effects of physical risk. I find that, first, climate change shocks have a significant negative effect on real estate prices in municipalities that share a similar risk profile to the affected municipalities, but are themselves not directly affected by the shock. This effect is not present in unaffected areas of lower physical risk. Secondly, real estate prices are lower in unaffected municipalities that are geographically near the municipalities hit by the shock. The result is robust to controlling for the ex-ante physical risk of the neighbouring municipality.

In an extension, I investigate how differential property exposure affects real estate prices. Focusing on flooding risk, property exposure is identified if a property is on the ground floor or has a basement. I find that real estate prices of properties with higher exposure to flooding risk decline more than those of non-exposed properties following a climate change shock. The result is in line with previous findings in the literature, which show that properties that are more exposed to flooding risk experience a bigger increase in real estate prices after climate change adaptation measures have been implemented (Benetton et al. (2022)).

As a supplementary analysis, this paper investigates the role of media attention in propagating climate change shocks. The baseline shock is interacted with an index of media attention, which captures the number of articles in Germany's most prominent national newspapers that mention each natural event in the ten days succeeding the event. This alternative shock specification does not affect the results from the baseline shock estimation. The results also suggest that natural events that attract more media attention do not elicit a stronger response in real estate prices to climate change shocks.

Related Literature. This paper contributes to the growing literature investigating the effect of climate change on housing markets. Several studies focus on the effects of single natural events, such as the flooding following Hurricane Katrina in New Orleans or Hurricane Sandy in New York, or the effects of sea level rise, which find mixed results (Beltrán et al. (2018); Vigdor (2008); Ortega and Taşpınar (2018); Addoum et al. (2021)). Unlike other papers in the literature, this paper studies the effects of different physical risks, and is not constrained to a single natural event, such as flooding. Furthermore, another novelty of this paper is that it exploits cross-sectional variation across municipalities to study the effects of climate change on housing markets. This enables me to study the effects on unaffected areas that share a similar risk profile or are geographically close to affected areas, which are overlooked by the literature.

A key propagation mechanism that could result to spillover effects is agents' changing beliefs about climate change. Baldauf et al. (2020) study whether beliefs about long-run climate change risks are incorporated into house prices. They find that houses projected to be underwater in believer neighborhoods in Florida sell at a discount compared to houses in denier neighborhoods.

Giglio et al. (2021) and Benetton et al. (2022) explore how the prices of properties that are deferentially exposed to climate risk change in response to a change in that climate risk. Giglio et al. (2021) exploit time-series variation in attention paid to climate risk in the housing market, whilst Benetton et al. (2022) focus on how climate adaptation measures affect real estate prices by focusing on the activation of the sea wall in Venice. Both studies use variation in the floor of the property and elevation of the property to identify exposure to physical risk from flooding. This paper answers a slightly different but related question, namely what is the effect of an increase in actual and expected risk of natural events. Unlike these studies, this paper does not focus solely on differential property exposure, but also on how location exposure affects real estate prices following climate change related events.

The paper is also related to the literature studying the interaction between lending and climate change. Sastry (2022) studies how residential mortgage contracts distribute flood risk exposures across banks, households, and the government flood insurer. On the other hand, Correa et al. (2022) study how corporate loan costs are affected by climate change-related natural disasters. The authors disentangle the direct effects of disasters from the effects of lenders updating their beliefs about the impact of future disasters. This paper also exploits belief updating as a mechanism through which climate change shocks are propagated to other non-affected areas *at-risk*, but focuses on residential real estate prices.

The remainder of the paper is organized as follows. Section 2.2 outlines a conceptual framework to formulate the hypotheses under investigation. Section 2.3 describes the key datasets and presents descriptive evidence. Section 2.4 shows the direct and indirect effects of climate change shocks on residential real estate prices. Section 2.5 explores the secondary question of the role of media in propagating climate change shocks in real estate markets. Finally, section 2.6 concludes.

2.2 Hypothesis development

This section outlines a conceptual framework of how climate change shocks affect residential real estate prices. It outlines the three hypotheses that are tested empirically.

Consider an infinite-horizon economy, consisting of N distinct and heterogeneous islands inhabited by agents that live for two periods. The islands are geographically separated and there is no trade or exchange of population between them. Agents receive utility from the consumption of non-durable consumption goods and housing. Agents receive an income endowment when young and choose how much to consume in a non-durable consumption good, in a housing good and how much to save in bonds. Agents buy housing when young at price p_t and sell it at old age, at price p_{t+1}. They use the proceeds from the housing sale and their savings in bonds to finance their second-period consumption of non-durable goods. The housing stock in each island grows at a fixed rate, which corresponds to the island-specific population growth rate.

The islands are hit by natural events, which are realizations of climate change shocks, leading to property destruction. This makes housing a risky good and property prices adjust in equilibrium to reflect this risk. The islands vary in their exposure to different types of physical risk, and therefore the probability of being hit by different climate change shocks. In particular, each island i has a probability π_i^n of being hit by natural event n (where n is flood, windstorm, etc.). Agents know their island's probability of being hit by a natural event.

Climate change shocks are drawn from a distribution that is shifting over time, due to climate change. In particular, both the size and frequency of shocks increase over time. Agents are unaware of the exact distribution and therefore form expectations over it, based on the realizations of the shock that they observe. Each shock realization acts as a signal that the agents use to update their expectations.

Agents observe the entire history of shocks in their own island, but not in other islands. Instead, they are informed by the media about natural events occurring in other islands. The media report large-scale natural events with higher probability, compared to small-scale, local events. When forming expectations, agents assign more weight to signals (i.e. shocks) that occur in islands closer to them, compared to islands that are physically more distant from their own island.

This leads to the following hypotheses:

Hypothesis 1: Following the realization of climate change shocks, areas that share a similar risk profile to the affected areas (i.e. "geologically close") will experience a drop in residential real estate prices, even if they have not been directly affected by the shock.

Hypothesis 2: Following the realization of climate change shocks, there is a bigger effect on real estate prices in areas *neighbouring* affected areas (i.e. "geographically close"), compared to non-neighbouring areas, even if they have not been directly affected by the shock.

Hypothesis 3: Properties more exposed to physical risk face a more significant price fall than non-exposed properties in areas where climate change shocks are realized.

Hypothesis 4: The pricing of climate change shocks is more pronounced the more media attention they attract.

The hypotheses are tested using different specifications of panel regressions, including a range of fixed effects. Detailed data on property prices and municipality exposures to different physical risk events are used, described in more detail in Section 2.3.

2.3 Data and Descriptive Evidence

This paper empirically estimates the effect of natural events attributed to climate change on residential real estate prices. To do so, a novel data set is introduced, which combines geo-spatial data on the occurrence of natural events with ex-ante physical risk and geo-located residential real estate prices and property characteristics. The monthly dataset focuses on Germany and spans 2013-2021. This section describes the key datasets and how they are combined.

2.3.1 Realizations of natural events

In order to identify climate change shocks, a timeline of major natural events that occurred in Germany throughout 2013-2021 is used. The timeline and geo-location of natural events were obtained from *Copernicus*, an EU program using satellite Earth Observation and in situ (non-space) data to provide timely information on the occurrence of natural events.

Between 2013 and 2021, there were ten major flooding episodes and four major windstorm episodes. The natural events affected a total of 209 municipalities in the time period under consideration[1]. In total, 209 municipalities have been affected by the natural events in the time period under consideration.

Figure 2.1 shows the monthly time series of the natural events and how many locations they have affected[2] and figure 2.2 shows the municipalities affected by such events during the time

[1]A municipality consists of a collection of post-codes and constitutes the lowest level of official territorial division in Germany. In total, there are 11,014 municipalities in Germany.

[2]Some of the flooding and windstorm events occurred in the same month. The shocks are aggregated to the monthly frequency.

period under study. Section 2.4.1 explains in more detail how this dataset is used to identify climate change shocks.

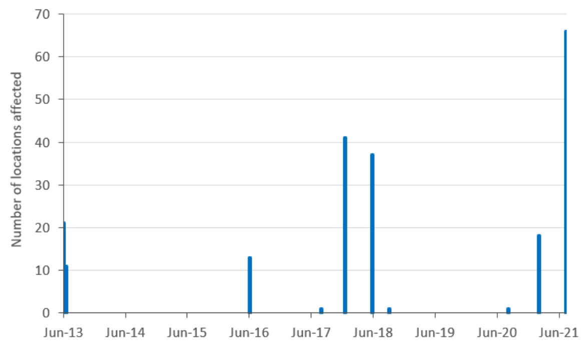


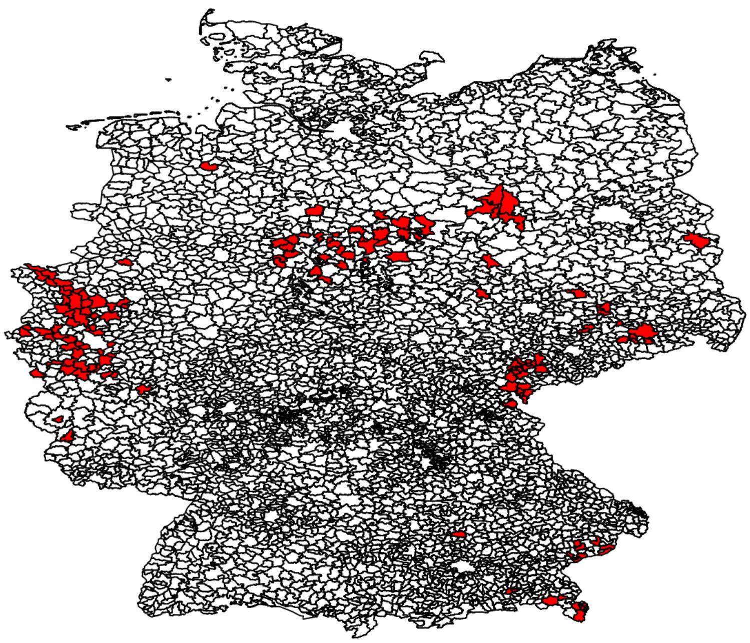
Figure 2.1: Time-series and cross-sectional variation in climate change shocks.

2.3.2 Housing data

The dataset on housing entails asked prices and rents of residential properties across Germany, posted on ImmobilienScout24, Germany's largest real estate listing website[3]. Information is available on flats and houses for sale or rent. The dataset includes the asking price for the property as well as detailed information on property characteristics, such as the number of bedrooms and bathrooms, the floor of the object, and whether a property has a basement, among others. The location of the property is also included on the post-code level. A list of all the variables available in the dataset can be found in Appendix B.1. Given that most of the information about a property in this dataset is provided by users and is, therefore, error-prone, an extensive data-cleaning procedure is performed. Details on the cleaning procedure we follow are provided in Appendix B.1. For the subsequent analysis, I focus on houses and apartments available for sale. The asking prices are deflated using monthly, federal state specific CPI index (all items).

[3]Immobilinescout24 contains roughly 50% of the advertised real estate objects in Germany (Georgi and Barkow (2010)).

Figure 2.2: Occurrences of natural events (flooding) across Germany.



Note: Red areas indicate the municipalities affected by the shock.

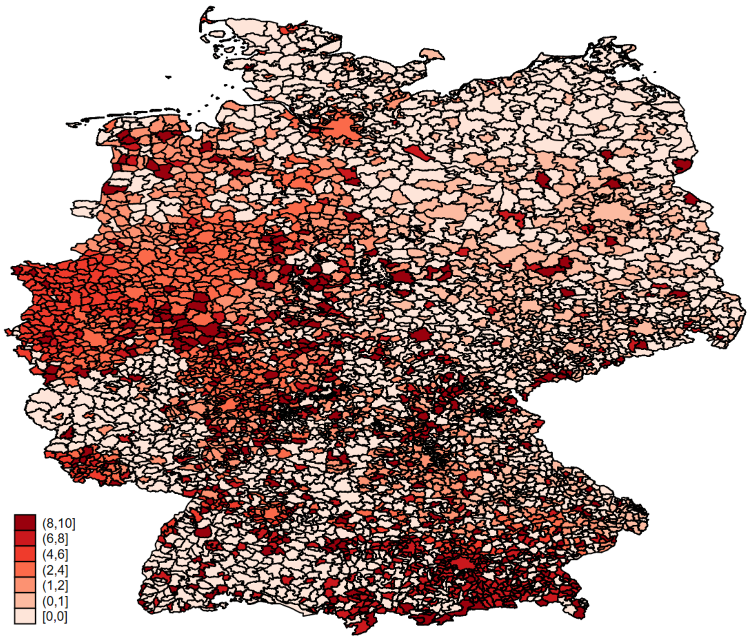
2.3.3 Ex-ante physical risk

Ex-ante physical risk refers to how exposed a specific location is to different natural events. Data on ex-ante physical risk is obtained from the data provider *Four Twenty Seven (427)*. The dataset contains separate climate risk scores at the post-code level for six different physical risk phenomena: floods, sea rise, hurricanes and typhoons, heat stress, and water stress. The score captures the frequency and severity of future extreme weather events and is based on meteorological models that use historical data to predict the likelihood of future events. The ex-ante physical risk score is time-invariant.

For each post-code in Germany and each natural event, there is a score between 0-100, with the highest indicating the highest level of risk for that post-code and that natural event. For example, post-codes in a floodplain will have a higher flooding ex-ante risk score than post-codes in higher elevation areas.

The data is aggregated at the municipality level using the average risk score across all post-

Figure 2.3: Distribution of physical risk (flooding) across Germany.



Sources: 427 and own calculations.
Note: Darker colours indicate higher ex-ante physical risk locations.

codes in the municipality.[4] For each natural event, each post-code is grouped into one of the following categories, based on their ex-ante risk: "low risk", "medium-low risk", "medium-high risk" and "high-risk", based on the quartile of the distribution in which they belong. Figure 2.3 displays the ex-ante physical risk distribution for flooding across Germany, where the darker the colour, the higher is the location's ex-ante physical risk for flooding.

2.4 Direct and spillover effects on house prices

2.4.1 Shock identification

Climate change shocks are identified through the timeline of major natural events that occurred in Germany throughout 2013-2021, as described in Section 2.3.3 above. Natural events provide exogenous variation over time and space, given that each natural event has affected several mu-

[4]Alternative aggregation methods, such as using the median physical risk score across the post-codes in the municipality, yield similar results

nicipalities at the same time[5].

In total, 209 municipalities have been affected by the natural events in the time period under consideration, of which only six have been affected more than once. This finding confirms the exogeneity of natural events in the sample, given that the events do not repeatedly affect the same municipalities, suggesting that agents expect or anticipate such events and, therefore, already incorporate them in house prices. Instead, for most municipalities affected, only a single event occurred during the period under study. Therefore, this is unlikely to have been anticipated or incorporated into agents' expectations and, therefore, in real estate prices.

The baseline shock variable, $Shock_t$, is a dummy that equals 1 on the month that the natural event occurred, and 0 otherwise. This variable captures solely the timeline of natural events. The baseline specification of the shock does not distinguish between flooding and windstorm events. Figure 2.4 shows the time series of the baseline shock. Additionally, $Exposure_{mt}$ is a dummy that equals 1 if municipality m was affected by the natural event at time t, and 0 otherwise. This variable captures the realizations of the shock.

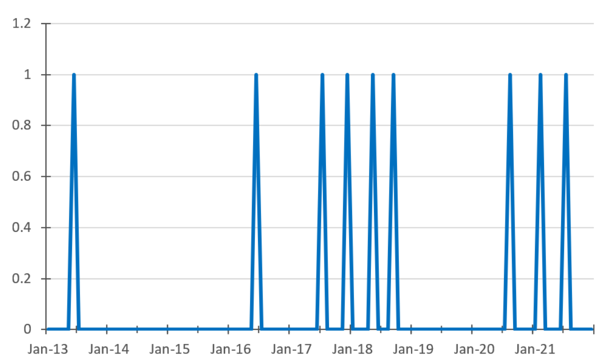


Figure 2.4: Baseline monthly shock. Based on dummy which equals 1 if the shock occurred in a given month and 0 otherwise.

[5]To verify the exogeneity of the shocks, I check whether local or national newspapers mention the event in the ten days prior to the occurrence. However, this should not be a concern because of the illiquidity of housing markets which implies that information is not priced in, in such short amount of time.

2.4.2 Static effects

To study the effect of the shock on house prices in different areas, the following equation is estimated:

$$y_{imt} = \alpha Shock_t + \beta Exposure_{mt} + \gamma_1^k \sum_{k=1}^{3} Risk(k)_m \times Shock_t + \gamma_2^k \sum_{k=1}^{3} Risk(k)_m \times Exposure_{mt}$$
$$+\delta Neighbour_{mt} + \theta X_{imt} + \lambda_m + \zeta_t + \epsilon_{imt} \tag{2.1}$$

where y_{imt} is the (log) real price per square meter for property i, at time t in municipality m. $Risk(k)_m$ indicates the quartile $k = 2, 3, 4$ in the ex-ante risk distribution that municipality m belongs to. $Risk(1)_m$, which refers to municipalities with the lowest ex-ante physical risk, is excluded from the specification and acts as the reference dummy. $Neighbour_{mt}$ is a dummy variable that equals 1 if municipality m belongs to a region in which the natural event occurred in time t, but is itself unaffected by the natural event, and 0 otherwise. X_{imt} includes property characteristics that could affect property prices[6]. The specification also includes time fixed-effects (ζ_t) and municipality fixed-effects (λ_m), to account for unobserved variation in real estate prices over time and space[7]. Finally, ϵ_{imt} is an error term that captures the unobservable determinants of real estate prices.

The results for different specifications of estimation of equation 2.3 are summarized in Table 2.1. While the coefficient of the shock variable is small, positive, and statistically significant, the coefficient of $Exposure_{mt}$ is negative. This indicates that, as expected, real estate prices of properties in municipalities that have been directly affected by the natural event fall following a climate change shock. The fall in real estate prices in directly affected municipalities ranges between 5 and 18%.

When considering the physical risk category in which a municipality directly affected by the

[6]Property characteristics include the type of property (house or flat), the number of rooms, number of bathrooms, presence of cellar in a property, the age of the property and the quarter in which the property was taken off the market. The results are robust to including alternative property characteristics in the estimation.

[7]For specifications that include municipality-level variables that are fixed over time (such as $Risk(k)_m$), region fixed-effects are included instead of municipality fixed-effects.

Table 2.1: Responses of real estate prices to climate change shocks.

	log(real price per square meter)				
Shock	0.0131***	0.0137***	0.0618***	0.0359***	0.0324***
	(0.00337)	(0.00385)	(0.00341)	(0.00386)	(0.00325)
Exposure	-0.178***	-0.168***	-0.0543***	-0.0183	-0.0387***
	(0.0119)	(0.0168)	(0.0012)	(0.0170)	(0.0114)
Medium-low risk × shock		0.00252		0.00777***	
		(0.00292)		(0.00292)	
Medium-high risk × shock		-0.0575***		0.0164***	
		(0.00326)		(0.00341)	
High risk × shock		-0.0350***		-0.0314***	
		(0.00361)		(0.00361)	
Medium-low risk × exposure		0.0645**		0.0589**	
		(0.0252)		(0.0252)	
Medium-high risk × exposure		-0.195		-0.179	
		(0.182)		(0.182)	
High risk × exposure		-0.143***		-0.145***	
		(0.0459)		(0.0459)	
Medium-low risk		-0.0564***		-0.0564***	
		(0.000555)		(0.000555)	
Medium-high risk		-0.0887***		-0.0887***	
		(0.000775)		(0.000775)	
High risk		-0.187***		-0.187***	-0.167***
		(0.000693)		(0.000693)	(0.000711)
Neighbour			-0.224***	-0.169***	-0.095***
			(0.00225)	(0.00232)	(0.0022)
High risk × neighbour					-0.143***
					(0.0015)
Property characteristics	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	No	Yes	No	No
Region FE	No	Yes	No	No	No
Observations	8,713,476	8,713,476	8,713,476	8,713,476	8,713,476

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

shock belongs, I find that the fall in real estate prices is driven by the response of properties belonging to municipalities in the highest risk category. The coefficient of *High risk* $\times$ *exposure* variable indicates that following a climate change shock, real estate prices decline significantly by around 14% in municipalities that belong to the highest risk category and have been directly affected by the event, relative to those municipalities in the lowest risk category. On the other hand, real estate prices in municipalities belonging to the medium-low risk category increase slightly compared to those in the lowest risk category. In contrast, the coefficient for the medium-high risk category is not statistically significant.

Non-realized risk

The first hypothesis to be tested is the presence of *spillover effects* in unaffected areas that share a similar risk profile as affected areas, following climate change shocks. The key coefficients are γ_2^k, where $k = 2, 3, 4$, which capture the response of real estate prices in unaffected areas based on which physical risk category the municipality belongs to. The coefficient of *High risk* $\times$ *shock* is -0.03 and is statistically significant. This indicates that properties in municipalities that have been unaffected by the shock but belong to the high-risk category for a given natural event are 3% lower compared to properties in municipalities that belong to the lowest-risk category. A similar effect is present for properties that belong in municipalities of medium-high risk, although the response is not consistent across different specifications of equation 2.3. Notably, the coefficient for municipalities of medium-low risk unaffected by the shock is zero, indicating that the response for low and medium-low risk is similar.

These results favour *Hypothesis 1*, namely that following a climate change shock, real estate prices decline not only in affected areas but also in unaffected areas of a similar risk profile. The findings indicate the presence of spillover effects only in areas of high ex-ante risk of the physical event, and there are no statistically significant effects in unaffected areas of no or low risk.

Table 2.2: Responses of real rental prices to climate change shocks.

	log(real rental price per square meter)				
Shock	-0.00644***	0.0184***	0.0227***	0.0305***	-0.00215
	(0.00211)	(0.00216)	(0.00214)	(0.00218)	(0.00192)
Exposure	-0.127***	-0.104***	-0.0446***	-0.0529***	0.0154**
	(0.00758)	(0.00951)	(0.00766)	(0.00958)	(0.00688)
Medium-low risk × shock		-0.0196***		-0.0211***	
		(0.00170)		(0.00170)	
Medium-high risk × shock		-0.0486***		-0.0250***	
		(0.00181)		(0.00189)	
High risk × shock		-0.0225***		-0.0319***	
		(0.00286)		(0.00286)	
Medium-low risk × exposure		0.0703***		0.0700***	
		(0.0148)		(0.0148)	
Medium-high risk × exposure		-0.169***		-0.174***	
		(0.0489)		(0.0489)	
High risk × exposure		-0.353**		-0.343**	
		(0.162)		(0.162)	
Medium-low risk		0.0336***		0.0336***	0.0321***
		(0.000324)		(0.000324)	(0.000343)
Medium-high risk		-0.0303***		-0.0303***	-0.0315***
		(0.000353)		(0.000353)	(0.000460)
High risk		0.0629***		0.0629***	0.0635***
		(0.000406)		(0.000406)	(0.000430)
Neighbour			-0.107***	-0.0599***	-0.00929***
			(0.00141)	(0.00136)	(0.00129)
High risk × neighbour					-0.0981***
					(0.000882)
Property characteristics	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	No	Yes	No	No
Region FE	No	Yes	No	No	No
Observations	7,612,158	7,612,158	7,612,158	7,612,158	7,612,158

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Neighbouring effects

In order to evaluate *Hypothesis 2*, regarding the presence of neighbouring effects, the key coefficient of interest is δ. The coefficient captures the response of real estate prices in a municipality that is close in distance to a municipality hit by the climate change shock, but itself has not been directly affected by the shock. The coefficient is large, negative, and statistically significant, ranging between -0.10 and -0.22 depending on the specification of equation 2.3. Importantly, the last column of Table 2.1 indicates that the results are not entirely driven by the location's ex-ante physical risk. After controlling for the neighbouring municipality being in an unaffected area of a similar risk profile as the affected area, the results indicate that real estate prices are still 9% lower compared to non-neighbouring municipalities. Furthermore, being in an unaffected neighbouring area of a similar risk profile as the affected area, suggests that real estate prices are even lower compared to being in a neighbouring area of lower physical risk. **The results are in favour of *Hypothesis 2***, since real estate prices fall in municipalities that have themselves been unaffected by the climate change shock, but are geographically close to affected municipalities.

Overall, the results confirm the presence of two spillover effects of physical risk in real estate markets: first, real estate prices fall in unaffected municipalities that have a similar risk profile to affected municipalities, and second, real estate prices fall in unaffected municipalities that are geographically close to municipalities hit by the shock.

2.4.3 Effects of differential property exposure

The dataset on real estate prices provides rich information on property characteristics, allowing us to explore how differential property exposure affects real estate prices following a climate change shock. Similar to Benetton et al. (2022) and Giglio et al. (2021), this exercise focuses solely on flooding risk because it is more straightforward to identify property exposure to this risk compared to other types of physical risk events, such as windstorms. While Benetton et al. (2022) focus on how climate change adaptation measures affect the real estate prices of exposed properties, this paper focuses on the opposite effect.

This section tests *Hypothesis 3*, namely that properties more exposed to flood risk face a more significant price fall than non-exposed properties, in areas where climate change shocks are realized. Furthermore, an additional question addressed is whether the real estate prices of exposed properties react differently in areas of different ex-ante physical risk.

Property exposure to flooding risk is identified in two ways: the floor of the property and the presence of a basement in the property. Being on a ground floor or having a basement as part of the property increases the chances of flooding relative to properties in higher elevations or that do not have a basement.

To compute the effect of differential property exposure following climate change shocks, the following specification is estimated:

$$\begin{aligned} y_{imt} &= \alpha PropertyExposure_i + \beta PropertyExposure_i \times Exposure_{mt} + \\ &\gamma^k PropertyExposure_i \times \textstyle\sum_{k=1}^{3} Risk(k)_m + \theta X_{imt} + \lambda_m + \zeta_t + \epsilon_{imt} \end{aligned} \tag{2.2}$$

where $PropertyExposure$ is a dummy that equals 1 if the property is either on a ground floor or the property has a basement, and 0 otherwise.

Table 2.3 shows the results from the estimation of equation 2.2. While the presence of a cellar increases the value of the residential property, having a cellar in a municipality which the shock has hit leads to lower real estate prices by 3%, compared to properties that do not own a cellar in an affected location. Furthermore, having a cellar in a high-risk municipality leads to lower real estate prices than the absence of a cellar.

On the other hand, being on the ground floor reduces the property's price compared to being on a higher floor. The results suggest that there is no statistically significant effect on real estate prices of being on the ground floor in an affected area. However, after the climate change shock, real estate prices are 2% lower for ground-floor properties compared to properties on higher floors. This is in line with expectations, given that those properties are more likely to be affected by flooding shocks in the future.

Overall, this finding is in line with the results of Benetton et al. (2022), who find that properties on ground floors experience a more significant increase in real estate prices compared to properties on higher floors after the introduction of climate change adaptation measures in Venice.

Table 2.3: Responses of real estate prices based on differential property exposure.

	log(real price per square meter)			
Shock	-0.051***		0.038***	0.036***
	(0.0035)		(0.0056)	(0.0055)
Exposure	-0.162***		-0.248***	-0.233***
	(0.0174)		(0.0186)	(0.0170)
D.cellar	0.196***			
	(0.0004)			
D.cellar × shock	0.131***			
	(0.0019)			
D.cellar × exposure	-0.037**			
	(0.0185)			
D.cellar × D.High risk		-0.012***		
		(0.0008)		
D.ground floor			-0.013***	-0.014***
			(0.0009)	(0.0010)
D.ground floor × shock			-0.022***	
			(0.0046)	
D.ground floor × exposure			0.037	
			(0.0462)	
D.ground × D.High risk				-0.0017***
				(0.0020)
D.High risk		0.051***		0.056***
		(0.0005)		(0.0007)
Property characteristics	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	No	Yes	No
Region FE	No	Yes	No	Yes
Observations	8,713,476	8,713,476	2,690,057	2,690,057

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.4.4 Dynamic effects

Despite the large and statistically significant effects of climate change shocks on property prices within the month of the shock, a crucial inquiry that warrants further investigation pertains to the potential transitory or persistent nature of these effects. Ideally, in order to address this question, the dataset on house and rental prices should be structured as a panel, enabling the execution of repeat-sales analyses. However, this is not possible given that the same property is not sold or rented multiple times over the time period under consideration.

In order to study the dynamic effects of the climate change shocks, for each horizon h the following equation is estimated:

$$\begin{aligned}
&y_{im,t+h} = \alpha_h Shock_t + \beta_h Exposure_{mt} + \gamma^k_{1,h}\textstyle\sum_{k=1}^{3} Risk(k)_m \times Shock_t \\
&+\gamma^k_{2,h}\textstyle\sum_{k=1}^{3} Risk(k)_m \times Exposure_{mt} + \delta_h Neighbour_{mt} + \theta X_{imt} + \lambda_m + \zeta_t + \epsilon_{imt} \\
&\text{for } h = 0, 1, 2, 3 \ldots H
\end{aligned} \tag{2.3}$$

where $H = 24$. The coefficients of interest are α_h, β_h and δ_h which give the dynamic response of y at time $t + h$ to the shock at time t. Furthermore, $\gamma_{1,h}$ and $\gamma_{2,h}$ capture the dynamic effects of the shocks in areas that vary in risk exposure to the natural event. The results are shown as impulse response functions, which are constructed as the sequence of the coefficients which are estimated in a series of single regressions for each of the H horizons.

The results show that the dynamic effects are not permanent. Most effects are significant within the first 6 months of the shock, and slowly die afterwards.

2.5 Propagation through media attention

This section extends the main analysis to address the following question: what is the role of media in propagating climate change shocks? Media is an important channel for transmitting a range of information to agents. Over the last decade, there has been increasing media attention to climate change and its effects (Boykoff et al. (2022)). This can affect agents' views and expectation formation, both directly and indirectly. Given the role of expectations in the transmission mechanism of shocks to real estate prices, it would be interesting to investigate how much the media affects these expectations and, in turn, the result on real estate prices.

An index of media attention is constructed using data from *Factiva.com*, which offers a broad collection of sources, including national and international newspapers. In the ten-day window following each natural event, the index of media attention captures the number of articles in the most prominent national newspapers in Germany that mention each event[8]. The keywords included in the search are "Germany", along with "flooding" or "flood(s)" for flooding events and "wind storm(s)" for wind storm episodes. Furthermore, a subset of those articles is distinguished, in which the keywords "climate change" appear along the mention of each event.

Figure 2.5: Number of articles in German newspapers that mention the natural events in a 10-day window following the event.

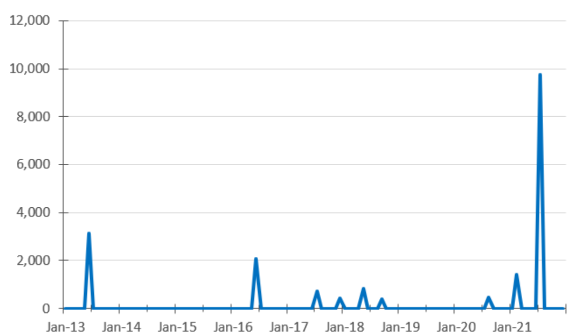
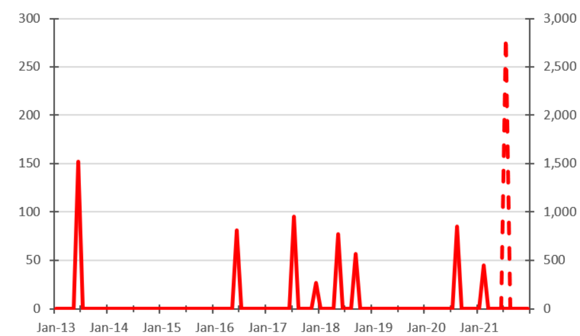


Figure 2.6: Number of articles in German newspapers that mention the natural events and refer to "climate change" in a 10-day window following the event.



Note: The dashed line corresponds to right hand side axis.

Figure 2.5 shows the number of articles in German newspapers that mention the natural events in a 10-day window following the event, whilst figure 2.6 shows a subset of these articles, which also mention the words "climate change". Larger-scale events that have affected a larger number of locations have attracted more media attention. For example, the floods of July 2021, which were considered the largest-scale climate change-related natural event of the last decade in Germany,

[8]The following newspapers (in print and online) are considered: *Die Welt, Die Zeit, Der Spiegel, Süddeutsche Zeitung, Handelsblatt, taz - die tageszeitung.*

have attracted a large amount of media attention.

In order to address this question, the "simple shock" from the previous section is interacted with the two indices of media attention to each natural event. Equation 2.3 is estimated using these new shock series that contain information not only on the occurrence of events, but also on the media attention they have attracted. To test *Hypothesis 4*, namely that the pricing of climate change shocks is more pronounced the more media attention they attract, an additional term is included in the regression, which is the square of the news attention index (i.e. the square of the number of articles mentioning the natural event).

I do not find evidence supporting *Hypothesis 4*. The coefficient of the square of the news attention index is statistically insignificant for both climate change media attention measures. This finding suggests that there are no size effects. Natural events that attract more media attention do not elicit a stronger response of real estate prices. This could be because the analysis already focuses on major natural events occurring in Germany over the last decade. Complementing the analysis with smaller-scale, more local events could change this result. I leave this analysis for future research.

2.6 Conclusion

Natural events, such as floods and windstorms, are likely to increase in both frequency and severity in the coming years due to climate change. This will have significant economic implications for the real economy and the financial system. An important implication is the destruction of property and the valuation effects on residential real estate prices. While a few empirical papers study how a range of natural events affect real estate prices in the affected areas, the conclusions are often contradicting and focus only on local effects.

This paper investigates the effects of physical risk from climate change on residential real estate prices in Germany. First, I identify climate change shocks using exogenous temporal and spatial variation in the occurrence of natural events in Germany between 2013 and 2021. Then, I empirically disentangle climate change shocks' direct and indirect effects on residential real estate prices.

This paper sheds light on two types of spillover effects of physical risk from climate change on residential real estate markets. First, real estate prices fall not only in the areas affected by a natural event, but also in unaffected areas that share a similar risk profile to the natural disaster with the affected areas. Second, there are neighbouring effects since real estate prices fall in municipalities that are close in distance to those directly affected by climate change shocks but have themselves not been directly affected by the natural event. Moreover, I find that properties more exposed to physical risk face a more significant price fall than non-exposed properties in areas where climate change shocks are realized. The effects are not permanent. Finally, this paper finds no evidence to support that the pricing of climate change shocks is more pronounced the more media attention these events attract.

Evaluating the impact of physical risk from climate change on residential real estate prices has important policy implications, especially regarding financial stability, given the role of residential real estate as collateral for banks. The results suggest that the effects of climate change on real estate markets are more widespread than previously thought and, therefore, should be treated as a systemic risk to the banking system, for example, by considering policies such as climate systemic risk buffers. An interesting extension of this work would be to study the dynamic effects of climate change shocks and to identify whether the presence of *spillover effects* is priced into mortgage contracts.

Chapter 3

MPC Heterogeneity and Homeowner Status

3.1 Motivation

Measurement of the consumption response to policy innovations is central to the analysis of monetary and fiscal policy. To the extent that these interventions impact income, there will be a consumption expenditure response. A common (and traditional) tool for capturing the consumption response to income variations is the marginal propensity to consume (MPC).

In modern macroeconomics, the MPC is state-dependent, reflecting the intertemporal choice of individuals as they evolve over time across states. Though application-dependent, the household state includes elements such as income, wealth, asset market participation, the liquidity of their portfolio, and homeownership status.

This paper focuses on the dependence of the MPC on homeownership status, particularly whether a household has a mortgage. These households are often viewed as high MPC entities reflecting occasionally binding liquidity constraints and thus central to the effects, through income, of monetary and fiscal interventions. That is, homeownership status can matter as it impacts the liquidity of wealth and thus hand-to-mouth (HtM) status.

As one of many examples, Cloyne et al. (2020) argue that the effects of monetary policy on consumption depend on housing status. For both the US and the UK, they find that the consumption of homeowners with mortgages responds more to a monetary innovation compared to a renter. Furthermore, they find that outright owners have the smallest consumption response to monetary innovations.

This evidence is supplemented by a recent study of Norwegian households by Holm et al. (2021). That paper distinguishes households by their liquidity and studies the consumption responses to monetary shocks. They do not find a monotone relationship of this response with respect to their liquidity measure. Instead, the consumption response of both low and high-liquidity households is larger than the middle group. They attribute these responses to both income and interest rate (return) variations induced by the monetary innovation.[1] Mortgages do play a role, linking the consumption response of relatively low-income households to the monetary shock through an interest rate effect.

Studies of fiscal policy also point to the high MPC of mortgagors. The starting point is Kaplan and Violante (2014) which builds a partial equilibrium model of household portfolio choice to study the consumption response to the 2001 tax rebates. They identify a set of households that are wealthy but constrained by their lack of immediate liquidity, including housing. With costs of adjustment, these households respond more than others to a tax rebate. This paper provides the link between the magnitude of the MPC and HtM status, where the latter is itself influenced by the amount of home equity. Misra and Surico (2014) go further and find that those households with debt (and high income) had a bigger consumption response to the tax change. This is based upon analysis of the CEX for both the 2001 and 2008 tax rebates.

Supporting evidence comes from studies that estimate MPCs and, in particular, those that identify relatively wealthy-HtM households. A leading example is Kaplan et al. (2014), who estimate MPCs for wealthy and poor-HtM households as well as non-HtM households.[2] A main

[1] The U-shaped pattern of response is similar to that reported in Ampudia et al. (2024), though for a different sample and a different methodology.

[2] Cloyne et al. (2020) also cite Kaplan and Violante (2014). We focus more on Kaplan et al. (2014) as our empirical approach and evidence builds directly on theirs.

finding in Kaplan et al. (2014) is that, indeed, wealthy-HtM households have higher MPCs. But Kaplan et al. (2014) do not directly condition on homeownership status. Moreover, in another highly cited study, Kaplan et al. (2018) stress the importance of HtM households for the monetary transmission process. But, in contrast to Cloyne et al. (2020), neither homeowner status nor mortgages play an explicit role in the model.

Putting these pieces together, a consistent story appears to emerge. The consumption response to monetary innovations detected in Cloyne et al. (2020) is a consequence of the higher MPC of households who own homes and have mortgages and thus fit into the wealthy hand-to-mouth group highlighted by Kaplan et al. (2014) and Kaplan et al. (2018). A parallel line of reasoning links fiscal responses to homeownership status.

But there are two (related) missing pieces in this stream of logic: (i) are homeowners with mortgages more likely to be HtM households, and (ii) do homeowners with mortgages have higher MPCs? The paper addresses two questions.

On the first question, HtM and homeownership status are quite different. For our sample, 68% of households with mortgages are not HtM, using the Kaplan et al. (2014) definition. As discussed below, the fraction of households with mortgages characterized as HtM does vary over our sample period and impacts the MPC estimates.[3]

As for the estimation of MPC differences, we do not find evidence that the marginal propensity to consume differs across households based upon mortgage status. In particular, we reject the hypothesis that the MPC out of transitory income shocks of homeowners with mortgages is higher than that of outright owners. The MPC out of transitory income is estimated at 0.123 for households with a mortgage and 0.0868 for households who are outright homeowners. This difference is not statistically significant.[4] The MPC of the renters is lower than the homeowners, and this difference is statistically significant. For this baseline estimation, the point estimate of the MPC for HtM mortgagors is larger than the MPC for HtM mortgagors not characterized as HtM. But

[3]From the SCF, Cloyne et al. (2020) find a larger share of HtM among mortgagors.

[4]The MPC for the mortgagors is significantly different from zero but that of the outright owners is not. Due to the large standard errors for the outright owners, the difference is not signficantly different from zero. See Table C.8 in the Appendix.

this difference is not statistically significant either. While our findings contradict the hypothesis that mortgagors have a higher MPC than other homeowner groups, they are still consistent with previous literature findings, that mortgagors respond more to monetary or fiscal policy shocks.

Our baseline estimates focus on transitory income shocks, with a sample from the PSID starting in 1999 and ending in 2019. There are a number of variations to consider, though, that highlight the importance of the choice of the sample period, the first-stage regressions, and the assumed persistence of the shock.

Regarding the sample period, there are large variations in the fraction of HtM households, particularly those with mortgages. This fraction falls by nearly 10 percentage points, comparing the first and second halves of the sample. The estimated MPC for mortgagors is higher for the first part of the sample, which is close to that of Kaplan et al. (2014). Conditioning on wealthy-HtM households with mortgages, the estimated MPC for this group is also the largest in the first part of the sample.[5]

As for the first stage regression, following Blundell et al. (2008), it has become common practice to include employment status in the first-stage regression that characterizes deterministic income. Our baseline specification treats employment status as random and thus excludes it from the first stage. This modeling choice matters. Generally, MPC estimates are larger when the first stage regression includes employment status.

The estimation is extended to consider the persistent income shocks. This seems more relevant for the analysis of monetary shocks since income responses last longer than the innovation, as documented, for example, in Figure 9 of Cloyne et al. (2020).[6] In response to the innovation of the persistent component of income, the MPC for mortgagors is below that of outright owners.

While the estimated MPC may not be statistically significantly different, the estimated MPCs might nonetheless generate large differences in the effects of monetary and fiscal policy on non-durable consumption expenditures. The model has predictions for both the magnitude of these

[5]The fact that the MPC changes with the sample conditioning on HtM status and mortgages implies heterogeneity within this group that varies over the sample.

[6]See the discussion below of the important point from Kaplan and Violante (2010) of potential biases with the Blundell et al. (2008) estimator with permanent shocks.

responses and the relative size by homeownership status.

For the leading case of persistent income shocks induced by monetary policy, in response to an innovation in the persistent component, the estimated model nearly matches the consumption response by renters from Cloyne et al. (2020), but the consumption responses of mortgagors and outright owners are very far, both in terms of magnitude and relative size.[7] So, putting the estimated MPCs together with the estimated income responses by homeownership, we are unable to match either the differences in consumption responses or the magnitude uncovered by Cloyne et al. (2020). There must be other factors at play, such as the effect of monetary shocks on the magnitude of mortgage payments and thus consumption emphasized in Eichenbaum et al. (2022).[8]

As for fiscal policy, our MPC estimates from transitory income variations are coupled with the 2001 US tax rebate. Compared to the effects described in Kaplan and Violante (2014), the consumption responses are smaller. This is likely due to two factors: (i) our MPCs are low, and (ii) we focus on the effects of the tax rebate in isolation and do not include general equilibrium effects.

While the analysis focuses on US data, there is relevant evidence from other countries. Jappelli and Pistaferri (2014) study survey responses, which are essentially reported values for MPCs by household. They are able to link these responses to measures of liquidity. They find that households with low cash-on-hand reported a higher MPC. Given other measures of financial status, homeownership did not have a significant effect on the MPC.[9]

Ampudia et al. (2024) estimate a household dynamic portfolio choice model for four European countries, including Italy. That paper creates distributions of MPCs from the decision rules of heterogeneous households as an input into monetary policy analysis. In an exploration of the role of housing, the MPC out of transitory income is estimated for Italian households using the techniques of Blundell et al. (2008). There are no significant differences across MPC estimates by homeownership status.

[7]For outright owners, Cloyne et al. (2020) reports a negative consumption response. Our estimated response is small, but positive.

[8]This is discussed further in the concluding section.

[9]This comes from Table 4 of Jappelli and Pistaferri (2014).

3.2 Household Dynamic Optimization

Here we present a simple partial equilibrium framework, building on Kaplan et al. (2014) by introducing uncertainty and housing explicitly.[10] This is not a structural estimation exercise. The model is intended to formalize the intuition that underlies the interpretation of the estimation results.

The household lives three periods, $t = 1, 2, 3$. It has income y_t and consumes c_t units of the non-durable consumption good in period $t = 1, 2, 3$. It also consumes housing services in periods $t = 1, 2, 3$. Income in period 2 is stochastic and conditional on (known) income in period 1. Income in period 3 is a fraction of income in period 2, representing retirement benefits.

There are two assets. The household can save through a liquid asset to obtain a gross rate, R. Further, the household can buy a house and sell it later. If the agent does not buy a house, then they rent to obtain housing services. There are frictions, in the form of loan-to-value restrictions and costs of borrowing against equity.

3.2.1 Agents

This subsection describes the optimization problems of agents by homeownership status. The next section outlines the channels of monetary policy.

Homeowners For homeowners, the house is purchased at the end of period 1 at a price q_1 and sold in period 3, at a price q_3. The household enjoys services from homeownership in periods $t = 2, 3$ and can spend the proceeds from the house sale in period 3.

The focus is on the effects of housing illiquidity on consumption in the middle period. The illiquidity comes from two sources. First, in period 2, the household can borrow amount b against the equity value of its house at a cost of Γ. Importantly, this is not a cost of adjusting the house but rather borrowing against its value. Else, consumption in the middle period is constrained by

[10] Kaplan et al. (2014) provide a simple 3 period model to illustrate the key factors determining HtM status. Supplementary Appendix J of Cloyne et al. (2020) contains a much richer general equilibrium model. Discussions on these problems with Aleksei Kiselev and Christopher Schang are appreciated.

income. Second, there is a limit on the amount that can be borrowed through a loan-to-value constraint.

A household in period 2 with income y_2, financial wealth A_2 and housing h, ie. in state (y_2, A_2, h), solves

$$V(y_2, A_2, h) = max\{V^b(y_2, A_2, h), V^n(y_2, A_2, h)\} \tag{3.1}$$

where $V^b(y_2, A_2, h)$ is the value if the household borrows and is given by

$$V^b(y_2, A_2, h) = max_{b \leq \bar{b}, A_3 \geq 0} u(y_2 + RA_2 - A_3 + b - \Gamma, h) + \beta u(y_3 - Rb + RA_3 + q_3 h, h) \tag{3.2}$$

where R is the rate of interest on the loan and savings and $\bar{b}$ is the limit on borrowing against home equity.[11] Given the fixed house size and the deterministic value of the house, $\bar{b} = \phi q_3 h$ would represent the limit where ϕ is the maximal loan-to-value ratio.

If the household does not pay the cost Γ in period 2, then the household can save $A_3 \geq 0$ but is unable to borrow and solves:

$$V^n(y_2, A_2, h) = max_{A_3 \geq 0} u(y_2 + RA_2 - A_3, h) + \beta u(y_3 + RA_3 + q_3 h, h) \tag{3.3}$$

In period 1, the household decides on how much housing to purchase, realizing that housing provides both a service flow and a source of liquidity.

$$max_{h, A_2} u(y_1 - A_2 - q_1 h) + \beta E_{y_2|y_1} V(y_2, A_2, h). \tag{3.4}$$

Renters If the minimum house size is not small enough, then agents with low incomes in period 1 and hence low expected discounted values of lifetime income will choose not to buy a house. They will obtain housing services through a rental market.

[11] Borrowing and lending rates are the same as there is no default risk.

Hand To Mouth Households In this model without any aggregate shocks, there are three frictions that can lead to "hand to mouth" behavior. First, a group of agents who experience low income realizations in period 2 but choose **not** to borrow against the value of their house. They solve the optimization problem in (3.3). If, in that solution, $A_3 \geq 0$ binds, then the household will be constrained. In fact, there will be a range of y_2 realizations such that the household falls into this so-called wealthy-HtM group, which are households that hold sizable amounts of illiquid assets (here housing) but very little or no liquid wealth.

Households with a sufficiently low y_2 realization will pay the cost to borrow against the house. For this group, they may still be constrained by $\bar{b}$. This is another group of wealthy-HtM households.

An important issue is whether outright owners can nonetheless be wealthy-HtM households. This depends on the nature of the frictions. If $\Gamma > 0$ so that borrowing against home equity is costly for everyone, then outright homeowners, choosing not to pay this cost, may be illiquid. If, instead, $\Gamma = 0$ while ϕ is low, then the cost comes from the maximal loan-to-value ratio. In this case, clearly, outright owners will be able to borrow more to smooth consumption against income shocks.

As for the renters, in the middle period, they will have no home equity to borrow against. In response to low income realizations in the middle period, the constraint of $A_3 \geq 0$ may bind. These would be termed poor-HtM households, which are households holding little or no liquid wealth and no illiquid wealth (housing).

3.2.2 Monetary and Fiscal Shocks

With this simple model in mind, it is relatively easy to think through the effects of policy innovations. The direct effects are through income and, thus, consumption expenditures. As noted, this response will be state dependent as households in different states will react differently.

Specifically, consider the effects of a monetary shock. Cloyne et al. (2020) emphasize the income channel, i.e. variations in income in period 2 from the effects of a monetary contraction on household income.[12] The above model then provides insights into the response of individuals, by homeowner status, to income variations that would apply.[13] Some would be bound by a borrowing constraint, including a subset of individuals with housing assets. Those agents would exhibit a higher MPC out of variations in current income. Others with a house may choose not to pay the fixed cost of borrowing, Γ, and then consume out of current income and financial assets.

A parallel logic links fiscal policy to income variations and thus consumption through tax rebates as well as the future tax obligations to pay for these debt-financed transfers. The key remains the determinants of the MPC.[14]

In the aggregate, the distribution of households across states becomes important. If, for example, at two distinct points in time, the fraction of wealth HtM households differs, then the aggregate response of consumption to a policy shock will differ as well. This point will be important later as a way to understand changes in the estimates of MPC across sample periods.

3.3 Data Summary

The data set is comparable to that used in Kaplan et al. (2014).[15] The income, consumption, and wealth measures are from the PSID bi-annual sample from 1999-2019. This is a longitudinal dataset that provides information on household income, consumption, and wealth, which are required to use the methodology of Kaplan et al. (2014) and Blundell et al. (2008). Income is labor income plus government transfers, net of federal income taxes.[16] The consumption measure includes spending on food at home, food away from home, utilities, gasoline, car maintenance, public transport, child care, health expenditure, and education.[17] The data includes both home-

[12]Further, the mortgage rate could be explicitly dependent on the monetary shock, reflecting either refinancing or the presence of variable rate mortgages. The sensitivity of the mortgage rate to monetary policy becomes of paramount importance. On this see the discussion in Eichenbaum et al. (2022).

[13]Of course, the consequent income variations could well reflect various channels linking income to monetary innovations.

[14]Although less frequently mentioned is the recognition of future tax obligations.

[15]Differences in the data and results are discussed below.

[16]Federal income taxes are not reported in the PSID survey and therefore have to be imputed. They are estimated from the latest version of the NBER's TAXSIM from income (TAXSIM32).

[17]From 1999 onwards, this measures covers about 70% of all consumption items available in CEX.

ownership status (renter, owner) as well as mortgage status. The data is trimmed according to Kaplan et al. (2014), leaving a sample of 60,284 observations over the years 1999-2019.[18]

3.3.1 Homeowner and HtM Status

Based on the data, we can study the cross-section of households by homeownership status and whether they are HtM or not. For homeownership status, either a household owns a house or is a renter. Conditional on owning a house, either they have (outstanding) mortgages or are outright owners. In our data, 61.4% of households are homeowners. Of them, 30.5% are outright owners.

For the HtM status, we follow Kaplan et al. (2014), looking at household income and asset positions. As discussed above in section 3.2, HtM households are those who appear to have binding borrowing constraints. They wish to consume more in the current period but are unable to either because: (i) they have no wealth and are unable to borrow or (ii) they have positive wealth, but it is illiquid. The former are called poor-HtM, and the latter wealthy-HtM.

In terms of measurement, Kaplan et al. (2014) categorize a household as poor-HtM if: (i) illiquid wealth is not positive and (ii) liquid wealth is half of income in a period. In a similar manner, a household is wealthy-HtM if: (i) illiquid wealth is positive and condition (ii) holds as well.

In their application to data, a key question is how to define liquid and illiquid assets. For the PSID, liquid assets include checking and savings accounts, money market funds, and even directly held shares of corporations and trusts. Liquid debt includes the value of debts other than mortgages (such as credit cards, student loans, medical bills, legal bills, and other personal loans). After 2011, liquid debt only includes credit card debt. Net liquid wealth is simply liquid assets minus liquid debt. On the other hand, net illiquid wealth mostly consists of home equity and other real estate net of mortgages, as well as the value of private annuities or IRAs and the value of investments in trusts or estates, bond funds, and life insurance policies.

Using these definitions, Figure 3.1 summarizes the joint distribution of HtM and homeown-

[18] Specifically, we drop households with top-coded income or consumption, those that appear in less than 3 waves. We focus on household heads between 22 and 55 years old.

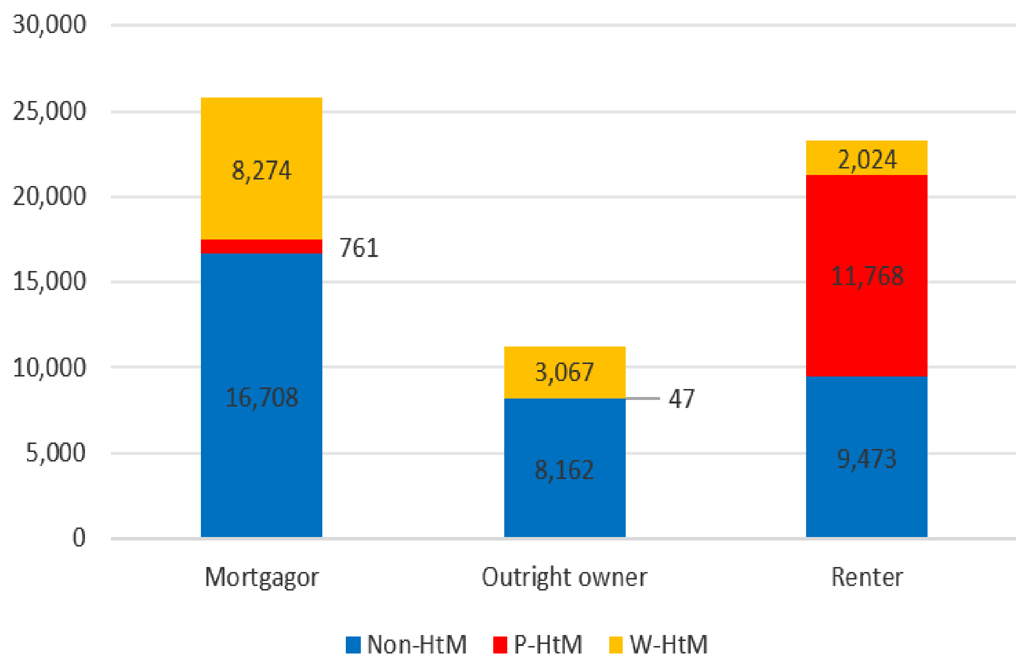


Figure 3.1: Joint distribution of HtM and homeownership status.

Notes: number of households that belong in each category. Pooled over the years 1999-2019. Definitions of HtM following Kaplan et al. (2014).
Sources: PSID and authors' calculations.

ership status.[19] The results are somewhat surprising in that **households with a mortgage are predominantly non-HtM**. In particular, 65% of mortgagors are non-HtM, while 32% are wealthy-HtM, and 3% are renters.[20] Further, about 23% of the wealthy-HtM are outright homeowners, and another nearly 15% are renters.

3.3.2 Income Process

Income for agent i in period t is given by:

[19]Cloyne et al. (2020) present related evidence from the SCF. They find a larger fraction of wealthy-HtM within the group of mortgagors.

[20]The same observation applies for the Kaplan et al. (2014) sample. In particular, 61% of mortgagors are non-HtM. Furthermore, 28% of the wealthy-HtM are outright homeowners and another nearly 11% are renters. Figure 3.2 shows that the fraction of wealthy-HtM mortgagors has decreased by about 5 percentage points over the sample.

$$y_{it} = \bar{y}_{it} + \tilde{y}_{it}. \tag{3.5}$$

For this income specification, there is a deterministic component, $\bar{y}_{it} = X_{it}\gamma$, and a stochastic component, $\tilde{y}_{it}$. The first stage regresses realized income on this deterministic component to estimate γ and generates the stochastic component as a residual.

Normally, X_{it} contains covariates that are predictable for the individuals, such as year, cohort, gender, race, and geographic variables, as well as their interactions. Other variables often included are education and family structure, though these could be viewed as choices. Kaplan et al. (2014), following Blundell et al. (2008), go further and include employment status, i.e. whether an individual is employed or not, in period t in the deterministic component. We study both the specification in which employment is treated as random and the alternative where employment status is part of the predictable component of income.

The second stage of the estimation focuses on $\tilde{y}_{it}$, splitting it into a permanent and a transitory component. The growth of stochastic income is given by:

$$\Delta\tilde{y}_{it} = \eta_{it} + \epsilon_{it} \tag{3.6}$$

where η_{it} is the innovation to the permanent component of stochastic income and follows a unit root process, and ϵ_{it} is the i.i.d. transitory shock.

The consumption series is treated in the same way, separating the consumption of household i in period t into a deterministic and a stochastic component. Let $\Delta\tilde{c}_{it}$ be the growth of the stochastic component of nondurable consumption.

3.4 MPC Estimates: Transitory Shocks

Using this specification of income, we estimate the MPC out of transitory income shocks by household type. Here the presentation follows Kaplan and Violante (2010), which itself builds from Blundell et al. (2008). Results for persistent income shocks are presented in Section 3.5.

The data set used differs from that underlying the results of Kaplan et al. (2014) in two respects. First, our sample extends from 1999-2019, while theirs ended in 2011. Second, we use an updated methodology of the NBER's TAXSIM model to calculate federal taxes. The new methodology results in different disposable income measures, and therefore the categorization of households by HtM status is slightly different.

Households that do not own a residential property are classified as renters whereas those who own one are considered homeowners. Of those, households that have outstanding mortgage balances are classified as mortgagors, whereas those with a mortgage value of zero are considered outright owners (either have repaid their mortgage or never took one). We then distinguish between mortgagors that are classified as wealthy-HtM and non-HtM according to Kaplan et al. (2014) definitions.[21]

3.4.1 MPC Estimates

Blundell et al. (2008), in their equation (6), stipulate that expected consumption growth for agent i in period t can be written as a linear function of the innovation to permanent and transitory income.[22] This implies that the MPC out of a transitory shock is:

$$MPC = \frac{cov(\Delta\tilde{c}_{it}, \epsilon_{it})}{var(\epsilon_{it})}. \tag{3.7}$$

As researchers are unable to observe the income innovation directly, Blundell et al. (2008) study an estimator, hereafter the "BPP estimator", of the transmission coefficient of transitory income shocks to consumption.[23] The estimation is implemented by an IV regression of $\Delta\tilde{c}_{it}$ on $\Delta\tilde{y}_{it}$, instrumented by $\Delta\tilde{y}_{i,t+1}$:

[21]For the baseline estimation, housing status by household can change in any period. Later we study the role of switching status.

[22]This is clearly an important step for the analysis and one that deserves further scrutiny. Kaplan and Violante (2010) provide simulation based evidence that the nonlinearities of models with borrowing constraints do not create a bias in the Blundell et al. (2008) estimator of responses to transitory income shocks. This is apparently not the case for more persistent shocks where the MPC estimator is biased downwards.

[23]To be careful, Blundell et al. (2008) estimate the consumption insurance rather than the MPC. Kaplan et al. (2014), among others, use this to estimate MPCs.

$$\widehat{MPC} = \frac{cov(\Delta\tilde{c}_{it}, \Delta\tilde{y}_{i,t+1})}{cov(\Delta\tilde{y}_{it}, \Delta\tilde{y}_{i,t+1})}. \tag{3.8}$$

The estimator is consistent under the assumptions of the household has no foresight or no advanced information about future shocks, ie. $cov(\Delta\tilde{c}_{it}, \eta_{i,t+1}) = cov(\Delta\tilde{c}_{it}, \epsilon_{i,t+1}) = 0$.

Table 3.1 shows the MPC by group using this estimator. The rows indicate the exclusions from the first stage regression. The columns are subsets of households. The first row reports our baseline estimation results by homeowner status, HtM status, and their interaction. For these results, the first stage includes year and cohort dummies, education, race, family structure, geographic variables, and interactions of year dummies with education, race, and region. The baseline **does not** include employment status in the first stage regression. For comparison to the literature, the table (far right three columns) also includes estimates for both poor and wealthy-HtM households and those who are not HtM. Again, these definitions follow Kaplan et al. (2014).

There are a couple of key findings. First, from the point estimates, the MPC for mortgagors exceeds that of outright owners, which, in turn, exceeds that of renters. Of these, only the MPC of the mortgagors is significantly different from zero. Second, our baseline estimates find that the MPC of the wealthy-HtM is slightly larger than that of non-HtM households, while the estimated MPC for the poor-HtM is effectively zero. Tests for differences across these MPC estimates are discussed in Appendix Section C.3. None of these differences in the estimated MPCs by HtM classification are significant except for the difference in the MPCs of mortgagors and renters, where the p-value is 0.045.

The interaction of homeownership and HtM status is of interest. As noted earlier, about 62% of wealthy-HtM households have mortgages though only about 32% of mortgagors are wealthy-HtM households. From Table 3.1, the MPC estimates, conditional on having a mortgage, are slightly higher for wealthy-HtM, though again, the differences are not statistically significant.

A concern with these results is that whether the BPP estimator is reliable in settings with occasionally binding borrowing constraints. Kaplan and Violante (2010) study the properties of the BPP estimator in such a setting and find that for transitory shocks, there is no evidence of

Table 3.1: MPC Estimates: Transitory Income Shocks

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-HtM	Mortgagor - W-HtM	P-HtM	W-HtM	N-HtM
Baseline	0.0312	0.116***	0.0868	0.123***	0.118***	0.148***	-0.00975	0.118***	0.0954***
	(0.0381)	(0.0238)	(0.0596)	(0.0261)	(0.0307)	(0.0484)	(0.0626)	(0.0417)	(0.0244)
Inc. empl. status	0.133***	0.195***	0.164***	0.203***	0.210***	0.200***	0.0985**	0.188***	0.186***
	(0.0313)	(0.0221)	(0.0571)	(0.0240)	(0.0286)	(0.0437)	(0.0484)	(0.0383)	(0.0221)
Exogenous vars only	-0.175***	-0.0175	-0.0218	-0.0151	-0.00955	-0.0118	-0.175**	-0.0377	-0.0601*
	(0.0526)	(0.0282)	(0.0698)	(0.0310)	(0.0362)	(0.0581)	(0.0819)	(0.0511)	(0.0309)
Observations	5876	12450	1765	10685	6986	3377	2955	4591	10780

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

bias.[24]

In sum, the evidence does not suggest large and significant differences in the marginal propensity to consume based on homeowner status.

3.4.2 Robustness

The estimated MPC by homeowner and HtM status are somewhat surprising given the findings of Kaplan et al. (2014). They estimated an MPC for the wealthy-HtM of around 0.30, 0.24 for the poor-HtM and an MPC of 0.127 for the non-HtM.[25] These differences with our results are not a consequence of methodology as we follow their approach.

Rather, there are differences in the first-stage regression, in the sample period, and the tax treatment. We discuss these in turn, as they are informative about the economic forces at play.

Alternative Stage 1 estimates

As is well understood, these MPC estimates are based on a process that eliminates deterministic elements of income and consumption to focus on the stochastic components. It seems that our findings are quite sensitive to this first stage.

The second row of Table 3.1, termed "Inc. empl. status" incorporates the same regressors in

[24]The more nuanced results with persistent shocks are discussed below.
[25]It appears that the difference between poor and wealthy-HtM was not statistically significant.

the first stage as Blundell et al. (2008) and Kaplan et al. (2014), including **realized employment status**. The estimated MPCs are much larger than in the baseline. Further, they are now all significantly different from zero at the 1% level, except for the poor-HtM group. The ordering by homeowner status remains the same with mortgagors having the highest and renters the lowest point estimate. But again none of these differences are statistically significant.[26] For this specification, the difference between wealthy-HtM and non-HtM, either alone or conditional on having a mortgage, is almost zero and not statistically significant.

The last row of Table 3.1 has a stage 1 regression with only exogenous variables, excluding education and family status from the first stage.[27] Here the point estimates of the MPC are all negative and are all statistically insignificant except for the renter.

The importance of the first stage regression comes from the estimator given in (3.8). Clearly alternative first-stage regressions will alter these correlations and thus the point estimates. We study this in detail in Table 3.2 which shows the MPC for mortgagors using two different stage 1 regressions.[28] From this table, the inclusion of employment status increases in absolute value the covariance of consumption and (the lead of) income growth and also increases the absolute value of the covariance of income growth over time. The effect on the numerator dominates so that the estimated MPC is higher for the full sample as well as various splits.

Alternative Sample

Our sample period is 1999-2019, longer than that used in Kaplan et al. (2014). Since those results represent a reference point, it is important to understand why our estimates differ. There are two distinct issues with the choice of the sample period. One has to do with variations in homeownership and HtM status over the years, and the other with the computation of taxes to determine disposable income. We study these in turn.

[26] These tests and results are shown in Appendix C.3.

[27] Log income and log consumption are regressed on year and cohort dummies, race, geographic variables and interactions of year dummies with race, region. Variables like employment status, education and family structure are excluded from this specification.

[28] The sample split components of the table are discussed below.

Table 3.2: Breakdown of (3.8) for Mortgagors (Transitory income shocks)

Sample period	$cov(\Delta\tilde{c}_{it}, \Delta\tilde{y}_{i,t+1})$	$cov(\Delta\tilde{y}_{it}, \Delta\tilde{y}_{i,t+1})$	MPC
	Baseline		
1999 – 2019	-0.005	-0.041	0.123
< 2008	-0.0076	-0.04686	0.163
≥ 2008	-0.0030	-0.03397	0.087
2008 – 2012	-0.0076	-0.04597	0.166
> 2012	-0.0010	-0.02741	0.036
	Inc. empl. status		
1999 – 2019	-0.009	-0.0443	0.203
< 2008	-0.0113	-0.0511	0.221
≥ 2008	-0.0079	-0.0404	0.196
2008 – 2012	-0.0117	-0.0559	0.210
> 2012	-0.0048	-0.0293	0.163

Sample Period There are relevant variations in the distribution of household status across the sample years. Figure 3.2 shows the fraction of households by homeowner and HtM status from 1998 to 2018. The fraction of W-HtM households with a mortgage has fallen throughout this period. Comparing 2006 to 2018, this fraction has fallen by almost 50%. All else the same, one would conjecture that the MPC of mortgagors would fall as well since fewer are likely to be liquidity constrained. In contrast, the fraction of non-HtM renters has grown over the period. And after 2014, the fraction of outright non-HtM households rose as well. From this, one might conjecture that MPC differentials may have fallen, particularly when contrasting mortgagor and outright owners.

Table 3.3 presents the MPC estimates from transitory income shocks from three sub-samples. The first block pertains to the years before 2008 when a larger fraction of households were wealthy-HtM with mortgages. The second block reports post-2008 estimates. The final block shows the MPC estimates after 2012 when the fraction of wealthy-HtM households with mortgages was lower than previously and stable over time. Again there are two treatments of the stage 1 regression, with the baseline results excluding employment status.

The results across the sample periods are quite different. For the pre-2008 sample, the MPC

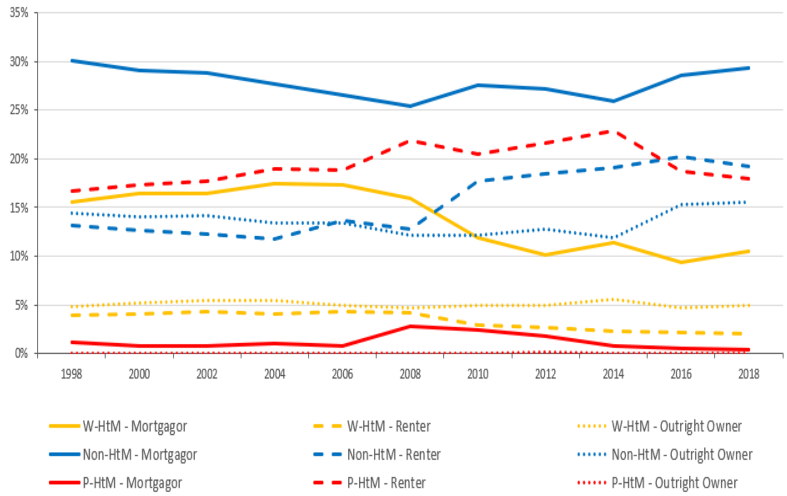


Figure 3.2: Joint distribution of HtM and homeownership status.

Notes: Time series of fraction of households that belong in each category. Definitions of HtM following Kaplan et al. (2014).
Sources: PSID and authors' calculations.

estimates are significantly different from zero for mortgagors, for both of the stage 1 regressions. The wealthy-HtM with mortgages have a lower MPC than the non-HtM but this difference is not statistically significant. After 2012, none of the estimated MPCs are significant. While it is the case that the sample is much smaller, similar patterns appear when the sample is from 2008 onwards. As seen in the middle panel, after 2008, the baseline MPC estimates are smaller than in the pre-2008 sample and only the mortgagors' MPC is significantly different from zero.

It is interesting to understand further why these MPC estimates change. First, note that household-specific decision rules are state-dependent. Differences in the household state are not completely captured by any group, such as "mortgagor". The distribution of the state variables within these groups as well as the relative size of the groups, is changing over time.

Second, the covariances in the estimator shown in (3.8) may change over the sample. Indeed, from the top panel of Table 3.2, comparing the pre-2008 and post-2012 samples, both the covariance of consumption growth and future income growth and the covariance of income growth across years is lower in the post-2012 period. The reduction in the covariance of consumption and income growth falls more so that the estimated MPC is lower. This is consistent with there being a lower

Table 3.3: MPC Estimates for sub-samples

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-Htm	Mortgagor - W-Htm	P-Htm	W-Htm	N-Htm
				Year $<$ 2008					
Baseline	-0.0482	0.127***	-0.00819	0.163***	0.174***	0.121*	-0.0144	0.0649	0.0890**
	(0.0718)	(0.0370)	(0.0920)	(0.0407)	(0.0479)	(0.0723)	(0.108)	(0.0650)	(0.0411)
Inc. empl. status	0.0477	0.193***	0.0758	0.221***	0.227***	0.184***	0.0861	0.127**	0.158***
	(0.0620)	(0.0356)	(0.0927)	(0.0385)	(0.0463)	(0.0659)	(0.0923)	(0.0605)	(0.0387)
Observations	1646	4579	723	3856	2446	1350	834	1816	3575
				Year $>$ 2008					
Baseline	-0.0218	0.0854**	0.0773	0.0870**	0.0775	0.0933	-0.0572	-0.0003	0.0763**
	(0.0549)	(0.0399)	(0.0991)	(0.0440)	(0.0540)	(0.0776)	(0.0973)	(0.0669)	(0.0388)
Inc. empl. status	0.111**	0.179***	0.106	0.196***	0.224***	0.129**	0.0539	0.0795	0.202***
	(0.0431)	(0.0360)	(0.102)	(0.0384)	(0.0483)	(0.0642)	(0.0720)	(0.0595)	(0.0335)
Observations	3014	4941	636	4305	3046	1099	1427	1549	4979
				Year $>$ 2012					
Baseline	0.0345	0.00724	-0.0608	0.0363	-0.0533	0.233	0.0601	0.175	-0.0386
	(0.135)	(0.0993)	(0.221)	(0.114)	(0.148)	(0.172)	(0.236)	(0.145)	(0.102)
Inc. empl status	0.130	0.124	0.0130	0.163	0.152	0.204	0.110	0.218*	0.110
	(0.100)	(0.0965)	(0.233)	(0.108)	(0.140)	(0.152)	(0.196)	(0.127)	(0.0836)
Observations	779	1180	151	1029	774	242	306	338	1315

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

fraction of wealth HtM agents with mortgages in the later period.

Table 3.4 shows the MPC estimates using the sample from 1999 to 2011 as in Kaplan et al. (2014). Most of their sample is in the period when the fraction of wealthy-HtM mortgagors was relatively large. For this sample period, regardless of the stage 1 specification, the highest MPC is that of the wealthy HTM with mortgages. As before, these MPCs are larger when employment status is included in the first stage regression.

Tax Treatment To understand these differences, the block labeled "TAXSIM9" in Table 3.4 reports estimates using their sample, as well as an earlier version of the tax simulator. Note first that in this sample, the MPCs of mortgagors and homeowners are almost the same and exceed that of renters. And, as before, there is no statistical difference between the wealthy-HtM mortgagors and the other mortgagors, though the point estimates differ more here than in the larger sample. And the differences in point estimates are also larger comparing mortgagors and outright owners.

And, in contrast to the larger sample, the MPC estimates for the wealthy-HtM is much larger than the non-HtM with the poor-HtM in between. These rankings accord with those reported in

Table 3.4: MPC Estimates: KVW sample (1999-2011)

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-HtM	Mortgagor - W-HtM	P-HtM	W-HtM	N-HtM
					TAXSIM32				
Baseline	0.157***	0.137***	0.0806	0.151***	0.152***	0.175***	0.122	0.204***	0.122***
	(0.0584)	(0.0281)	(0.0699)	(0.0308)	(0.0360)	(0.0557)	(0.0787)	(0.0521)	(0.0333)
Inc. empl. status	0.176***	0.208***	0.173***	0.216***	0.207***	0.243***	0.201***	0.268***	0.149***
	(0.0504)	(0.0264)	(0.0657)	(0.0289)	(0.0343)	(0.0508)	(0.0650)	(0.0477)	(0.0312)
Exogenous	-0.0274	0.0222	-0.0145	0.0314	0.0496	0.0277	-0.0226	0.0577	-0.00761
vars only	(0.0768)	(0.0336)	(0.0856)	(0.0365)	(0.0420)	(0.0682)	(0.100)	(0.0646)	(0.0409)
					TAXSIM9				
Baseline	0.158**	0.145***	0.0755	0.163***	0.167***	0.188***	0.150*	0.210***	0.120***
	(0.0618)	(0.0312)	(0.0759)	(0.0343)	(0.0397)	(0.0633)	(0.0822)	(0.0586)	(0.0362)
Inc. empl. status	0.178***	0.224***	0.179**	0.236***	0.226***	0.269***	0.224***	0.283***	0.151***
	(0.0535)	(0.0291)	(0.0711)	(0.0320)	(0.0378)	(0.0570)	(0.0685)	(0.0534)	(0.0337)
Exogenous	-0.0297	0.0195	-0.0313	0.0329	0.0590	0.0193	0.00649	0.0422	-0.0147
vars only	(0.0802)	(0.0368)	(0.0926)	(0.0401)	(0.0453)	(0.0781)	(0.103)	(0.0729)	(0.0437)
Observations	3297	8220	1209	7011	4313	2515	1799	3350	6368

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Kaplan et al. (2014).[29]

The bottom panel of Table 3.4 uses their sample, with disposable income calculated using the more recent NBER tax simulator, denoted "TAXSIM32". Comparing the blocks, there are some modest differences in MPCs. Overall, it seems that the differences in MPC estimates between Tables 3.1 and 3.4 lies in the longer sample for the first set of results.[30]

Alternative Instruments

The original BPP estimator assumes that log-consumption follows a random walk. Commault (2022) argues that this estimator is not robust to a departure from this assumption and proposes a robust estimator which uses future log-income growth at $t + k + 1$ as an instrument, where $k \geq 1$.[31] Table 3.5 reports the MPC estimates out of transitory income shocks based on our baseline specification and using the proposed robust estimator. The first row corresponds to the

[29]Though the actual entries are a little different.

[30]It might be, for example, that the estimates depend on whether house prices were increasing or decreasing over the sample as this might interact with the frequency in which borrowing constraints could bind.

[31]The income specification include a MA(k) component.

original BPP estimator, which uses $\Delta ln(y_{i,t+1})$ as an instrument for $\Delta ln(y_{i,t})$. The remaining three rows correspond to different specifications of the robust estimator proposed by Commault (2022) for different values of $k = 1, 2, 3$.

Table 3.5: MPC estimates: Alternative Instruments

Instrument	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-Htm	Mortgagor - W-Htm	P-Htm	W-Htm	N-Htm
$\Delta ln(y_{i,t+1})$	0.0312	0.116***	0.0868	0.123***	0.118***	0.148***	-0.00975	0.118***	0.0954***
	(0.0381)	(0.0238)	(0.0596)	(0.0261)	(0.0307)	(0.0484)	(0.0626)	(0.0417)	(0.0244)
$\Delta ln(y_{i,t+2})$	0.0704*	0.114***	0.0836	0.121***	0.110***	0.135**	0.0742	0.132***	0.0879***
	(0.0411)	(0.0276)	(0.0706)	(0.0301)	(0.0358)	(0.0568)	(0.0590)	(0.0440)	(0.0302)
$\Delta ln(y_{i,t+3})$	0.0186	0.115***	0.169**	0.103***	0.0938**	0.122*	0.0671	0.161***	0.0329
	(0.0493)	(0.0324)	(0.0782)	(0.0358)	(0.0429)	(0.0654)	(0.0697)	(0.0517)	(0.0361)
$\Delta ln(y_{i,t+4})$	0.0888	0.0966**	0.107	0.101**	0.153***	0.0483	0.0466	0.0883	0.119***
	(0.0603)	(0.0381)	(0.0965)	(0.0417)	(0.0465)	(0.0807)	(0.0879)	(0.0635)	(0.0413)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

These experiments strengthen our findings. Instrumenting with income growth at leads bigger than 0, the estimated MPC of the mortgagor is always less than that of the outright owner. For $k = 2, 3$, among mortgagors, the wealthy-HtM group does have a higher estimated MPC. From Table C.11, none of these differences are statistically significant.

Homeownership status: Role of switchers

For the estimates reported in Table 3.1, households are allowed to change homeownership or HtM status over time. This means that the same household could appear in different columns of Table 3.1 for different years. In this section, instead, we restrict our sample to households that don't change their homeownership or HtM status over time.

Tables C.5, C.6 and C.7 in Appendix C.2 show the transition probabilities between period $t-1$ (rows) and t (columns) for homeownership, HtM, and employment status respectively. Overall, homeownership status is more persistent than HtM status, given that households are more likely to change their HtM status over time compared to their homeownership status. Among homeownership status, renters and mortgagors are equally likely to retain their status in the next period, whereas outright owners are more likely to change status. With regards to HtM households, wealthy-HtM are the least persistent group and are the ones most likely to change status in the next period. On the other hand, non-HtM households are less likely to change their HtM status, compared to poor-HtM and wealthy-HtM.

With regards to employment, those households that are in employment in period t have a 91% probability to remain in employment next period. Retirement is also a highly persistent employment state. On the other hand, unemployment is the least persistent state since a household that is unemployed in period t has a 23% probability of remaining unemployed in the next period.

Importantly, from those households that change employment status between t and $t+1$, only a small proportion change their homeownership and HtM status during the same time. This ensures that for the majority of our sample, changes in homeownership or HtM status are not driven by changes in employment.

Focusing on non-switchers, the MPC estimates in Table 3.6 are higher than those in Table 3.1, for all homeownership groups and for both first stage regressions.[32] The opposite holds for the HtM status, which, as described above, is more volatile, and therefore the estimation sample decreases significantly once we exclude households that change their HtM status over time. The difference between renters and mortgagors has decreased, compared to the results reported in Table 3.1.

Table 3.6: MPC Estimates: No Switchers

	Renter	Homeowner	Outright owner	Mortgagor	P-HtM	W-HtM	N-HtM
Baseline	0.140**	0.192***	0.181	0.193***	-0.0317	-0.370	0.00369
	(0.0568)	(0.0396)	(0.125)	(0.0421)	(0.221)	(0.397)	(0.0460)
Inc. empl. status	0.220***	0.290***	0.270**	0.293***	0.0960	-0.0488	0.120***
	(0.0442)	(0.0367)	(0.123)	(0.0387)	(0.133)	(0.324)	(0.0413)
Observations	2758	4230	314	3916	358	145	3428

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[32] Compared to the baseline, the covariance of consumption and income growth is larger in absolute value and the covariance of income growth over time is smaller in absolute value. Both of these contribute to the higher MPC estimate for mortgagors.

3.5 Persistent Shock

As documented in Cloyne et al. (2020), monetary shocks have a persistent though not permanent impact on income. Thus the response to a transitory shock might understate the actual MPC associated with an income change induced by a monetary shock.

In the Blundell et al. (2008) framework, shocks to the income process can either be fully permanent or fully transitory, such that income follows an $I(1)$ process. If monetary shocks have a persistent effect on income, then the method of Blundell et al. (2008) suffers from a misspecification error.

To study this, following Kaplan and Violante (2010), we relax the assumption that the permanent shock follows a random walk and instead consider the following AR(1) process with parameter $\rho < 1$:

$$z_{it} = \rho z_{it-1} + \eta_{it}. \tag{3.9}$$

With this income process, the previous estimators are no longer valid.[33] Instead, taking the parameter ρ as given, the new MPC estimates for transitory and persistent shocks are given by:

$$\widehat{MPC} = \frac{cov(\Delta\tilde{c}_{it}, \tilde{\Delta}y_{i,t+1})}{cov(\tilde{\Delta}y_{it}, \tilde{\Delta}y_{i,t+1})} \tag{3.10}$$

$$\widehat{MPC^{\rho}} = \frac{cov(\Delta\tilde{c}_{it}, \rho^2\tilde{\Delta}y_{i,t-1} + \rho\tilde{\Delta}y_{i,t} + \tilde{\Delta}y_{i,t+1})}{cov(\tilde{\Delta}y_{it}, \rho^2\tilde{\Delta}y_{i,t-1} + \rho\tilde{\Delta}y_{i,t} + \tilde{\Delta}y_{i,t+1})} \tag{3.11}$$

where $\tilde{\Delta}y_t = \tilde{y}_t - \rho\tilde{y}_{t-1}$.

Table 3.7 shows the baseline MPC estimates out of transitory and persistent shocks by homeownership status. Note that (3.10) and (3.11) use the serial correlation of income shocks to construct the estimators. As reported in Appendix C.4, we estimated the serial correlation by

[33]Kaplan and Violante (2010) show that the BPP estimator still performs well for high degrees of persistence.

homeownership and HtM status as an input into this estimator.[34] Two versions of the stage 1 estimation are reported, the baseline excludes employment status from the deterministic component of income, while the second case includes it.

The estimated MPCs from the transitory component are shown in the top block of Table 3.7. The MPC for the outright owners and mortgagors is close to the estimated reported in Table 3.1 for both versions of the stage 1 regressions. Again the MPC for mortgagors is slightly higher, but the difference is not statistically significant. The wealthy-HtM have a higher estimated MPC than the non-HtM among mortgagors, but the difference is not statistically significant. As before, the MPC for the outright owners and the renters is not significantly different from zero in the baseline.

But the point of studying the case with persistent shocks is to see the consumption responses to shocks to the persistent component. These are shown in the bottom block of the table. Looking at the baseline, the MPC for renters is very large and quite close to that of the poor-HtM. The point estimate of the MPC for mortgagors is larger than that of outright owners and is significantly different from zero. As in the other cases, the MPC of outright owners is not significantly different from zero. Among the mortgagors, the wealthy-HtM have a slightly lower MPC. These differences among homeowners are not significant.[35] If employment status is included in the first stage regression, then again, the estimates are much more precise, and the estimated MPC out of the transitory component is generally larger. For the persistent component, two differences from the baseline emerge. First, outright owners have a higher estimated MPC than mortgagors, though again, the estimate for the outright owners is only significant at the 10% level. Second, the MPC for the wealthy-HtM is now slightly larger, though the difference is not statistically significant.

A key part of the contribution in Kaplan and Violante (2010) is to study the properties of the Blundell et al. (2008) estimator in the presence of borrowing frictions. Their Table 5 compares the actual insurance coefficients and those estimated using the Blundell et al. (2008) methodology

[34] Kaplan and Violante (2010) display the MPC estimates for different values of the serial correlation of income. Table C.13 shows the baseline MPC estimates out of transitory and persistent shocks for different values of ρ. The MPC estimates change very little for the different levels of serial correlation.

[35] To evaluate the statistical significance of these differences, we assume that the serial correlation in the income process is the same in order to use the methodology outlined in Appendix C.3. From the estimates reported in Table C.12, there are very small differences in the estimated serial correlations by homeownership status.

Table 3.7: MPC Estimates: Persistent Income Shocks

	Renter	Home-owner	Outright - owner	Mortgagor	Mortgagor - N-Htm	Mortgagor - W-Htm	P-Htm	W-Htm	N-Htm
Transitory component									
Baseline	0.0157	0.106***	0.0836	0.112***	0.111***	0.130**	-0.0315	0.0843*	0.0878***
	(0.0410)	(0.0262)	(0.0644)	(0.0291)	(0.0338)	(0.0581)	(0.0693)	(0.0494)	(0.0264)
Inc. empl. status	0.127***	0.192***	0.164***	0.199***	0.210***	0.192***	0.0905*	0.170***	0.184***
	(0.0332)	(0.0240)	(0.0609)	(0.0264)	(0.0313)	(0.0511)	(0.0523)	(0.0439)	(0.0237)
Observations	5876	12450	1765	10685	6986	3377	2955	4591	10780
Persistent component									
Baseline	0.439***	0.168***	0.110	0.179***	0.180***	0.144**	0.496***	0.201***	0.225***
	(0.0569)	(0.0349)	(0.104)	(0.0367)	(0.0451)	(0.0660)	(0.0762)	(0.0580)	(0.0396)
Inc. empl. status	0.455***	0.163***	0.195*	0.154***	0.146***	0.151**	0.481***	0.206***	0.223***
	(0.0735)	(0.0384)	(0.118)	(0.0401)	(0.0499)	(0.0705)	(0.0946)	(0.0638)	(0.0465)
Observations	4118	9805	1378	8427	5569	2590	2110	3476	8337

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

for persistent and transitory shocks, with different forms of borrowing constraints and different levels of the serial correlation on the income innovation.[36] In the case of a persistent shock with serial correlation above 0.95 and a constraint such that the household cannot borrow, then the true insurance coefficient is larger than the Blundell et al. (2008) estimator. In other words, the MPC estimate is biased upwards for this case.

3.6 Impact of Monetary and Fiscal Policy Shocks

Given these MPC estimates, we return to the motivating discussion of the impact of monetary and fiscal policy on consumption by homeownership status. In doing so, we go beyond the standard errors and statistical tests of MPC differences to generate consumption responses to these shocks.

3.6.1 Monetary Policy

To study the effects of money shocks on consumption, we use two pieces of evidence from Cloyne et al. (2020). The first is the effects of monetary shocks on income. The last column of Table 1 in Cloyne et al. (2020) indicates that the mean (after tax) income response to the innovation

[36]The estimated insurance coefficients are effectively 1 minus the MPC.

is dependent on homeowner status. Though the standard errors are large enough that Cloyne et al. (2020) do not point to these income differences as the basis for the pattern of consumption responses, we use these mean values. Second, the first column of Table 1 Cloyne et al. (2020) provides the mean effects on consumption of the monetary innovation, again by homeownership status. Both the income and consumption responses are cumulative over four years. To be clear, these consumption responses to the monetary innovation come from all sources of variation induced by that shock, not just the income channel.

We contrast these estimated consumption responses with those predicted by interacting our MPC estimates with the income variations estimated by Cloyne et al. (2020). The goal is to understand how much of the consumption response in Cloyne et al. (2020) can be explained by a combination of our MPC estimates along with their estimated income responses.

Table 3.8 shows the predicted consumption response to a monetary innovation in which the income process is broken into a transitory and a persistent component.[37] The first two rows of the table are the estimated changes in income and consumption from Cloyne et al. (2020). The lower parts of the table indicate the predicted consumption response using our estimates of MPC by homeowner status for both the transitory and persistent components of persistent income shocks.

For homeowners, the predictions do not accord with the ordering nor the magnitude consistent with the consumption responses reported in Cloyne et al. (2020). The results for the renters in response to the persistent component are close to their findings.

Our predictions differ from their estimated changes in consumption from the persistent component along two dimensions. Firstly, even though we find that mortgagors have the highest consumption response to a monetary policy shock, similar to Cloyne et al. (2020), the increase in consumption is only about 45% of their estimated response. Secondly, Cloyne et al. (2020) find that outright owners reduce their consumption after an expansionary monetary policy shock. Our estimates instead show that outright owners, like mortgagors and renters, increase their consumption following a temporary cut in interest rates.

[37] As in Table 3.7, the consumption responses for different values of the serial correlation parameter are shown in Table C.14.

Table 3.8: Estimated change in consumption following a temporary 25bp cut in monetary policy, based on estimated MPCs from Persistent Shocks.

	Mortgagor	Outright owner	Renter
Estimated change in income (Cloyne et al. (2020))	757.3	585.3	439.3
Estimated change in consumption (Cloyne et al. (2020))	305.8	-72.3	223.3
Transitory component			
MPC	0.112	0.084	0.016
Change in consumption	84.8	49.2	6.9
Persistent component			
MPC	0.179	0.110	0.439
Change in consumption	135.6	64.4	192.9

The table reports the overall dollar change in income/expenditure over the four-year period following a temporary 25bp cut in monetary policy. The first two rows refer to the estimates reported in Table 1 of Cloyne et al. (2020).

3.6.2 Fiscal Policy

In this section, we study the heterogeneous effects of fiscal policy shocks on consumption. Specifically, we use our estimated MPCs to estimate the impact of an unexpected \$500 check from the government in the form of a tax rebate, on consumption by homeownership status. We compare our results to Kaplan and Violante (2014). In this exercise, we only consider the direct impact of the fiscal policy change on income, which is the increase in disposable income due to the check.

Given our estimated MPCs for the transitory component of persistent income shocks, following the tax rebate, the non-durable consumption of mortgagors would increase by \$56, by \$42 for outright owners, and by only \$8 for renters. While Kaplan and Violante (2014) do not explicitly consider a household's homeownership status, for the purposes of this exercise, we assume that mortgagors represent wealthy-HtM, renters are poor-HtM, and finally, outright owners are non-HtM. Based on their model, wealthy-HtM households have a tax rebate coefficient of 44%, followed by poor-HtM (34%) and, finally, non-HtM (7%). The MPC of non-HtM (outright owners) appears to be similar to our estimated MPC.

Clearly, these estimates are much lower than those in Kaplan and Violante (2014), reflecting our relatively low estimated values of MPCs for all groups. These are also smaller responses than reported by Misra and Surico (2014). Again, an important difference is that we only focus on the MPC out of the direct change in disposable income from the tax rebate and thus do not allow for additional income effects through general equilibrium responses.

Table 3.9: Estimated change in consumption following a temporary fiscal action

	Mortgagor	Outright owner	Renter
Estimated change in income	500	500	500
Kaplan and Violante (2014) Estimates			
MPC	0.44	0.07	0.34
Change in consumption	220	35	170
Transitory component Estimates			
MPC	0.112	0.084	0.016
Change in consumption	56	42	8

The table reports the dollar change in income/expenditure in response to a temporary fiscal policy measure. The MPC estimates are from the transitory component reported in Table 3.7.

3.7 Conclusion

The goal of this study was to provide evidence on the marginal propensity to consume by homeownership status. These estimates are of direct importance given the emphasis on homeownership in the literature relating consumption to monetary and fiscal policy innovations

The estimation closely follows Kaplan et al. (2014), with its emphasis on HtM households, which is based upon the methodology for estimating the consumption response to income variations Blundell et al. (2008). There are many treatments presented, distinguished by the type of income shock (transitory, persistent) as well as by the first stage regression and sample period.

We have three main findings. First, homeownership and HtM status should not be equated. Over the sample, the fraction of wealth HtM households with mortgages varies considerably. To the extent that monetary and fiscal policy operates through channels specific to these households, those policies are state-dependent.

Second, there is no robust evidence that the MPC for mortgagors exceeds that of outright homeowners. The estimation with transitory income shocks does produce an estimated MPC for mortgagors which is higher than for others, but in that case, the MPC is higher for outright owners compared to renters. And none of these differences are statistically significant. For persistent income shocks, the MPC out of the persistent component is highest for reenters and lowest for owners. In this case, the MPC for non-HtM mortgagors exceeds (but not statistically) the point estimate for the HtM mortgagors.

Third, sample selection and specifications matter. As demonstrated, the fraction of HtM mortgagors varies considerably across the sample and influences the estimates. Further, if employment status is included in the first-stage regression, thus treated as part of deterministic income, then the MPC estimates are generally higher compared to our baseline.

Compared to other studies, our estimated values for the MPC of outright owners and those with mortgages are lower. These translate into a reduced consumption response to both monetary and fiscal policies.

There is one point of concern with the analysis, which is that the estimation procedure stipulates a linear relationship between consumption growth and income innovations, following Blundell et al. (2008). This may be theoretically inconsistent with the policy function generated by a model with state-dependent borrowing constraints. Understanding the properties of the Blundell et al. (2008) estimator with borrowing constraints was studied in Kaplan and Violante (2010). But there was no housing in that model therefore, further work along these lines is warranted.

In addition, the results of Cloyne et al. (2020) highlight the differential effects of monetary policy on durable consumption expenditures. As in other studies, such as Di Maggio et al. (2017), these responses also depend on housing and mortgage status. Extensions of the environment and empirical methodology to study durable expenditure patterns are of interest.

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Appendix A

Appendix Chapter 1

A.1 Data Definitions and Sources

Table A.1: Data Definitions and Sources

Variable	**Source (Code)**	**Details**
Stock market data		
International Stock Price Index	Goldman Sachs/Bloomberg (GSTHINTL)	Daily index based on the stock price of the 50 S&P 500 companies across 11 sectors with the highest international sales based on the geographical breakdown reported in 10-K filings (detailed financial report that public companies have to submit to the US Securities and Exchange Commission (SEC))
Domestic Stock Price Index	Goldman Sachs/Bloomberg (GSTHAINT)	Daily index based on the stock price of the 50 S&P 500 companies across 11 sectors with the highest domestic revenue exposure based on the geographical breakdown reported in 10-K filings.
S&P 500 index	Yahoo Finance	Daily
VIX	Cboe	Monthly, measures market expectation of volatility based on S&P 500 option prices
Macroeconomic data		
Ind. production	FRED (INDPRO)	Monthly, seasonally adjusted
Ind. prod.: Manufacturing	FRED (IPMAN)	Monthly, seasonally adjusted
Ind. prod.: Consumer goods	FRED (IPCONGD)	Monthly, seasonally adjusted
Ind. prod.: Durable Cons. Goods	FRED (IPDCONGD)	Monthly, seasonally adjusted
Ind. prod.: Non-durables	FRED (IPNCONGD)	Monthly, seasonally adjustedy
Ind. prod.: Construction supply	FRED (IPB54100S)	Monthly, seasonally adjusted
Ind. prod.: Final goods	FRED (IPB50002N)	Monthly, seasonally adjusted
Manufacturing industries sales	BLS	Monthly, seasonally adjusted
Federal Funds Rate	FRED (FEDFUNDS)	Monthly
Broad Dollar Index	FRED (TWEXBMTH)	Monthly
Global Price Index of All Commodities	FRED/IMF (PALLFN-FINDEXM)	Index 2016 = 100, Monthly

Table A.1: Data Definitions and Sources (continued)

Variable	Source	Details
Macroeconomic data (continued)		
CPI (Total All Items)	FRED (CPALTT01USM661S)	Monthly
PPI (all commodities)	FRED/BLS (PPIACO)	Monthly
Exports of goods and services	Census	Monthly, deflated, seasonally adjusted, in per capita terms
Imports of goods and services	Census	Monthly, deflated, seasonally adjusted, in per capita terms
Consumption of goods	BLS	Monthly, seasonally adjusted, in per capita terms
Consumption of durable goods	BLS	Monthly, seasonally adjusted, in per capita terms
Consumption of nondurable goods	BLS	Monthly, seasonally adjusted, in per capita terms
Unemployed rate	FRED (UNRATE)	Monthly, seasonally adjusted
Population	FRED (POPTHM)	Monthly
Hire rate (hires/employed)	Fred/BLS (JTSHIR)	Monthly
Commercial and Industrial Loans	FRED/Federal Reserve Board	Monthly, seasonally adjusted, in per capita terms
Consumer Loans	FRED/Federal Reserve Board	Monthly, seasonally adjusted, in per capita terms
Other variables used for Granger causality tests		
GDP deflator	Congressional Budget Office	Quarterly, Chain Price Index (2012=1), seasonally adjusted
Effective federal funds rate	Congressional Budget Office	Quarterly, % p.a.
Natural Rate ($r*$)	Holsten et al. (2017)	Quarterly, Final data point: 2019Q2
Total factor productivity	Fernald (2012)	Quarterly, Utilization-adjusted, percent change at an annual rate (=400 * change in natural log), Final data point: 2019Q2
Oil shock	Hamilton (2003)	Quarterly, Final data point: 2019Q2
Recession probability	Piger/ FRED (RECPROUSM156N)	Monthly, Smoothed US Recession Probabilities obtained from a dynamic-factor markov-switching model originally proposed by Chauvet (1998) and applied to monthly data for non-farm payroll employment, real personal income excluding transfer payments, the index of industrial production, and real manufacturing and trade sales

Table A.1: Data Definitions and Sources (continued)

Variable	Source	Details
Other variables used for Granger causality tests (continued)		
Coincident Economic Activity Index	Philadelphia Fed/ FRED (USPHCI)	Monthly, seasonally adjusted, based four indicators: the unemployment rate, non-farm payroll employment, average hours worked in manufacturing and wages and salaries
PMI Composite Index	ISM-Manufacturing	Monthly, Purchasing Manager's Index based on a survey of purchasing managers and supply management executives, PMI>50: expansion of the manufacturing sector compared with the previous month, PMI>50:no change, PMI<50: contraction of the manufacturing sector
Chicago Fed National Activity Index	Chicago Fed/ FRED (CFNAI)	Monthly, CFNAI=0: economy is expanding at its historical trend rate of growth, CFNAI<0: below-average growth; CFNAI>0: above-average growth
Macroeconomic Uncertainty	Jurado et al. (2015)	Monthly, extract common variation in uncertainty from hundreds of macroeconomic indicators, reflects uncertainty around forecasts, rather than perceptions by market participants
Financial Uncertainty	Jurado et al. (2015)	Monthly, extract common variation in uncertainty from hundreds of financial indicators, reflects uncertainty around forecasts, rather than perceptions by market participants
Economic Policy Uncertainty (EPU)	Baker et al. (2016)	Monthly, based on the frequency of key words mentioned in US newspapers such as "uncertainty" or "economic" and "Congress", "deficit", "Federal Reserve", "legislation", etc.
Trade Policy Uncertainty (EPU Trade)	Baker et al. (2016)	Monthly, based on the frequency of key words mentioned in US newspapers such as "uncertainty", "tariff" and "war"

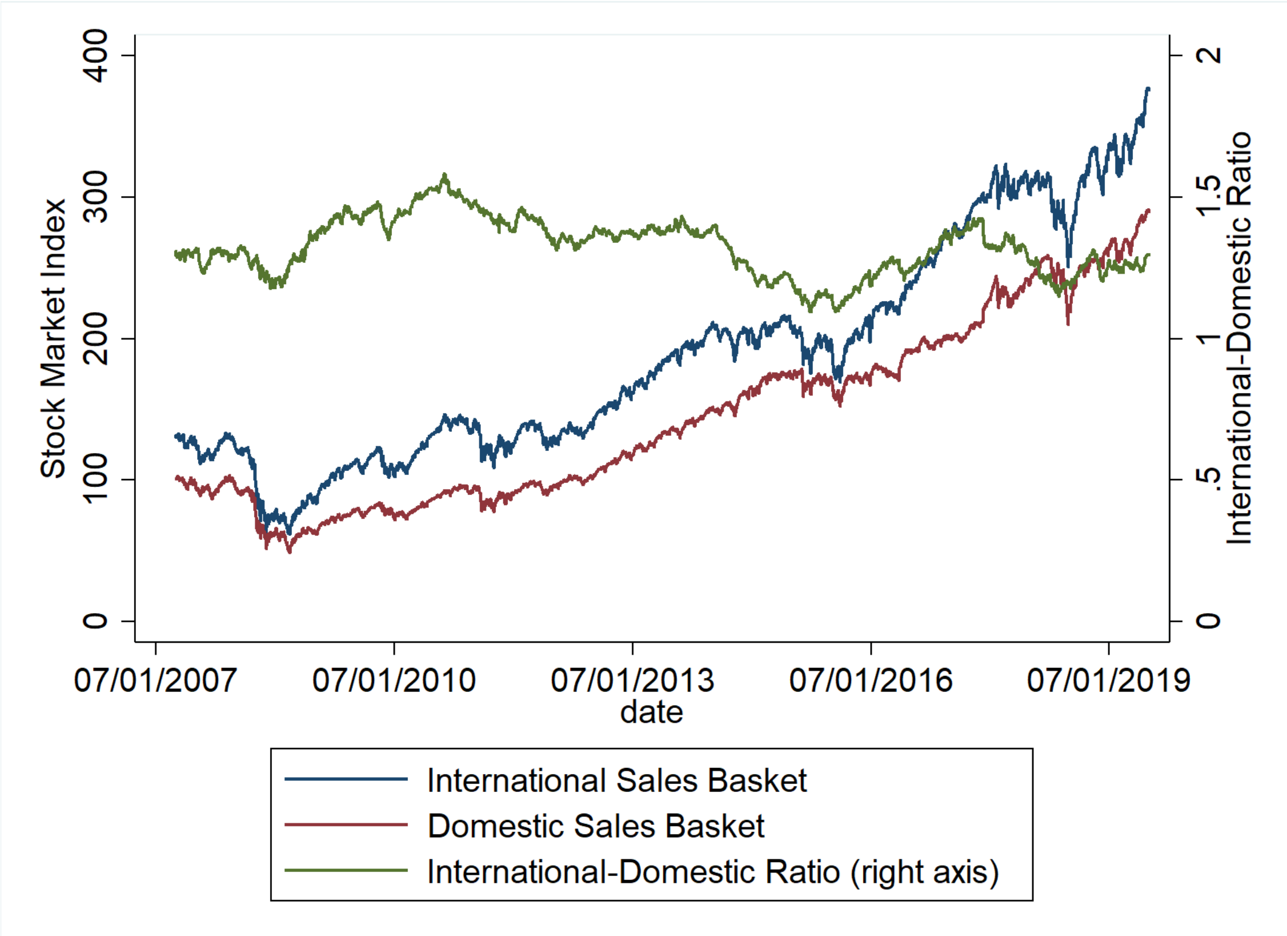


Figure A.1: Stock price indices of the 50 S&P 500 firms with the highest and lowest international sales (left axis) and their ratio (right axis)

A.2 Trade policy statements and identified shocks

A.2.1 Trade policy statements

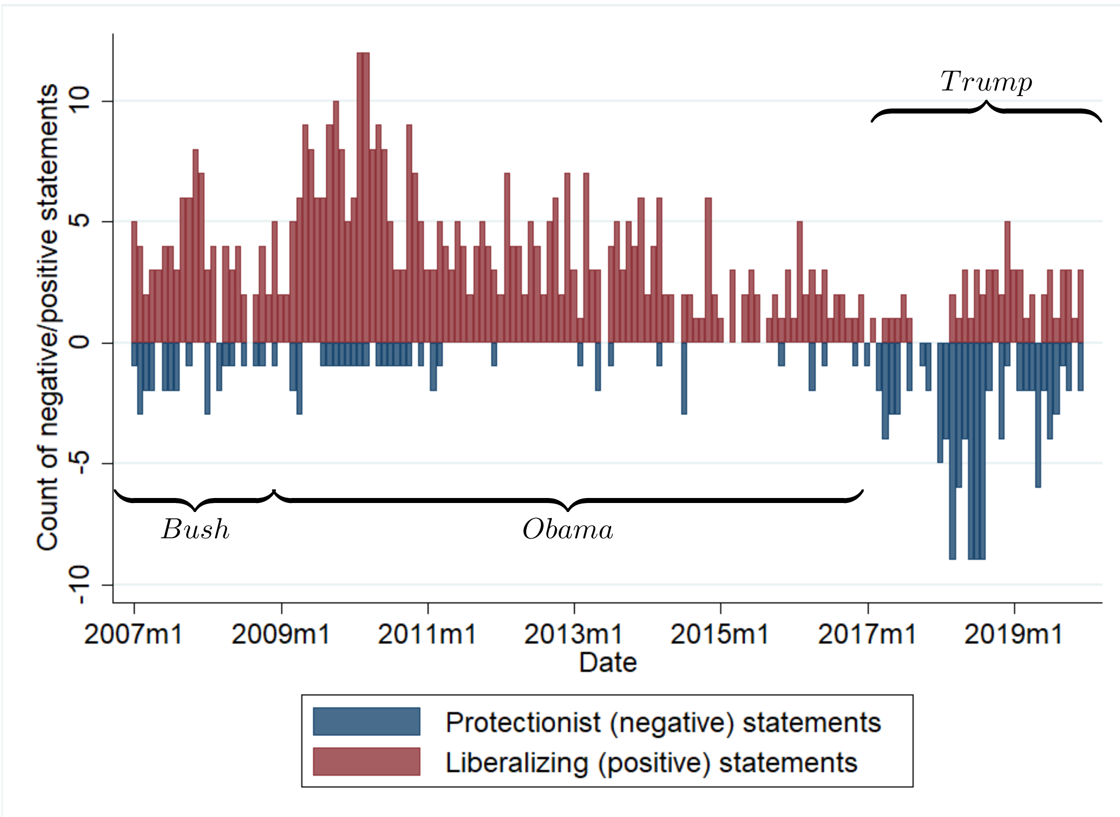


Figure A.2: Number of **major and minor** trade policy statements by sign, aggregated to the monthly frequency.

A.2.2 Identified trade policy shocks

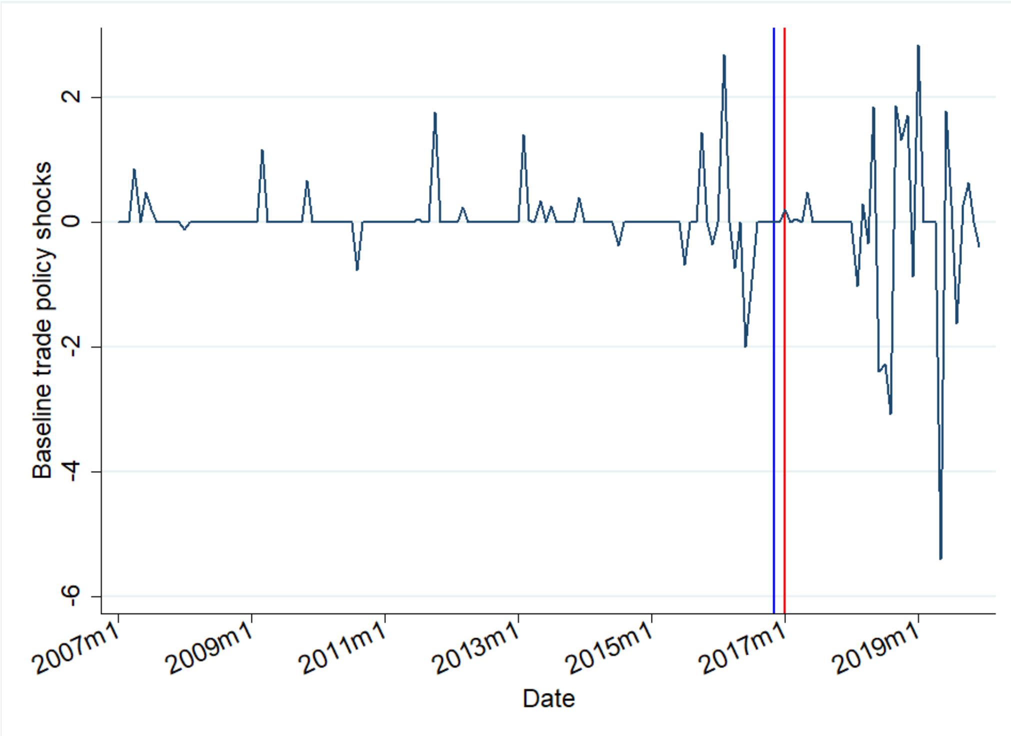


Figure A.3: **Monthly** baseline trade policy shocks. The vertical lines represent the day on which the 2016 election results were announced (blue) and President Trump took office (red).

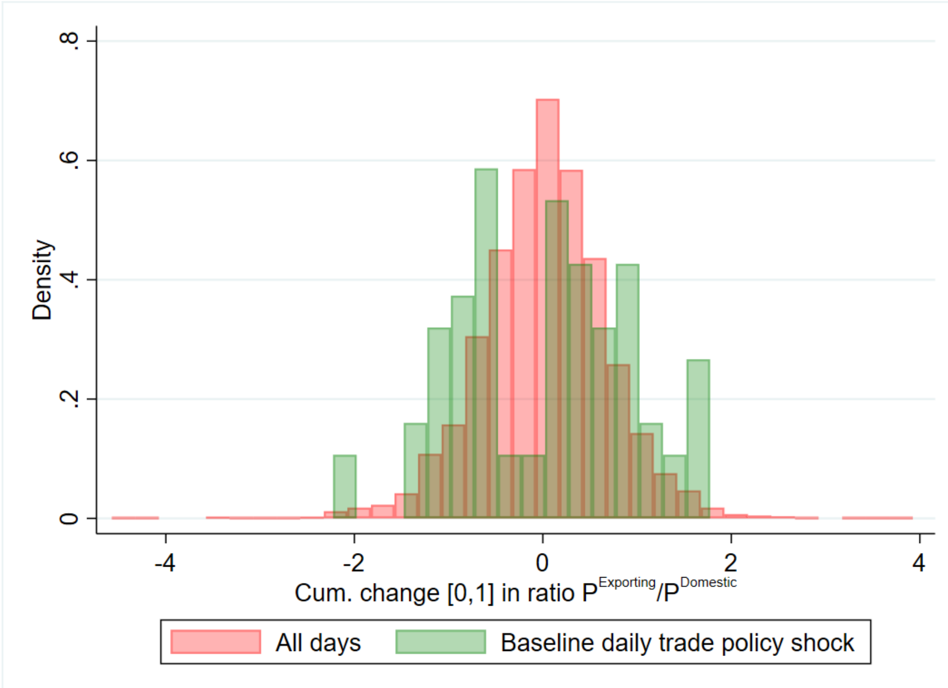


Figure A.4: Distribution of baseline trade policy shocks at the **daily** frequency.

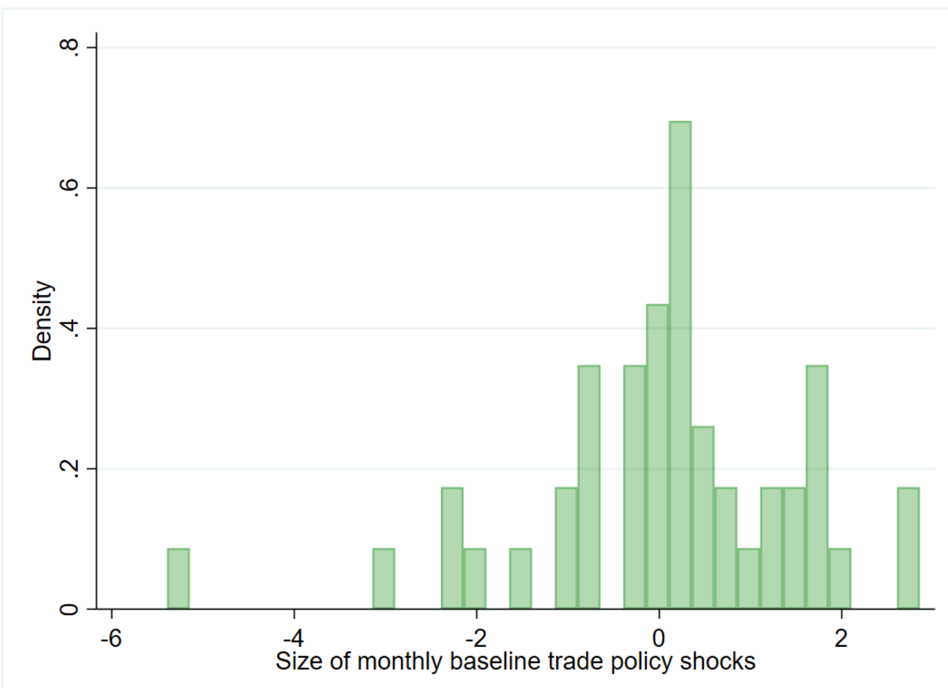


Figure A.5: Distribution of baseline trade policy shocks at the **monthly** frequency.

A.3 Timeline of selected major trade policy events, January 2007 - August 2019

Note: Trade policy implementations and formal approvals are written in **bold**.

1 April 2007	Negotiations for US-Korea Free Trade Agreement completed
30 June 2007	**Signing of the United States-Korea Free Trade Agreement (KORUS FTA)**
1 January 2008	**Full implementation of NAFTA**
29 July 2008	WTO Doha round negotiations collapse over disagreement about special safeguard mechanisms in agricultural trade between the US, China and India
22 September 2008	Trans-Pacific partners and United States launch negotiations for a free trade agreement
18 March 2009	**Government of Mexico retaliates in trucking dispute by imposing tariffs between 10 and 45% on range of US industrial and agricultural products ($2.4 billion worth of exports)**
6 July 2011	Mexico announces plan to drop retaliatory tariffs by 50%
8 July 2011	**Mexico suspends the first by 50% of its retaliatory tariffs**
21 October 2011	**Government of Mexico suspends the last of the retaliatory tariffs it had imposed more than two years ago**
1 November 2011	Trans-Pacific Partnership countries announce the achievement of the broad outlines of the agreement
15 March 2012	**US-Korea Free Trade Agreement takes effect**
27 September 2012	Announcement of agreement with Gulf Cooperation Council to expand trade
15 January 2013	US Trade Representative Kirk plans to negotiate a new international trade agreement on services and notifies the Congress
13 February 2013	US and EU announce launch of negotiations on a Transatlantic Trade and Investment Partnership
28 May 2013	**Signature of United States-Caribbean Community Trade and Investment Framework Agreement**
10 November 2014	US and China announce that they agreed on expanding the scope of goods covered by the Information Technology Agreement

24 July 2015	US leads WTO expansion of the Information Technology Agreement
16 December 2015	**US and WTO members announce final agreement on expanding the Information Technology Agreement**
3 February 2016	**Trans-Pacific Partnership agreement signed**
17 February 2016	US and ASEAN Trade Ministers agree on strengthening trade ties
29 February 2016	Successful T-TIP round in Brussels yields proposed text in the vast majority of negotiating areas
23 June 2016	UK voted to leave the EU (with implications for the UK-US tariff rates applied in the future)
1 July2016	**US and WTO partners start implementing the expanded Information Technology Agreement**
23 January 2017	Trump announces withdrawal from the Trans-Pacific Partnership Negotiations and Agreement
22 February 2017	**WTO Trade Facilitation Agreement (TFA) entered into force**
18 May 2017	The US Congress is notified by Trade Representative Robert Lighthizer that President Trump intends to renegotiate NAFTA
1 March 2018	Trump announces tariffs on all trading partners except Mexico and Canada of 25% on steel and 10% on aluminum
7 March 2018	The EU announces contingent retaliatory tariffs in case the US follows through with tariffs (25% tariff on $3.4 billion of US exports)
22 March 2018	President Trump directs US Trade Representative to impose tariffs (under Section 301 investigation) on Chinese imports worth approx. $50 billion
23 March 2018	**25% on steel and 10% on aluminum tariffs take effect**; China proposes $3 billion of US imports subject to retaliation under Sec. 232
28 March 2018	Announcement that US and South Korea have agreement on a revised US-South Korea Free Trade Agreement
2 April 2018	**China retaliates with 15-25% tariffs affecting US exports $2.4 billion**
3 April 2018	Trump releases list of 1,333 Chinese products worth $50 billion that will potentially hit by a 25% tariffs, which covers $46.2 billion of US imports
4 April 2018	China publishes list of 106 products subject to forthcoming 25% tariffs in retaliation to the US Section 301 tariffs, covering $50 billion of Chinese imports from the US
5 April 2018	President Trump announces having instructed the USTR to investigate possibility of levying $100 billion additional retaliatory tariffs on China

4 May 2018	Trade talks with China conclude without a deal
29 May 2018	President Trump announces imposing a 25% tariff on $50 billion of goods imported from China
31 May 2018	Announcement that Mexico, Canada, and the European Union are no longer exempt from 25% tariff on steel and 10% on aluminum
1 June 2018	**US imposes 25% tariff on steel and 10% on aluminum from the EU, Canada, and Mexico**
5 June 2018	**Effective immediately Mexico announces to increase duties on a number of US and other foreign-origin products. Goods listed will be subject to 15%, 20%, or 25% duties.**
15 June 2018	Trump Administration announces to impose a 25% tariff on Chinese goods (first applied to $34 billion worth of imports from China and later on an additional $16 billion (in total 1,102 tariff lines targeted), mostly on industrial sector products.
16 June 2018	China threatens to retaliate through 25% tariff on approximately $34 billion worth of US vehicles and agricultural products; later tariffs worth $16 billion on medical equipment, chemicals and energy products from the US should be added.
18 June 2018	President Trump asks USTR to identify an additional $200 billion worth of Chinese goods for a future 10% tariff. Additional threat to impose tariffs on $200 billion of tariffs if China retaliates.
22 June 2018	**EU retaliates activating its previously announced tariffs of 10-25% tariffs on initial list covering $3.2 billion of US products.**
1 July 2018	**Canada implements tariffs on US products worth $12.8 billions, half of these affect steel and aluminum (25% tariff rate)**
6 July 2018	**Additional tariffs of 25% on Chinese products that are part of the first list under Sect. 301 take effect, $34 billion worth of Chinese imports. China said it immediately imposes retaliatory tariffs of a similar size.**
10 July 2018	USTR announces it will impose a 10% additional tariff on Chinese imports worth $200 billion
20 July 2018	Trump states he intends to impose tariffs on all US imports from China with a total value of $504 billion. This would affect the remaining $262 billion of imports not already subject to previously announced tariffs.
1 August 2018	President Trump contemplates increasing the previously announced additional tariff on $200 billion worth of US imports from China from 10% to 25%.

3 August 2018	China threatens to add tariffs of 5-25% on $60 billion of US goods.
7 August 2018	USTR releases the final list of Chinese imports worth $16 billion that will be affected by a 25% additional tariff (list contains almost all of the tariff lines originally proposed on June 15, 2018). China retaliates announcing to levy a 25% additional tariff on $16 billion of US exports (333 different goods affected).
23 August 2018	**The additional tariffs on $16 billion worth of imports from China go into effect.**
27 August 2018	USTR announces that the US and Mexico have "reached a preliminary agreement in principle" to revise NAFTA.
18 September 2018	President Trump formerly announces to impose duties on $200 billion worth of imports from China. China announces to implement its retaliatory tariffs on approx. $60 billion US goods.
24 September 2018	**US tariffs on Chinese imports worth $200 billion that were announced on September 17 are implemented. Retaliatory tariffs by China on $60 billion of US imports announced on September 18 also take effect. US and South Korea sign the revised US-Korea Free Trade Agreement (KORUS).**
30 September 2018	The US reaches an agreement with Canada and Mexico on the updated NAFTA, now: United States-Mexico-Canada Agreement (USMCA).
16 October 2018	The US Congress is informed of Trump's intention to negotiate Free Trade Agreements with the EU, Japan and the UK.
30 November 2018	**US-Mexico-Canada Free Trade Agreement officially signed.**
1 December 2018	Following a bilateral meeting, President Trump announces that on January 1, 2019, the 10% tariffs on $200 billion worth of Chinese products will not increase to 25%. In return, China agrees to by industrial, agricultural, energy and other product from the US
9 January 2019	The US and China conducted a two-day trade talk to de-escalate the tariff war.
8 April 2019	USTR releases a list of EU products on which it plans to levy tariffs in response to the EU's aircraft subsidy.
5 May 2019	USTR proposes to impose additional tariffs on EU products worth $21 billion in response to EU Aircraft Subsidies.
10 May 2019	President Trump announces a 10% section 301 tariffs on imports from China worth approx. $300 billion as of September 1, 2019.

13 May 2019 — China announces tariff retaliation on $75 billion worth of imported goods (5%-10% tariff rate) which will take effect in two steps on 1 September 2019 and on 15 December 2019. In response, President Trump intends to increase the tariffs on approx. $550 billion worth of Chinese imports by 5%. For the 25% tariffs on approx. $250 billion worth of Chinese imports, tariffs will increase to 30%, effective October 1. The 10% tariffs on approx. $300 billion worth of Chinese imports announced earlier would be implemented at a higher rate (15%).

17 May 2019 — Trump intends to increase the 10% section 301 tariffs on $200 billion imports from China to 25% and threatens to impose tariffs on all remaining imports from China.

30 May 2019 — **The Trump Administration officially increases tariffs on $200 billion imports from China to 25%.**

1 June 2019 — China announces to increase its retaliatory tariff on US exports worth $60 billion

1 July 2019 — The Trump administration suggests that certain automobiles and automobile parts represent a threat to national security and that their imports need to be limited. **Additionally, it is announced that the Section 232 tariffs on Canadian and Mexican steel and aluminum are removed. In return, Canada and Mexico agree to removing their retaliatory tariffs.**

1 August 2019 — President Trump announces his intent to impose gradually increasing tariffs on all imports from Mexico starting on June 10, 2019 (first 5%, then 10%, then 15%, then 20%, then 25%).

23 August 2019 — **China raises tariffs on $60 billion of US goods as announced on May 13.**

A.4 Examples of firms included in the International and Domestic Exposure baskets

Table A.2: Example companies and sectors included in **Domestic** sales basket.

Company name	**NAICS 1 classifcation**
Domestic sales basket	
Aetna Inc	Insurance Agencies and Brokerages
Allstate Corp	Insurance Agencies and Brokerages
Altria Group Inc	Other Farm Product Raw Material Merchant Wholesalers
Anthem Inc	Direct Life Insurance Carriers
AutoZone Inc	Automotive Parts and Accessories Stores
BB&T Corp	Commercial Banking
Centene Corp	All Other Miscellaneous Ambulatory Health Care Services
Charles Schwab	Investment Advice
CSX Corp	Line-Haul railroads
CVS Health Corp	Insurance Agencies and Brokerages
Dollar General	Supermarkets and Other Grocery Stores
Dominion Resources	Other Electric Power Generation
Fiserv Inc	Custom Computer Programming Services
Fox Corp	Television Broadcasting Stations
J.B. Hunt Transport Services Inc	Freight Transportation Arrangement
Kroger Co.	Supermarkets and Other Grocery Stores
Lowe's Companies Inc	Home Centers
Marathon Petroleum Corp	Petroleum and Petroleum Products Merchant Wholesalers
Norfolk Southern Corp	Freight Transportation Arrangement
Nucor Corp	Fabricated Structural Metal Manufacturing
Paychex Inc	Payroll Services
PNC Financial Svc. Grp	Commercial Banking
Public Storage	General Warehousing and Storage
Ross Stores Inc	Department Stores
Southern Co	Other Electric Power Generation
SunTrust Banks Inc	Commercial Banking
T-Mobile US Inc	All Other Telecommunications
U.S. Bancorp	Commercial Banking
Verizon Communications Inc	Telephone Communications
Wells Fargo And Company	Commercial Banking

Table A.3: Example companies and sectors included in **International** sales basket.

Company name	**NAICS 1 classifcation**
International sales basket	
Abbott Laboratories	In-Vitro Diagnostic Substance Manufacturing
AES Corp	Other Electric Power Generation
Aflac Inc	Insurance Agencies and Brokerage
Alphabet Inc	Custom Computer Programming Services
Aon Corp	Insurance Agencies and Brokerages
Boeing	Aircraft manufacturing
Baker Hughes	Oil and Gas Field Machinery and Equipment Manufacturing
Citigroup Inc	Comercial Banking
Colgate-Palmolive	Soap and Other Detergent Manufacturing
Coty Inc	Drugs and Druggists' Sundries Merchant Wholesalers
Danaher Corp	Instruments and Related Products Manufacturing for Measuring, Displaying, and Controlling Industrial Process Variables
Facebook Inc	Web Search Portals and All Other Information Services
General Electric	Turbine and Turbine Generator Set Units Manufacturing
Intel Corp	Semiconductor and Related Device Manufacturing
Internat. Flavors And Fragrances Inc	Miscellaneous Chemical Product and Preparation Manufacturing
McDonald's Corp	Full-Service Restaurants
Mettler Toledo International Inc	Analytical Laboratory Instrument Manufacturing
Microchip Technology	Semiconductor and Related Device Manufacturing
Mondelez Intl	Cheese Manufacturing
Newmont Mining Corp	All Other Metal Ore Mining
Philip Morris Intl	Tobacco Manufacturing
Priceline	Travel agencies
QUALCOMM Inc	Radio and Television Broadcasting and Wireless Communications Equipment Manufacturing
TechnipFMC	Oil and Gas Field Machinery and Equipment Manufacturing
Texas Instruments	Semiconductor and Related Device Manufacturing
Tiffany And Co	Jewelry Stores
United Technologies Corp	Office Administrative Services
Westinghouse Air Break Technologies	Railroad Rolling Stock Manufacturing
Wynn Resorts	Hotels (except Casino Hotels) and Motels
3M Co	Surgical and Medical Instrument Manufacturing

A.5 Examples of trade policy statements that are not unequivocally liberalizing or protectionist

This section provides the rationale for being ex-ante agnostic about the sign of our shocks. There are at least three types of situations in which classification into liberalizing and protectionist is difficult.

First, some announcements and implementations contain only slight but potentially important departures from previous announcements. For example, on April 30, 2018, President Trump voiced his intention to follow through with the previously announced punitive tariffs on steel and aluminum imports. However, Mexico, Canada, and the EU would be exempt until the end of May 2018 to allow for further negotiations. It is unclear whether markets are now upset that the initial threat will be realized or whether they are relieved that large trade partners are initially excluded. Hence, the difficulty in classifying statements may be due to a lack of information on the market's prior expectations. Similarly, there are statements announcing to follow through with a tariff but at a lower rate than previously stated.

Second, on some days, several statements are issued, which may offset each other. For example, on May 17, 2019, President Trump announced that tariffs on Canadian and Mexican steel and aluminum would be removed. Simultaneously, the US administration declared that some automobiles and automobile parts threatened national security. The statement further says that talks with the EU, Japan, and other trade partners would be held to ask them to reduce their auto exports. It is difficult to judge which of these two statements dominates.

Third, statements that refer to the US filing WTO complaints about trade partners' policies could be protectionist as they may lead to retaliations and could ultimately culminate in a trade war. However, they could also be liberalizing if the complaint is justified and leads trade partners to remove the barrier, which opens up the market to US exporters.

A.6 Examples of liberalizing, protectionist and unclassified official statements

1. Trade liberalizing official statement

   "**US, EU Announce Decision to Launch Negotiations on a Transatlantic Trade and Investment Partnership.** We, the Leaders of the United States and the European Union, are pleased to announce that, based on recommendations from the US-EU High Level Working Group on Jobs and Growth co-chaired by United States Trade Representative Kirk and European Trade Commissioner De Gucht, the United States and the European Union will each initiate the internal procedures necessary to launch negotiations on a Transatlantic Trade and Investment Partnership. (...)" (The White House, 13.02.2013)

2. Protectionist official statement

   "**Under Section 301 Action, USTR Releases Proposed Tariff List on Chinese Products.**

   As part of the US response to China's unfair trade practices related to the forced transfer of US technology and intellectual property, the Office of the US Trade Representative (USTR) today published a proposed list of products imported from China that could be subject to additional tariffs. (...) Sectors subject to the proposed tariffs include industries such as aerospace, information and communication technology, robotics, and machinery. The proposed list covers approximately 1,300 separate tariff lines and will undergo further review in a public notice and comment process, including a hearing. (...)" (Office of the US Trade Representative, 03.04.2018)

3. Unclassified (neither protectionist nor liberalizing) statements

   [Alteration of previously announced imposition of punitive tariffs on steel]

   "**Presidential Proclamation Adjusting Imports of Steel into the United States.**

In Proclamation 9705 of March 8, 2018 (Adjusting Imports of Steel Into the United States), I concurred in the Secretary's finding that steel mill articles are being imported into the United States in such quantities and under such circumstances as to threaten to impair the national security of the United States, and decided to adjust the imports of steel mill articles (...) by imposing a 25 percent ad valorem tariff on such articles imported from all countries except Canada and Mexico. (...) Recognizing that each of these countries and the EU has an important security relationship with the United States, I determined that the necessary and appropriate means to address the threat to national security posed by imports of steel articles from these countries was to continue the ongoing discussions and to exempt steel articles imports from these countries from the tariff proclaimed in Proclamation 9705 until May 1, 2018." (The White House, 30.04.2018)

A.6.1 Testing for trade policy shock exogeneity

As described in Section 1.4.2, the fact that conventional monetary policy shocks are not yet available for recent time periods, forces us to build our own series. We do so by estimating the standard Taylor rule

$$i_t = r* + \pi_t + \alpha_\pi(\pi_t - \pi*) + \alpha_y(y_t - y*), \tag{A.1}$$

where i_t is the interest rate set by the central bank. $r*$ denotes the long-run equilibrium interest rate, π_t the rate of inflation, $\pi*$ the inflation target, y_t output measured by the log of real GDP and $y*$ the potential output. Equation (A.1) is estimated for the pre-sample period 1961Q1-2006Q4 using data on the equilibrium interest rate from Holsten et al. (2017). Estimates of the output gap, the GDP deflator and federal funds rates are from the Congressional Budget Office. The estimated coefficients $\hat{\alpha_\pi}$ and $\hat{\alpha_y}$ are then used for predicting interest rates for 2007Q1-2019Q2. Deviations of the realized federal funds rate from our predicted values are used as monetary shocks. To verify the robustness of our Granger causality tests we also estimate the Taylor rule for different sub-samples. Furthermore, we construct a second shock series by using equal weights for the output and inflation gaps, i.e. $\alpha_\pi = \alpha_y = 0.5$, as originally suggested by Taylor (1993). Both shock series bear the shortcoming that they may be inaccurate since interest rates were constrained by the zero lower bound after the Great Recession.

Table A.4: Orthogonality between trade policy shocks and external macroeconomic shocks, current, past and future economic conditions

Instruments	**Correlation**	(p-value)	**Granger F-test**	(p-value)
Macroeconomic shocks				
TFP growth shocks[a]	0.0382	(0.7925)	1.3589	(0.2678)
Oil price shock shocks[b]	-0.0627	(0.6655)	1.5742	(0.2189)
Terms of trade shocks[c]	-0.0500	(0.5418)	0.32807	(0.7209)
Monetary policy shocks I.[d]	-0.1005	(0.4827)	1.4446	(0.2468)
Monetary policy shocks II.[e]	0.2689	(0.0564)	2.181	(0.1250)
Current/past economic conditions				
Unemployment rate	0.1029	(0.2072)	1.4265	(0.2435)
Growth in industrial production	-0.1442	(0.0774)	0.79187	(0.4550)
Recession probability	0.0301	(0.7123)	0.0607	(0.9411)
Economic Activity Index (Growth)	-0.0044	(0.9575)	1.5475	(0.2163)
PMI Composite Index	-0.0804	(0.3249)	0.65687	(0.5200)
Chicago Fed Nat. Activity Index	-0.0926	(0.2566)	0.90597	(0.4064)
Expected future economic conditions				
Consumer confidence (Univ. of Michigan)	-0.1155	(0.1566)	1.0117	(0.3662)
Business confidence (OECD)	-0.0779	(0.3400)	0.47563	(0.6225)
Real oil price[f]	0.0213	(0.7947)	1.2605	(0.2866)
S&P500 return	0.0352	(0.6676)	3.4949*	(0.0330)
Uncertainty				
Macroecon. uncertainty (Jurado et al.)	0.0625	(0.4476)	0.22939	(0.7953)
Financial uncertainty (Jurado et al.)	0.0234	(0.7766)	0.6776	(0.5095)
VIX[k]	0.075	(0.3549)	2.7497*	(0.0673)
EPU (Baker et al.)	0.0336	(0.6815)	0.52118	(0.5949)
EPU Trade → Trade policy shock	-0.2837***	(0.0004)	2.5232	(0.0837)
Trade policy shock → EPU Trade			6.8834**	(0.0014)

NOTE: Cells contain pairwise correlations and Granger causality tests performed with two lags of each instrument (*p<0.05, **p<0.01, ***p<0.001). The dependent variable is the **baseline trade policy shock** which is aggregated to the quarterly frequency when testing for orthogonality wrt. TFP, monetary policy and oil price shocks and is kept at the monthly frequency, otherwise. For detailed data sources see Appendix A.1.
a - Following Caldara et al. (2020), residuals are obtained from an AR(1) model of the log-difference in total factor productivity (TFP) adjusted for utilization; see Fernald (2012).
b - Crude oil supply shocks are obtained from Hamilton (2003).
c - Terms of trade shocks are proxied by estimating an AR(1) model of the ratio of export and import prices and extracting the residuals.
d - From estimating a Taylor rule for the pre-sample period (1961q1-2006q4) and taking the difference between the actual and the predicted federal funds rate.
e - Using the standard Taylor rule and imposing equal weights on the output and inflation gap (0.5), predicted values for the interest rate are calculated. Shocks are measured by the difference between the realized and the predicted federal funds rate.
f - Unlike when testing for orthogonality wrt. oil price shocks, trade policy shocks are included at the monthly frequency when verifying their relationship to real oil prices.

Table A.5: Endogeneity of the stock price ratio of exporting and domestically oriented firms

Instruments	**Correlation**	(p-value)	**Granger F-test**	(p-value)
Macroeconomic shocks				
TFP growth shocks[a]	-0.0755	(0.6022)	0.51656	(0.6002)
Oil price shock shocks[b]	0.2164	(0.1312)	1.4355	(0.2492)
Terms of trade shocks[c]	-0.1876*	(0.0210)	0.86042	(0.4251)
Monetary policy shocks I.[d]	-0.6290***	(0.0000)	1.4439	(0.2470)
Monetary policy shocks II.[e]	-0.0721	(0.6153)	9.9323***	(0.0003)
Current/past economic conditions				
Unemployment rate	0.6919***	(0.0000)	4.2079*	(0.0167)
Growth in industrial production	0.2607**	(0.0012)	0.14725	(0.8632)
Recession probability	-0.1542	(0.0579)	0.17705	(0.8379)
Economic Activity Index (Growth)	0.0493	(0.5476)	1.8169	(0.1662)
PMI Composite Index	0.3986***	(0.0000)	2.0425	(0.1334)
Chicago Fed Nat. Activity Index	0.1922*	(0.0177)	1.7277	(0.1813)
Expected future economic conditions				
Consumer confidence (Univ. of Michigan)	-0.4580***	(0.0000)	1.5459	(0.2166)
Business confidence (OECD)	0.3992***	(0.0000)	4.9417**	(0.0084)
Real oil price[f]	0.5663***	(0.0000)	2.4473	(0.0901)
S&P500 return	0.1893*	(0.0199)	0.38541	(0.6809)
Uncertainty				
Macroecon. uncertainty (Jurado et al.)	0.0029	(0.9720)	0.31015	(0.7338)
Financial uncertainty (Jurado et al.)	0.0445	(0.5889)	6.364**	(0.0023)
VIX[k]	-0.0050	(0.9513)	2.5996	(0.0778)
EPU (Baker et al.)	0.3896***	(0.0000)	2.9456	(0.0557)
EPU Trade → Stock ratio	-0.1755*	(0.0306)	0.25306	(0.7768)
Stock ratio → EPU Trade			0.02377	(0.9765)

NOTE: Cells contain pairwise correlations and Granger causality tests performed with two lags of each instrument (*p<0.05, **p<0.01, ***p<0.001). The dependent variable is the **stock price ratio of exporting and domestically oriented firms** which is aggregated to the quarterly frequency when testing for orthogonality wrt. TFP, monetary policy and oil price shocks and is kept at the monthly frequency, otherwise. For detailed data sources see Appendix A.1.
a, *b*, *c*, *d*, *e*, *f* - See Table A.4 above.

A.7 Plots of impulse responses

A.7.1 Trade liberalizing vs protectionist shocks

The following plots show the responses of a trade liberalizing shock (first column) and a protectionist shock (second column), estimated from Equation (1.4). To facilitate the interpretation, we flip the sign of the coefficient obtained for a protectionist shock. The light-shaded area represents the 90% confidence interval and the dark-shaded area indicates the 68% confidence interval. The third column shows the t-statistics from a Wald test with null hypothesis that the response to a liberalizing and protectionist shock are equal. The dark-shaded area represents the 90% acceptance interval (±1.645).

Liberalizing shock **Protectionist shock** **t-statistics**

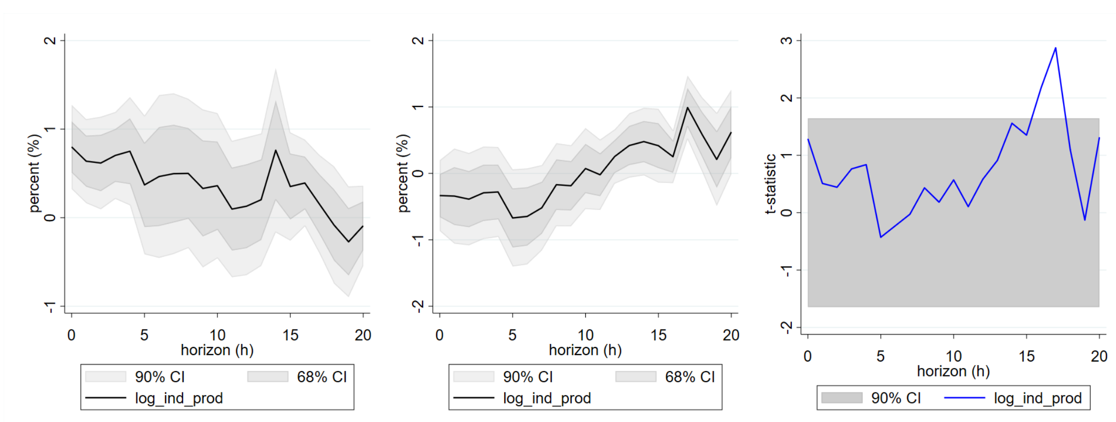


Figure A.6: Industrial production (in logs)

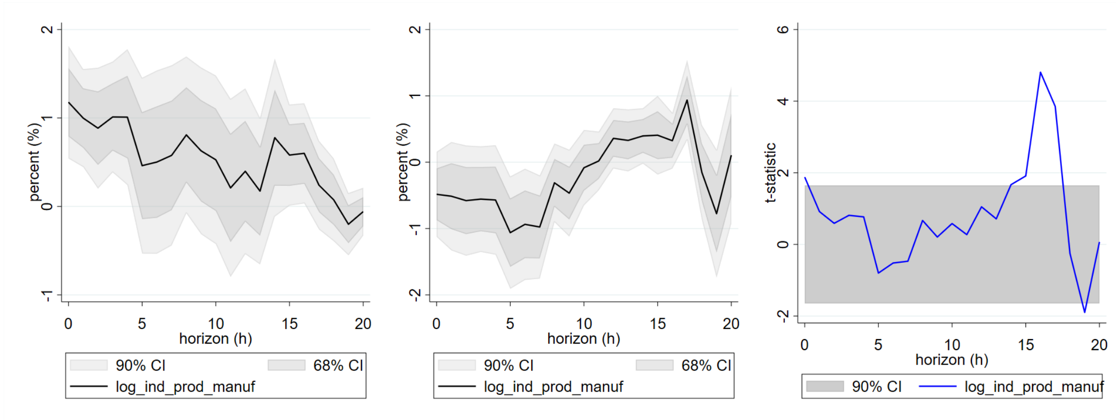


Figure A.7: Industrial production: Manufacturing (in logs)

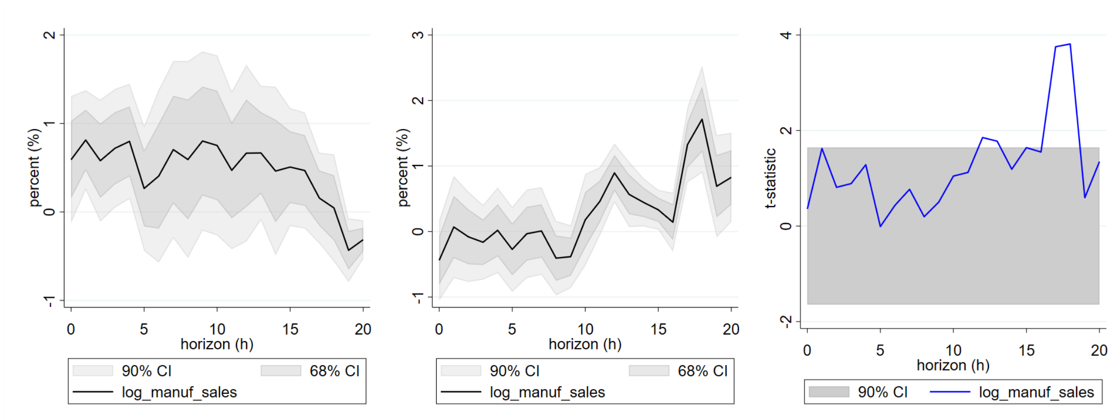


Figure A.8: Manufacturing sales (in logs)

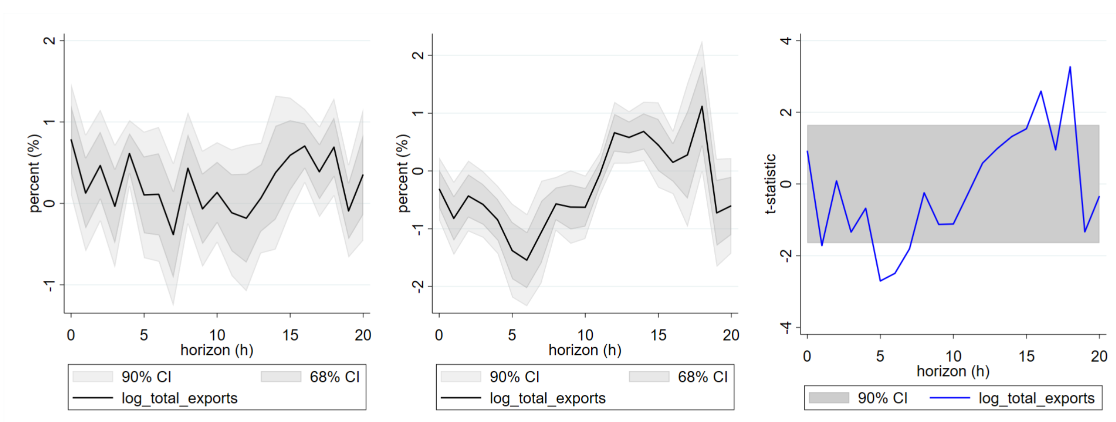


Figure A.9: Exports (in logs)

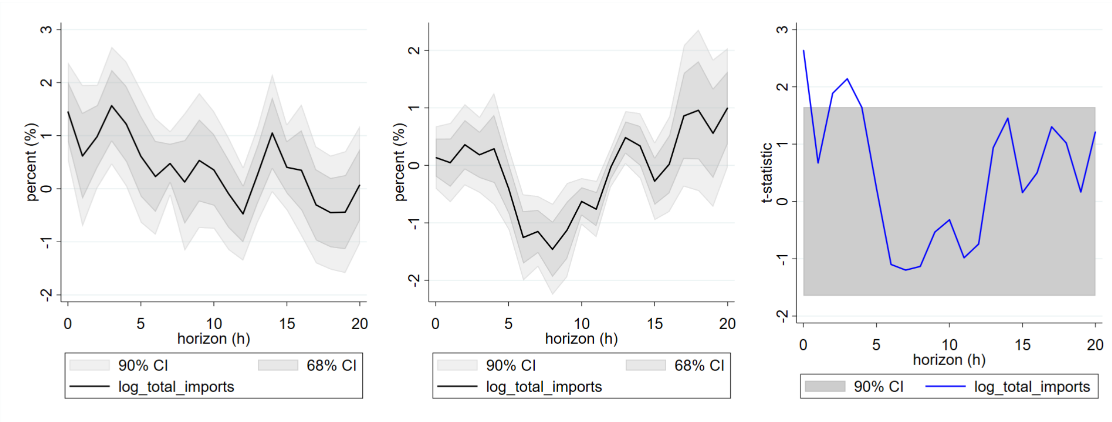


Figure A.10: Imports (in logs)

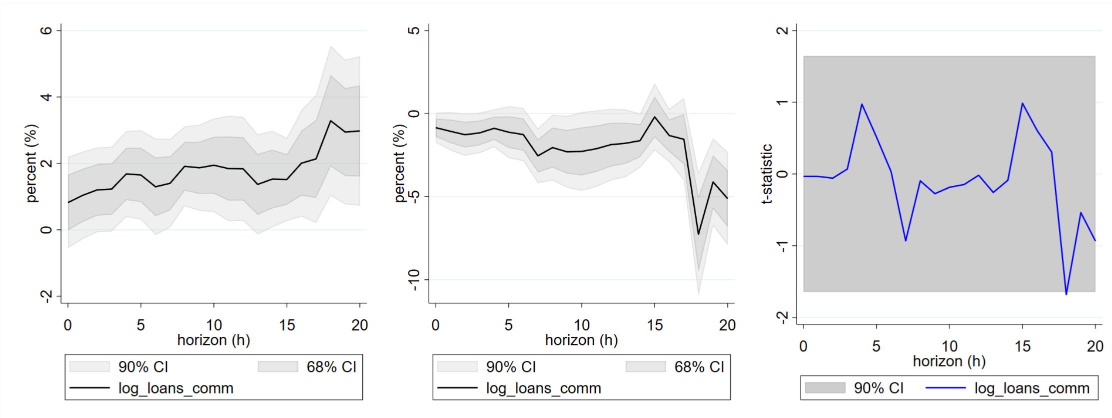


Figure A.11: Commercial loans (in logs)

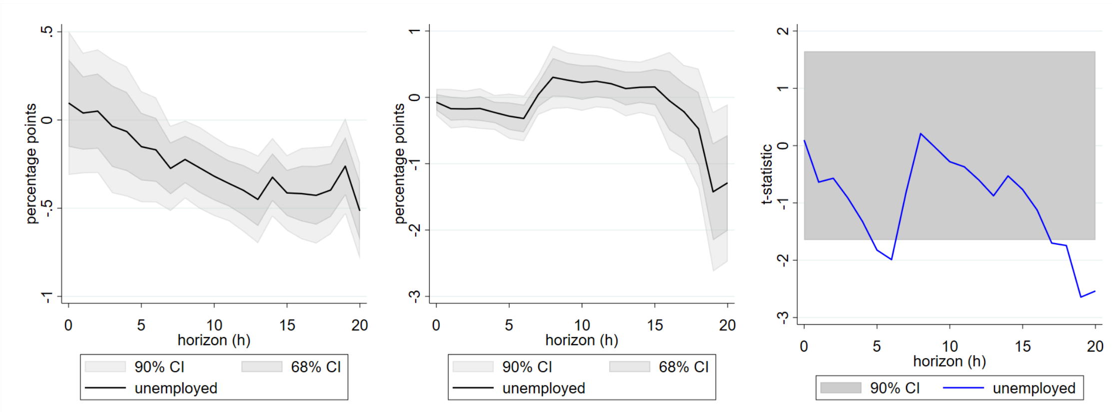


Figure A.12: Unemployment rate

A.7.2 Implementation vs Announcement shocks

The following plots show the responses of a shock associated with an implementation of trade policy (first column) and a shock concerning announcements of trade policies (middle column), estimated from Equation (1.4). The light and dark-shaded areas represent the 90% and 68% confidence intervals, respectively. The third column shows the t-statistics from testing the null hypothesis that the response to an implementation and an announcement shock are equal. The dark-shaded area represents the 90% acceptance interval (±1.645). If the blue line lies above the acceptance interval, there is evidence that the effect of a trade policy implementation is significantly more positive than an announcement - vice versa, if it falls below the interval.

Implementation **Announcement** **t-statistics**

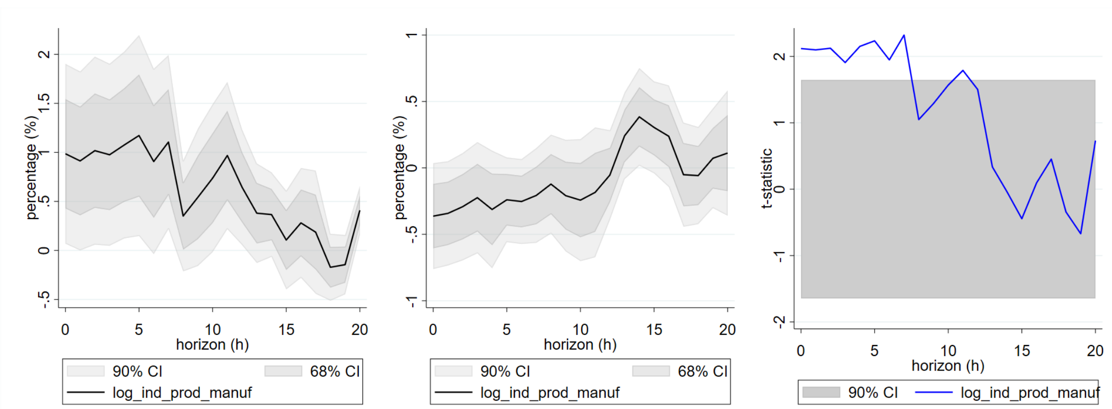


Figure A.13: Industrial production: Manufacturing (in logs)

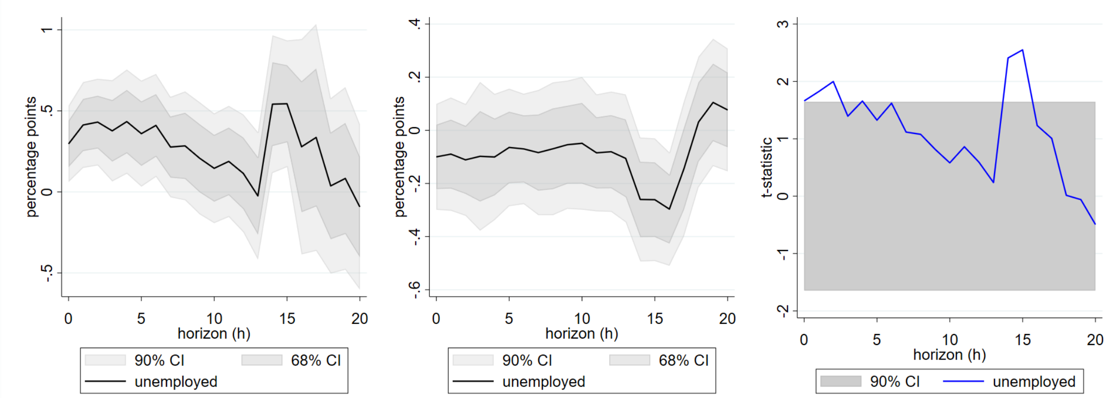


Figure A.14: Unemployment rate

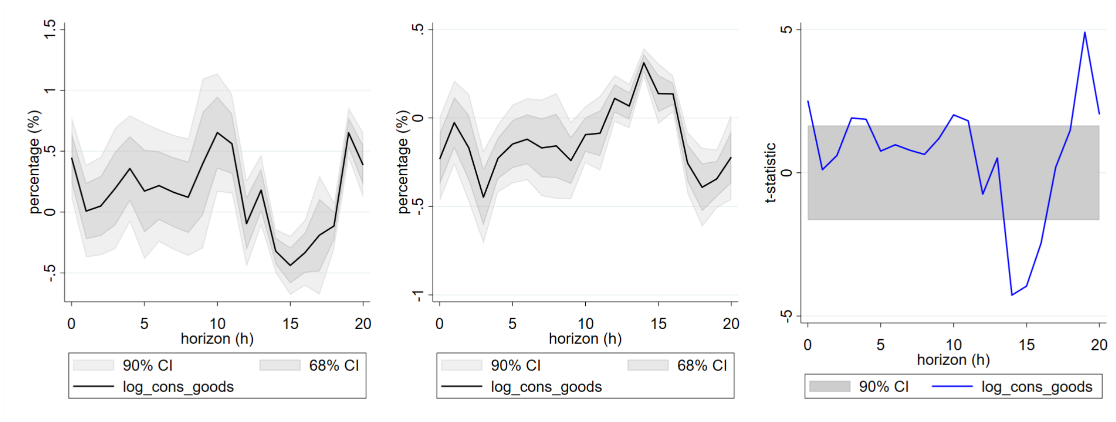


Figure A.15: Consumption of goods (in logs)

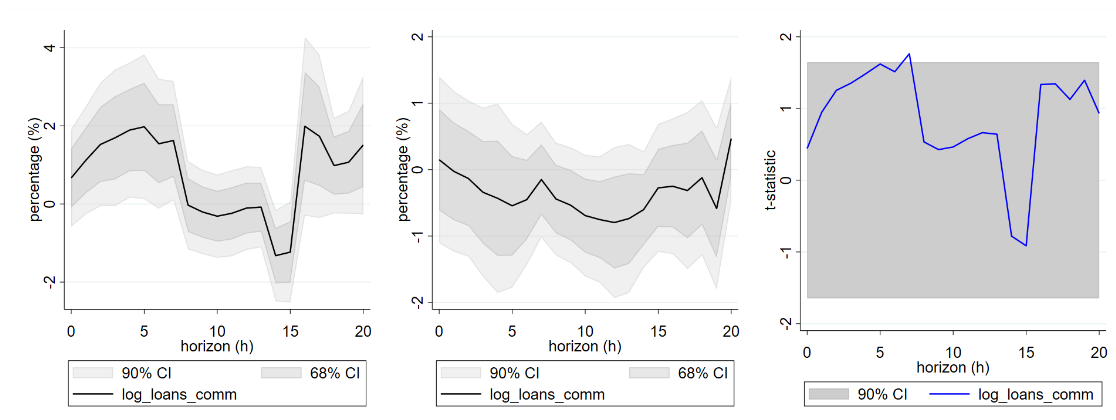


Figure A.16: Commercial loans (in logs)

A.7.3 US vs Foreign shocks

The following plots show the responses of a shock originating from the US (first column) and a shock representing the response of a foreign trade partner (middle column), estimated from Equation (1.4). The light and dark-shaded areas represent the 90% and 68% confidence intervals, respectively. The third column shows the t-statistics from the test with null hypothesis that the response to a US shock and a foreign shock are equal. The dark-shaded area represents the 90% acceptance interval (±1.645). If the blue line lies above the acceptance interval, there is evidence that the effect of a trade policy change initiated by the US is significantly more positive than of those made by a trade partner - vice versa, if the line falls below the interval.

US **Foreign** **t-statistics**

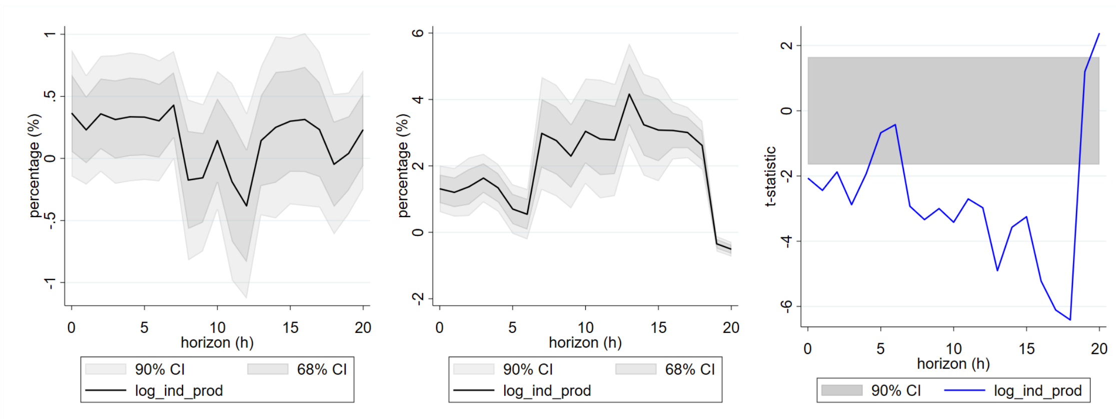


Figure A.17: Industrial production (in logs)

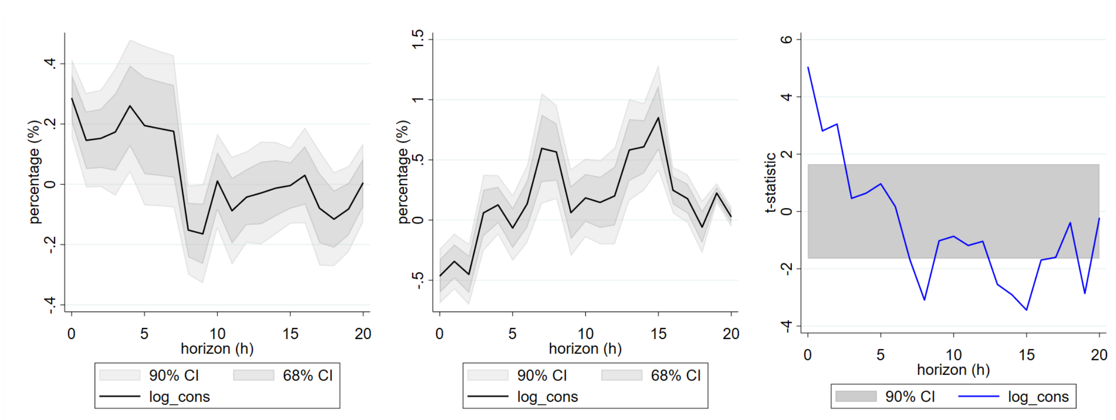


Figure A.18: Consumption (in logs)

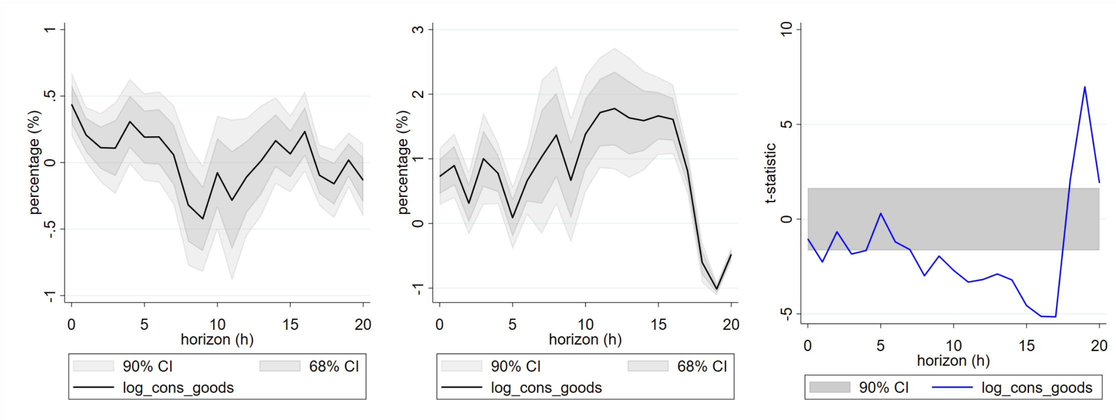


Figure A.19: Consumption of goods (in logs)

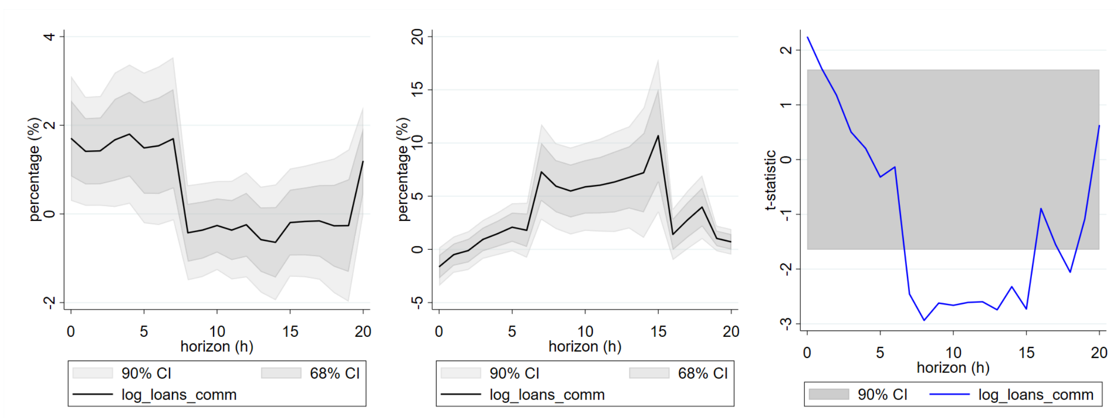


Figure A.20: Commercial loans (in logs)

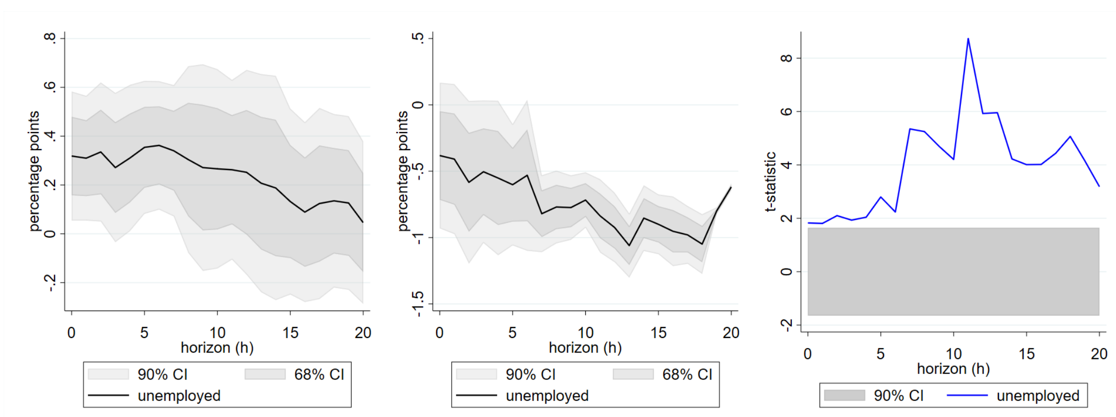


Figure A.21: Unemployment rate

A.7.4 Non-linearities in shock size

Based of estimating Equation (1.5), the following plots show the coefficient for the quadratic term for each horizon (first column) and the associated t-statistics together with the 90% acceptance interval (±1.645) (middle column). The third column shows the p-values at each horizon from a likelihood-ratio test of whether the linear-only nested model fares better than the model with the squared shock term. If the t-statistics (blue line, second column) falls outside the grey shaded area and the p-value (blue line, third column) stays within, we fail to accept the null hypothesis of no size dependence. Only plots of variables for which we find evidence of size dependence on impact are displayed.

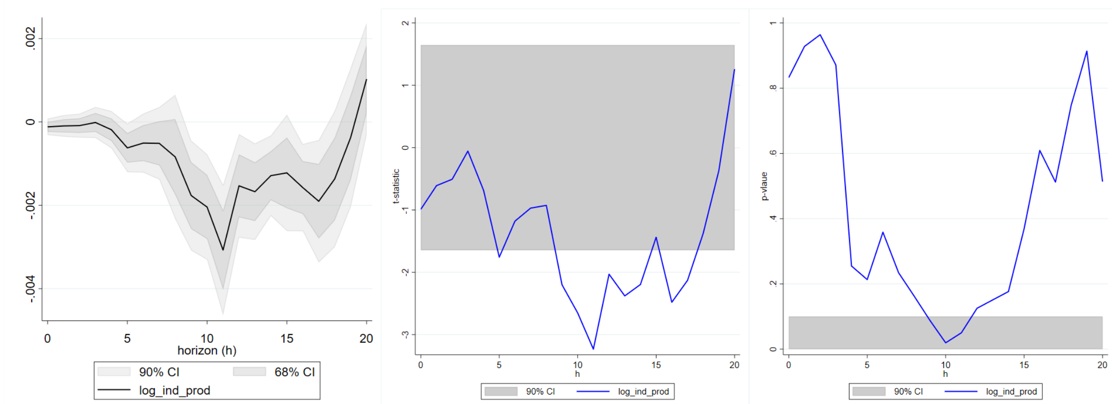


Figure A.22: Industrial production (in logs)

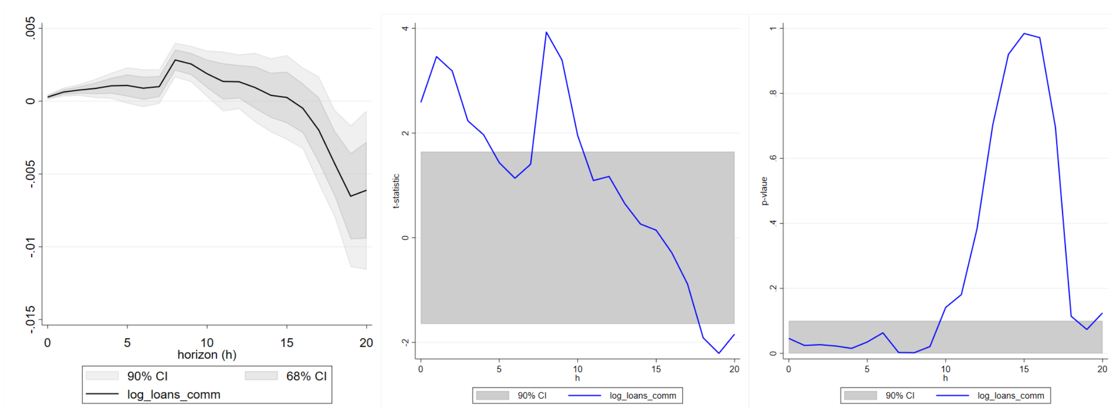


Figure A.23: Commercial loans (in logs)

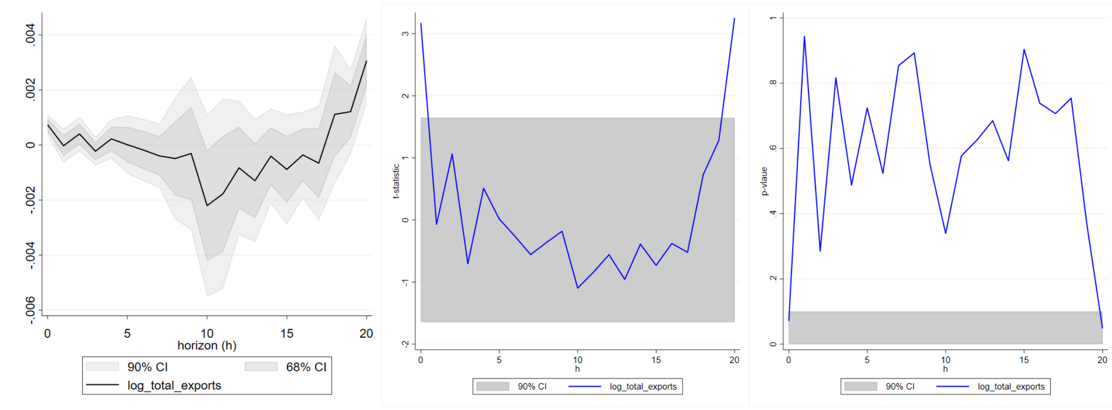


Figure A.24: Exports (in logs)

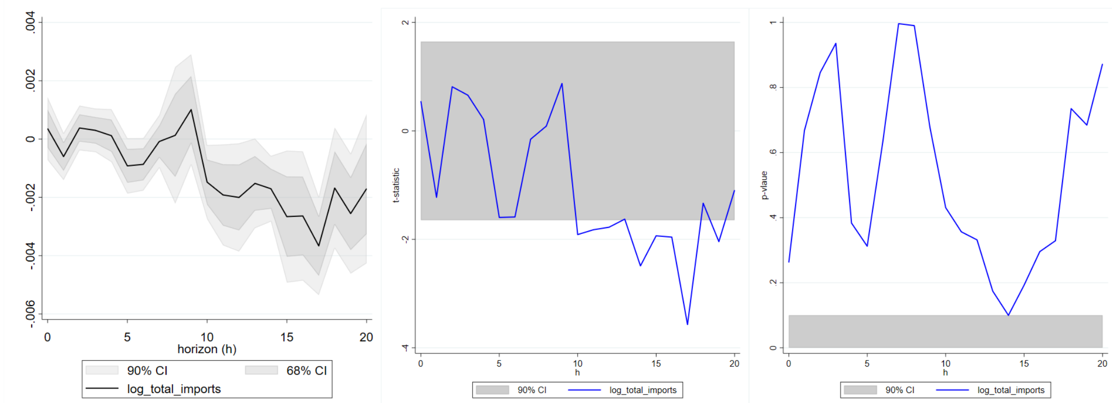
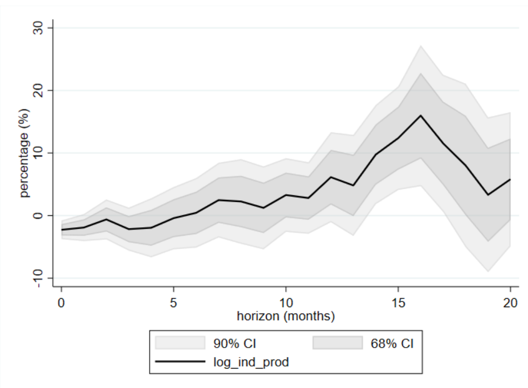


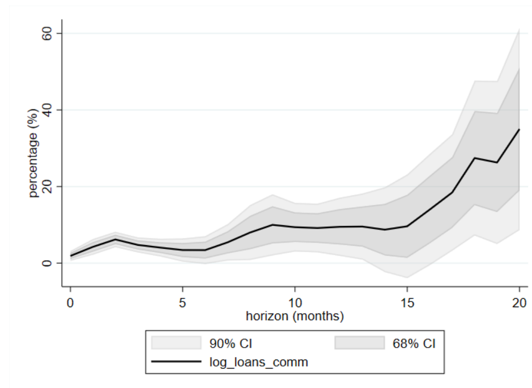
Figure A.25: Imports (in logs)

A.8 Plots from robustness checks

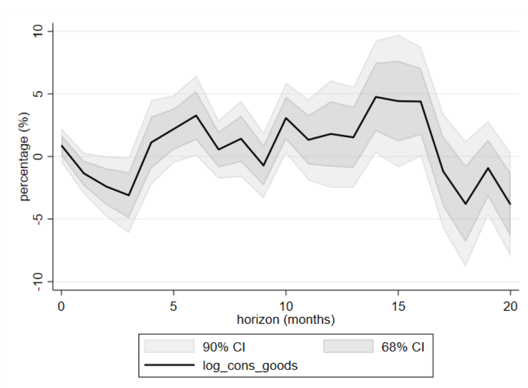
A.8.1 Different ways of constructing the baseline trade policy shock



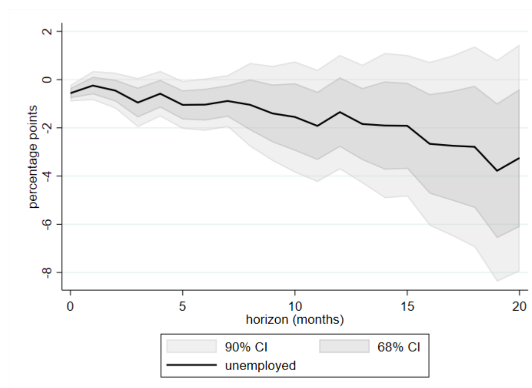
(a) Industrial production (in logs)



(b) Commercial loans (in logs)

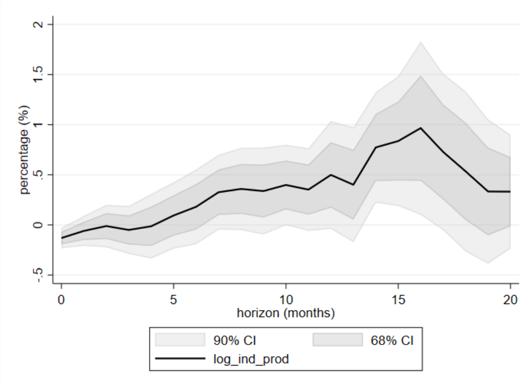


(c) Consumption of goods (in logs)

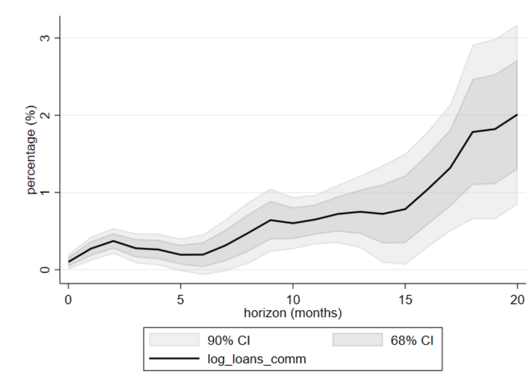


(d) Unemployment rate

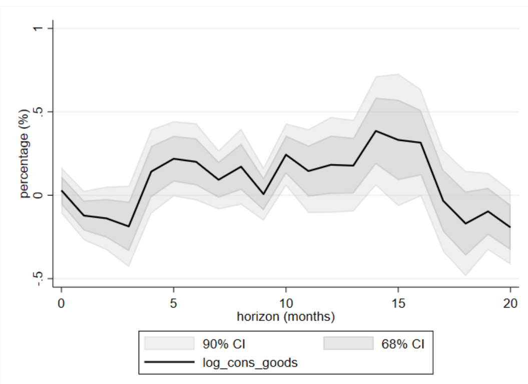
Figure A.26: IRFs from trade policy shocks for which daily shocks are averaged over the month



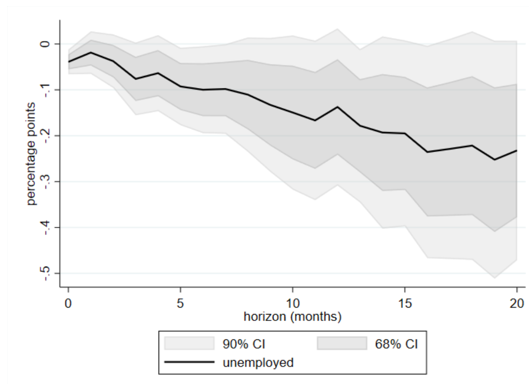
(a) Industrial production (in logs)



(b) Commercial loans (in logs)

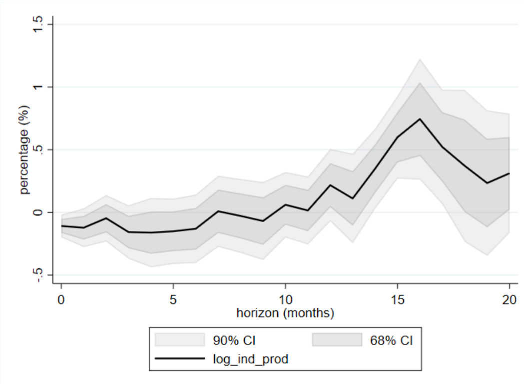


(c) Consumption of goods (in logs)

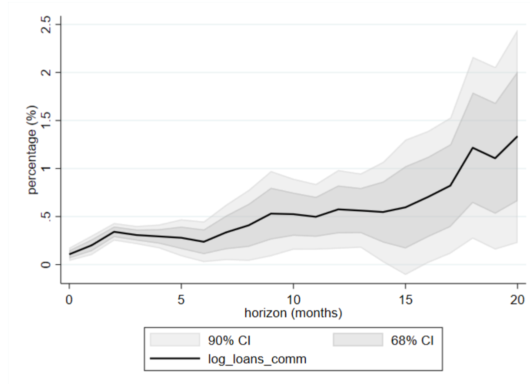


(d) Unemployment rate

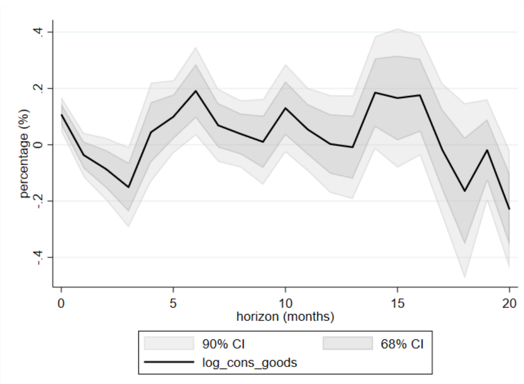
Figure A.27: IRFs from trade policy shocks for which only the largest daily shock (in absolute terms) is chosen if there are several during the same month



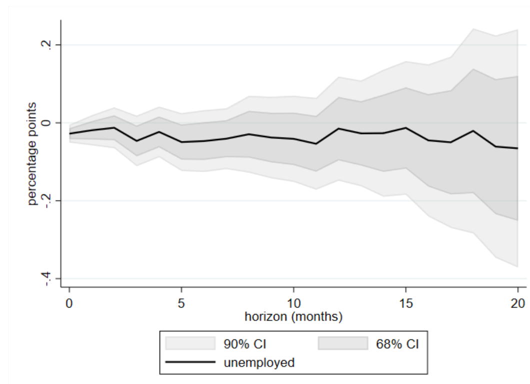
(a) Industrial production (in logs)



(b) Commercial loans (in logs)

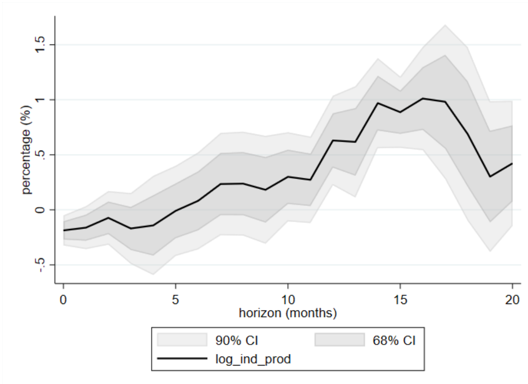


(c) Consumption of goods (in logs)

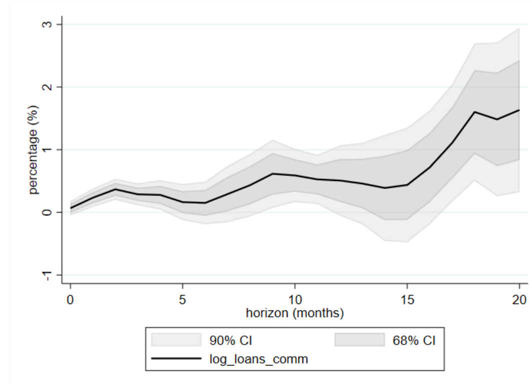


(d) Unemployment rate

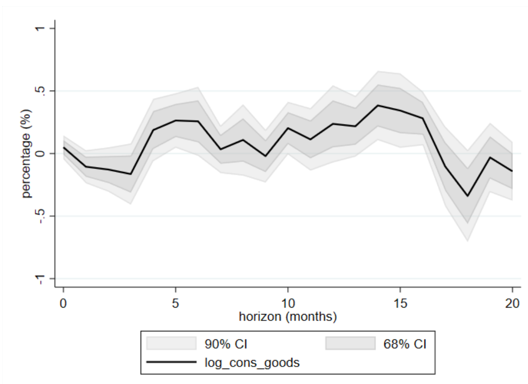
Figure A.28: IRFs from only using "correctly" identified trade policy shocks based on ex-ante classification of statements into liberalizing and protectionist



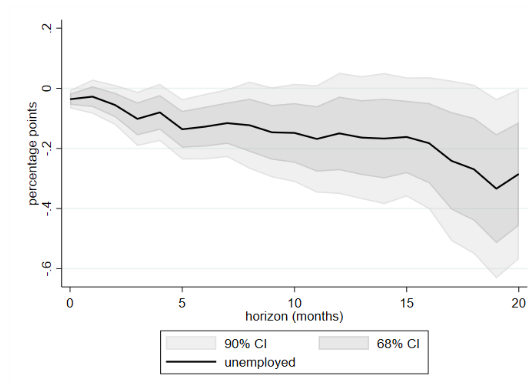
(a) Industrial production (in logs)



(b) Commercial loans (in logs)

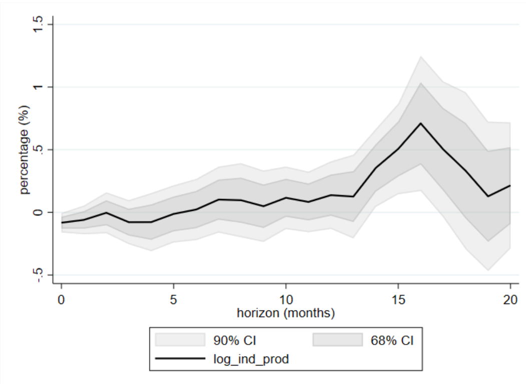


(c) Consumption of goods (in logs)

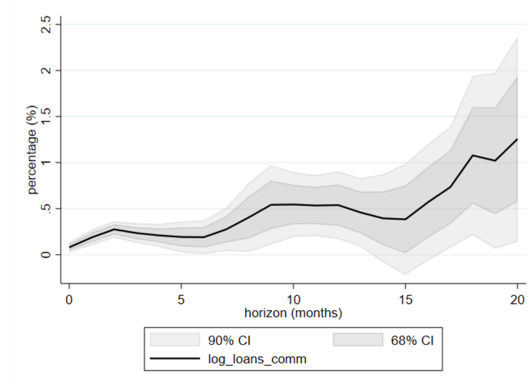


(d) Unemployment rate

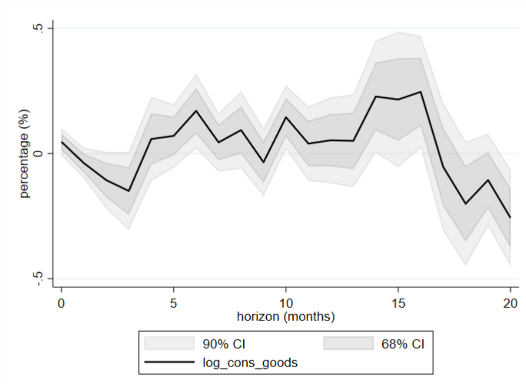
Figure A.29: IRFs from building shock based on excess returns on the exporters' basket compared to the S&P500



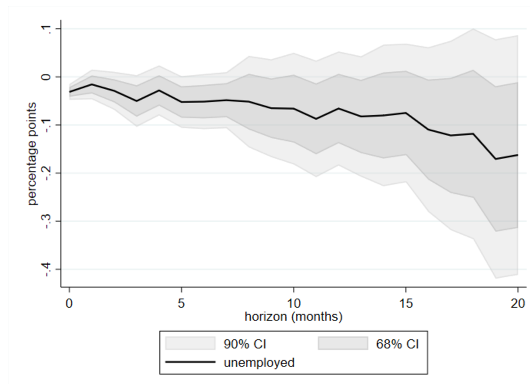
(a) Industrial production (in logs)



(b) Commercial loans (in logs)

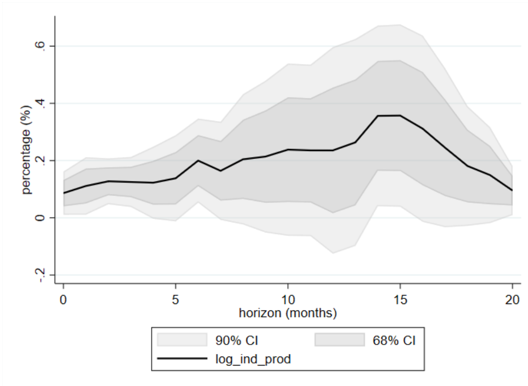


(c) Consumption of goods (in logs)

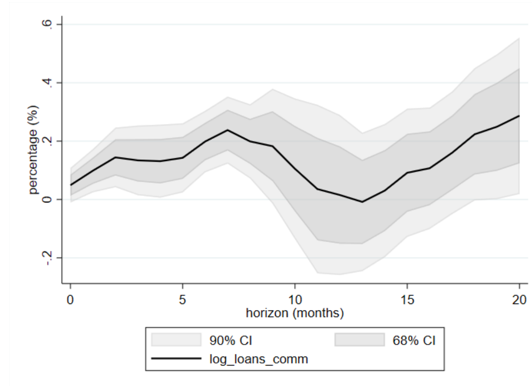


(d) Unemployment rate

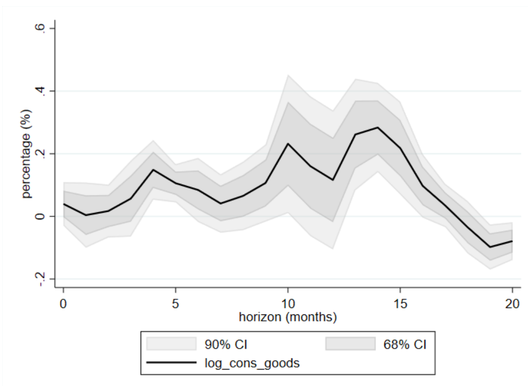
Figure A.30: IRFs using the trade policy shock that is “cleaned” of other macroeconomic news



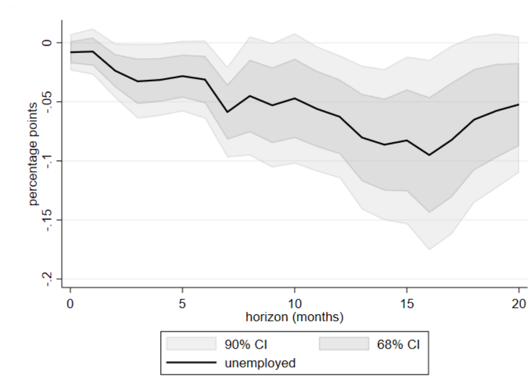
(a) Industrial production (in logs)



(b) Commercial loans (in logs)



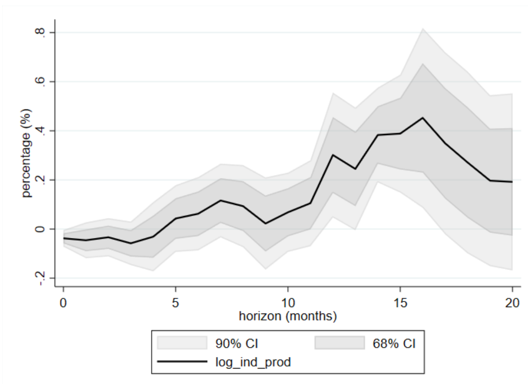
(c) Consumption of goods (in logs)



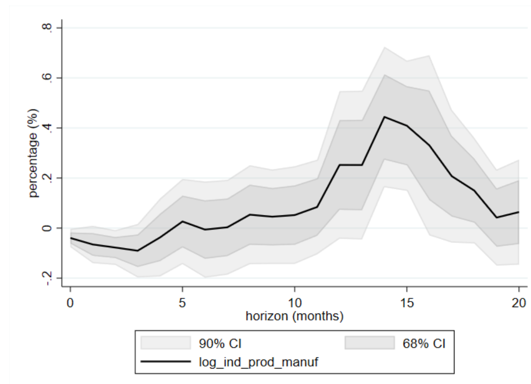
(d) Unemployment rate

Figure A.31: IRFs using large movements in the ratio of exporters to non-exporters independently of whether a trade policy statement has been issued

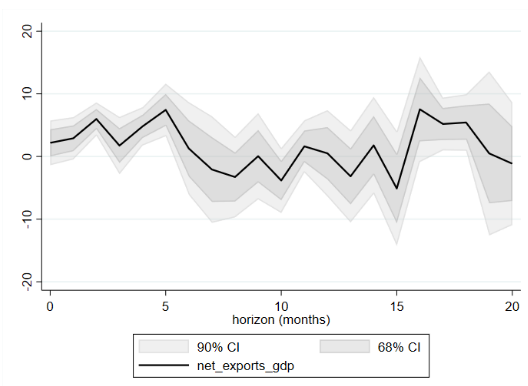
A.8.2 Building shocks based on importers' stock price index



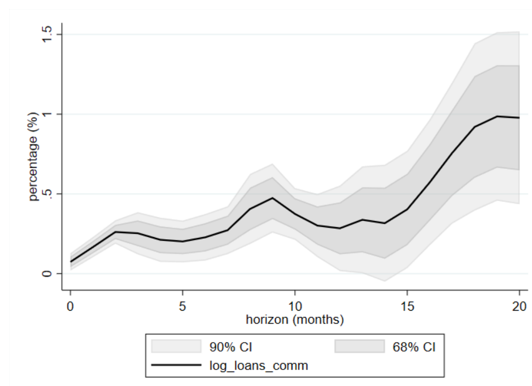
(a) Industrial production (in logs)



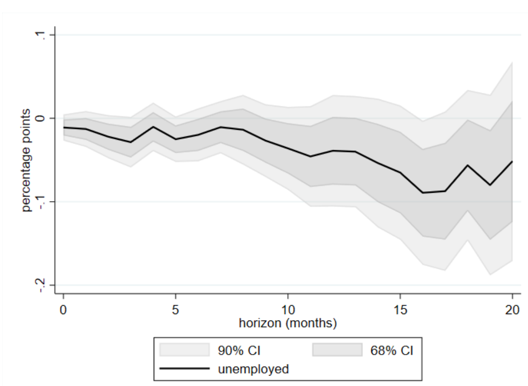
(b) Ind. prod.: Manufacturing (in logs)



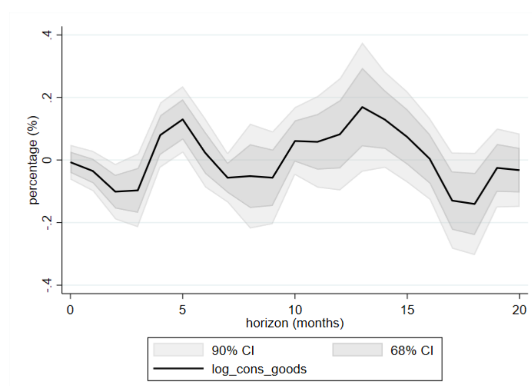
(c) Net exports to GDP



(d) Commercial loans (in logs)



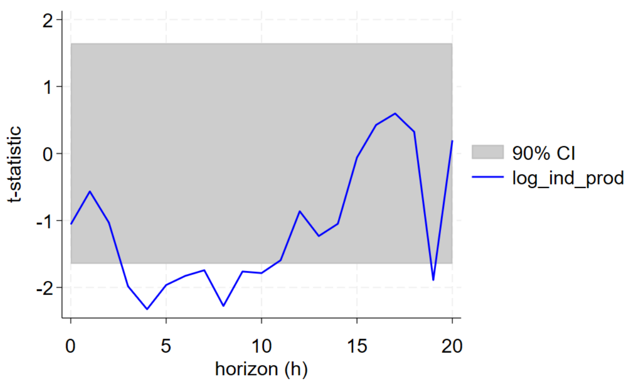
(e) Unemployment rate



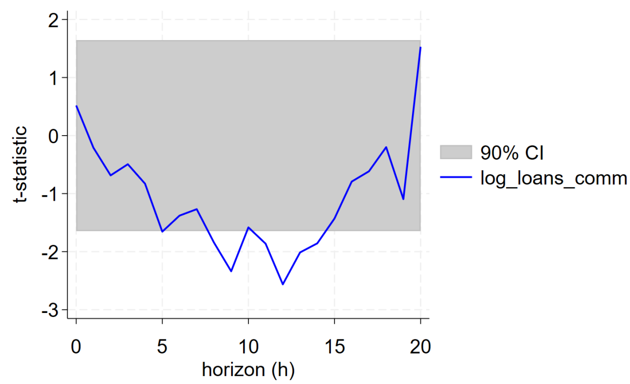
(f) Consumption of goods (in logs)

Figure A.32: IRFs from trade policy shocks based on importers' stock price index

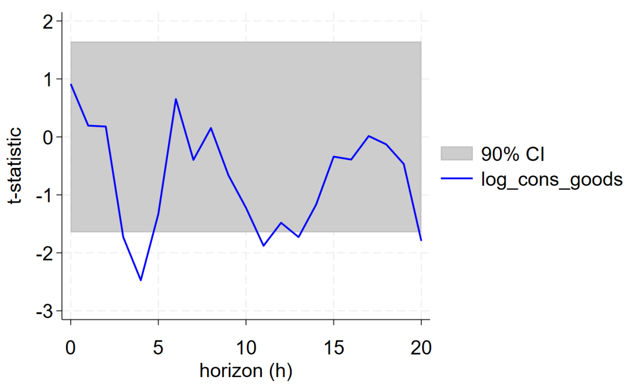
A.8.3 Including interaction dummy capturing differential effects under different presidents



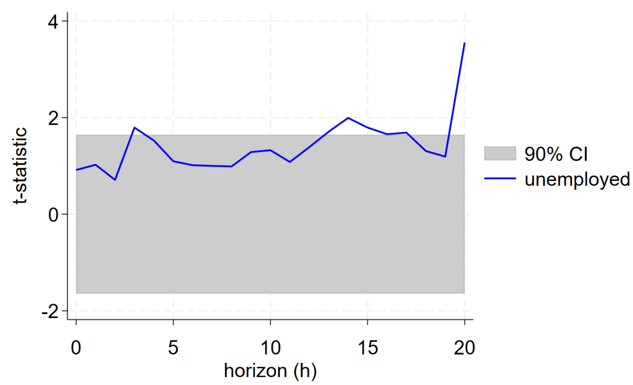
(a) Industrial production (in logs)



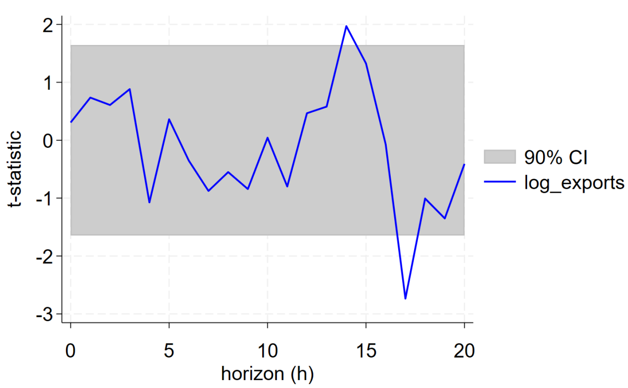
(b) Commercial loans (in logs)



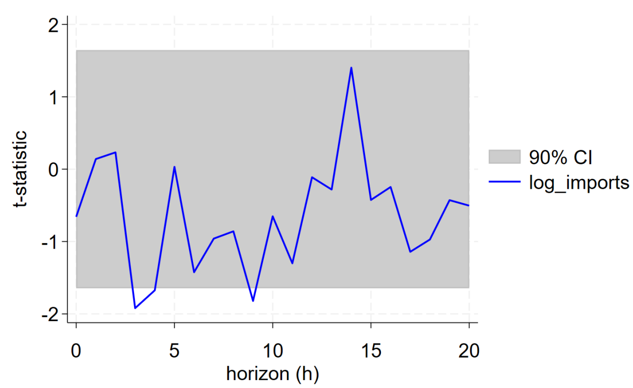
(c) Consumption of goods (in logs)



(d) Unemployment rate



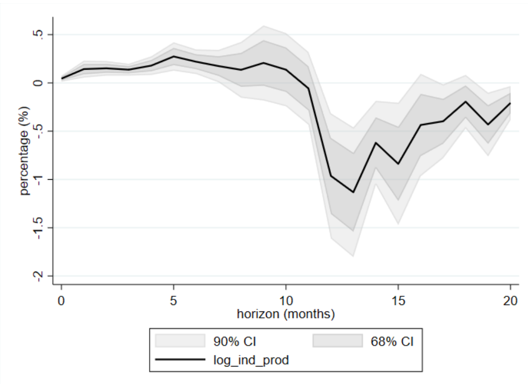
(e) Exports (in logs)



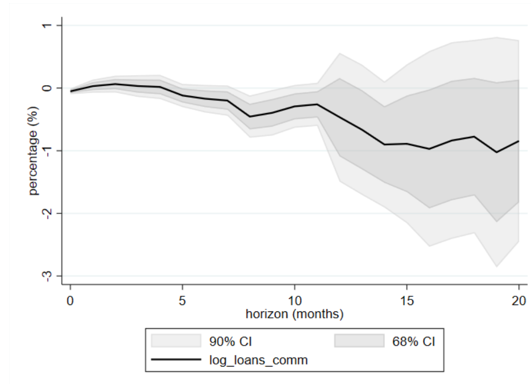
(f) Imports (in logs)

Figure A.33: t-statistics from null hypothesis that the interaction term (between dummy for President Trump years in office and baseline trade policy shock) is not statistically significant.

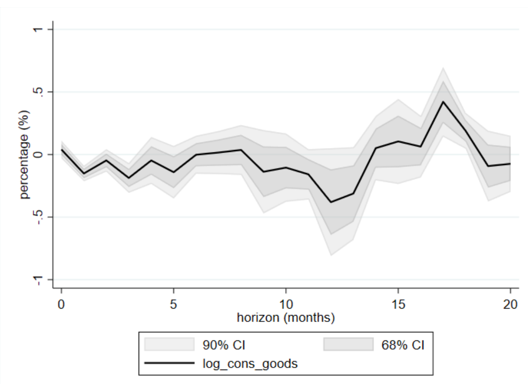
A.8.4 Using shocks based on President Trump's tweets on trade instead of official statements



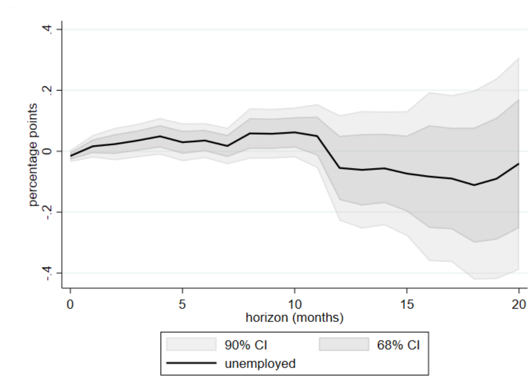
(a) Industrial production (in logs)



(b) Commercial loans (in logs)



(c) Consumption of goods (in logs)



(d) Unemployment rate

Figure A.34: IRFs using shocks based on President Trump's tweets (agnostic approach regarding shock sign)

A.8.5 Stock market effects of official statements versus tweets

During Donald Trump's presidency, official trade policy communication channels have become complemented by messages released via the social media platform Twitter. Comparing the content transmitted via both channels reveals the lack of specificity, simple language and larger noise component of tweets. The question that arises is whether stock returns reflect these differences.

We include official statements and trade-related tweets published by Donald J. Trump simultaneously in our definition. Since some of the president's tweets simply mention and comment on foreign trade actions, we use all official statements, i.e., those issued by the US and its trade partners, for comparability. Unlike in the previous sections in which we use data from 2007 onward, the sample now starts in 2017 when President Trump took office.

Estimates of the effect of official policy statements are qualitatively in line with previous sections. Strikingly, once we control for official statements, tweets have no significant effect on any of the dependent stock market variables.

Overall, there is some evidence that investors continue to respond to news obtained from government agencies despite the rise of novel communication channels. Exporters' unresponsive stock returns reflect that tweets do not provide additional information.

Appendix B

Appendix Chapter 2

B.1 Housing data

The data on house prices includes all houses and apartments offered for sale or rent on the Immobilienscout24 website on a monthly basis for the period spanning January 2007 up to December 2021. Properties are separated into four main categories, namely houses for sale, apartments for sale, houses for rent, and apartments for rent. Table B.1 displays the variables included in the dataset.

Multiple entries

The original dataset does not exclude multiple entries for the same item. In case of multiple entries within a six-month period, only the last price is used, and all previous listings are dropped. Spells for the same property that are at least six months apart are treated as different postings.

Data cleaning

The sample excludes all postings with a zero sale or rental price. Moreover, very luxurious apartments or houses are excluded from the analysis. Specifically, properties are dropped from the sample if they (i) have additional utility costs higher than 5 thousand euros per month or (ii) have a real estate sale value of more than 6 million euros or (iii) cost more than 15 thousand euros per square meter. Flats with more than 8 rooms and houses with more than 15 rooms are also omitted. The living area is restricted between 15 and 400 square meters for apartments and

15 to 600 square meters for houses. Furthermore, all entries with a sale price lower than 10 000 euros and those with a living area of less than 25 square meters are excluded from the sample. Properties that have no information regarding key property characteristics, such as the number of rooms or location, are dropped from the sample. Furthermore, very old properties (dated before 1700) are excluded.

Phantom and scam postings

A common issue in online property listings is fraudulent phishing postings. Usually, they look like legitimate ad postings, often at lower than the market price to attract potential customers. While Immobilienscout24 has developed a sophisticated algorithm to detect and remove those postings, the data could still be contaminated by phishing postings. Therefore, the sample excludes viral postings that receive disproportionate attention. In particular, ads with clicks beyond the 99% range for each of the four types are omitted. Finally, postings that stay longer than 1000 days on the website are also dropped.

Table B.1: ImmobilienScout24 list of variables

Category	**Variables**	
Identifier	Unique object ID	
Time period	Beginning of ad (year)	
	Beginning of ad (month)	
	End of ad (year)	
	End of ad (month)	
Object features	Elevator in object	Number of rooms
	Facilities of object	Number of floors
	Number of bathrooms	Construction phase
	Balcony at object	Assisted living for elderly
	Protected historic building	Granny flat in object
	Kitchenette in object	Public housing
	Floor in which object is lcoated	Type of real estate
	Usable as holiday home	Rented when sold
	Available from	Rental income per month in euros
	Guest toilet in object	Number of aniclary rooms
	(Shared) garden available	Accessible, no steps
	Pets allowed	Number of bedrooms
	House type	Living area
	Flat type	Plot area
	Cellar in object	Usable floor space
	Common charge for community association (in euros/month)	Garage/parking space available
Energy structure information	Year that object was built	Heating costs
	Type of energy performance certificates	Type of heating
	Energy efficiency rating	Year of last modernisation of object
	Energy consumption per year/sq.m.	Condition of object
	Warm water consumption included in energy consumption	
Price information	Brokerage at contract conclusion	Inclusive rent in euros
	Heating costs covered by inclusive rent	Utilities in euros
	Purchasing price in euros	Price of parking space in euros
	Security deposit	
Regional information	German state	Municipality Identifier (AGS, 2015)
	$1km^2$ raster cell following INSPIRE	Postcode of address
	Local labour market (Kosfeld and Werner, 2012)	District identifier (AGS, 2015)
Meta-information of ad	Number of clicks on customer profile	Number of hits of ad
	Number of clicks on contact button	Days of availability of ad
	Number of clicks on customer URL	Date of data retrieval
	Number of clicks on share button	Spell counter within object identifier
	Number of hits of ad	

Appendix C

Appendix Chapter 3

C.1 Alternative Samples

Table C.1: MPC - Pooled stage 1 - PSID Extended data (Taxsim32; year<2008)

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-Htm	Mortgagor - W-Htm	P-Htm	W-Htm	N-Htm
Baseline	-0.0482	0.127***	-0.00819	0.163***	0.174***	0.121*	-0.0144	0.0649	0.0890**
	(0.0718)	(0.0370)	(0.0920)	(0.0407)	(0.0479)	(0.0723)	(0.108)	(0.0650)	(0.0411)
Inc. empl. status	0.0477	0.193***	0.0758	0.221***	0.227***	0.184***	0.0861	0.127**	0.158***
	(0.0620)	(0.0356)	(0.0927)	(0.0385)	(0.0463)	(0.0659)	(0.0923)	(0.0605)	(0.0387)
Observations	1646	4579	723	3856	2446	1350	834	1816	3575

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: MPC - Pooled stage 1 - PSID Extended data (Taxsim32; year $\geq$ 2008)

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-Htm	Mortgagor - W-Htm	P-Htm	W-Htm	N-Htm
Baseline	-0.0218	0.0854**	0.0773	0.0870**	0.0775	0.0933	-0.0572	-0.000330	0.0763**
	(0.0549)	(0.0399)	(0.0991)	(0.0440)	(0.0540)	(0.0776)	(0.0973)	(0.0669)	(0.0388)
Inc. empl. status	0.111**	0.179***	0.106	0.196***	0.224***	0.129**	0.0539	0.0795	0.202***
	(0.0431)	(0.0360)	(0.102)	(0.0384)	(0.0483)	(0.0642)	(0.0720)	(0.0595)	(0.0335)
Observations	3014	4941	636	4305	3046	1099	1427	1549	4979

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: MPC - Pooled stage 1 - PSID Extended data (Taxsim32; years 2008-2012)

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-Htm	Mortgagor - W-Htm	P-Htm	W-Htm	N-Htm
Baseline	0.0932	0.204***	0.362**	0.166**	0.0983	0.243	0.185	0.0957	0.169**
	(0.109)	(0.0594)	(0.146)	(0.0656)	(0.0742)	(0.156)	(0.174)	(0.104)	(0.0671)
Inc. empl. status	0.119	0.234***	0.354**	0.210***	0.183***	0.181*	0.200	0.107	0.213***
	(0.101)	(0.0530)	(0.155)	(0.0562)	(0.0676)	(0.110)	(0.141)	(0.0903)	(0.0648)
Observations	713	1319	172	1147	772	299	381	433	1218

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: MPC - Pooled stage 1 - PSID Extended data (Taxsim32; year>2012)

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-Htm	Mortgagor - W-Htm	P-Htm	W-Htm	N-Htm
Baseline	0.130	0.124	0.0130	0.163	0.152	0.204	0.110	0.218*	0.110
	(0.100)	(0.0965)	(0.233)	(0.108)	(0.140)	(0.152)	(0.196)	(0.127)	(0.0836)
Excl. empl. status	0.0345	0.00724	-0.0608	0.0363	-0.0533	0.233	0.0601	0.175	-0.0386
	(0.135)	(0.0993)	(0.221)	(0.114)	(0.148)	(0.172)	(0.236)	(0.145)	(0.102)
Observations	779	1180	151	1029	774	242	306	338	1315

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.2 Transition probabilities

Table C.5: Transition matrix of homeownership status

t-1 \ *t*	Renter	Mortgagor	Outright owner
Renter	0.85	0.11	0.05
Mortgagor	0.06	0.86	0.08
Outright owner	0.09	0.14	0.77

Table C.6: Transition matrix of HtM status

t-1 \ *t*	Poor-HtM	Wealthy-HtM	Non-HtM
Poor-HtM	0.62	0.12	0.26
Wealthy-HtM	0.10	0.51	0.39
Non-HtM	0.08	0.15	0.77

Table C.7: Transition matrix of employment status

t-1 \ *t*	Employed[1]	Unemployed[2]	Retired	Not in labour force
Employed[1]	0.91	0.04	0.03	0.02
Unemployed[2]	0.62	0.23	0.03	0.12
Retired	0.05	0	0.89	0.05
Not in labour force	0.20	0.06	0.15	0.59

Notes: 1 includes those temporarily out of employment, 2 refers to involuntary unemployment

C.3 Testing coefficient equality

To test whether the difference between the MPC estimates of different groups is statistically significant, we repeat the estimation in a pooled regression framework adding group dummies that indicate whether the household is a mortgagor, outright owner or renter. The MPC estimates (and standard errors) are identical to those presented in the main analysis above. The pooled regression analysis allows us to test coefficient equality. We test the null hypothesis that the MPC estimate of group i (where i = mortgagor, outright owner or renter) is equal to the MPC estimate of group j (where j = mortgagor, outright owner or renter and $j \neq i$).

Below we present the point estimates, standard errors and p-values for the three null hypotheses (*mortgagor = outright owner, mortgagor = renter, renter = outright owner*), for all the specifications presented in our main analysis. $p < 0.05$ means that the MPC estimates of the two groups are statistically significantly different at the 5% confidence level.

Table C.8: Null hypothesis: Mortgagor = Outright Owner

	estimate	*standard error*	*p-value*
Table 1 (Transitory shock)			
Baseline	0.036	0.077	0.635
Inc. empl. status	0.038	0.063	0.543
Exogenous	0.007	0.084	0.936
Table 4 (KVW sample, TAXSIM9)			
Baseline	0.087	0.096	0.364
Inc. empl. status	0.057	0.097	0.557
Exogenous	0.064	0.117	0.584
Table 6 (Non-switchers)			
Baseline	0.012	0.161	0.939
Inc. empl. status	0.023	0.160	0.885
Table 7 (Persistent shocks)			
Transitory			
$\rho = 0.93$	0.029	0.075	0.694
$\rho = 0.95$	0.031	0.076	0.678
$\rho = 0.97$	0.033	0.073	0.646
$\rho = 0.99$	0.035	0.078	0.648
Persistent			
$\rho = 0.93$	0.070	0.117	0.549
$\rho = 0.95$	0.068	0.115	0.552
$\rho = 0.97$	0.066	0.118	0.577
$\rho = 0.99$	0.062	0.126	0.622

Table C.9: Null hypothesis: Mortgagor = Renter

	estimate	*standard error*	*p-value*
Table 1 (Transitory shock)			
Baseline	0.092	0.046	0.045
Inc. empl. status	0.070	0.048	0.142
Exogenous	0.16	0.062	0.011
Table 2 (KVW sample, TAXSIM9)			
Baseline	0.005	0.076	0.953
Inc. empl. status	0.058	0.068	0.394
Exogenous	0.063	0.091	0.492
Table 3 (Non-switchers)			
Baseline	0.053	0.080	0.506
Inc. empl. status	0.073	0.071	0.304
Table 4 (Persistent shocks)			
Transitory			
$\rho = 0.93$	0.102	0.059	0.083
$\rho = 0.95$	0.099	0.054	0.070
$\rho = 0.97$	0.096	0.052	0.068
$\rho = 0.99$	0.093	0.049	0.058
Persistent			
$\rho = 0.93$	-0.248	0.073	0.001
$\rho = 0.95$	-0.260	0.075	0.001
$\rho = 0.97$	-0.271	0.077	0.000
$\rho = 0.99$	-0.280	0.080	0.000

Table C.10: Null hypothesis: Renter = Outright owner

	estimate	*standard error*	*p-value*
Table 1 (Transitory shock)			
Baseline	0.056	0.079	0.479
Inc. empl. status	0.031	0.064	0.625
Exogenous	0.153	0.097	0.114
Table 2 (KVW sample, TAXSIM9)			
Baseline	-0.083	0.113	0.462
Inc. empl. status	0.001	0.102	0.99
Exogenous	-0.002	0.139	0.991
Table 3 (Non-switchers)			
Baseline	0.041	0.165	0.806
Inc. empl. status	0.050	0.159	0.754
Table 4 (Persistent shocks)			
Transitory			
$\rho = 0.93$	0.073	0.080	0.361
$\rho = 0.95$	0.067	0.083	0.419
$\rho = 0.97$	0.062	0.082	0.447
$\rho = 0.99$	0.058	0.079	0.465
Persistent			
$\rho = 0.93$	-0.318	0.127	0.012
$\rho = 0.95$	-0.328	0.122	0.007
$\rho = 0.97$	-0.336	0.126	0.008
$\rho = 0.99$	-0.342	0.136	0.012

Table C.11: Testing statistical difference between coefficients using other instruments

	estimate	*standard error*	*p-value*
$\boldsymbol{\Delta ln(y_{i,t+2})}$			
Mortgagor=Renter	0.051	0.049	0.298
Outright owner=Renter	0.013	0.077	0.863
Outright owner=Mortgagor	0.037	0.078	0.631
$\boldsymbol{\Delta ln(y_{i,t+3})}$			
Mortgagor=Renter	0.085	0.058	0.145
Outright owner=Renter	0.150	0.087	0.083
Outright owner=Mortgagor	-0.066	0.087	0.451
$\boldsymbol{\Delta ln(y_{i,t+4})}$			
Mortgagor=Renter	0.012	0.069	0.860
Outright owner=Renter	0.019	0.105	0.860
Outright owner=Mortgagor	-0.006	0.105	0.952

C.4 Serial Correlation of Income Shocks

Table C.12: Persistence parameter

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-Htm	Mortgagor - W-Htm	P-HtM	W-HtM	N-HtM
Baseline	0.931***	0.928***	0.946***	0.916***	0.919***	0.885***	0.907***	0.909***	0.929***
	(0.0139)	(0.00771)	(0.0207)	(0.00846)	(0.0102)	(0.0167)	(0.0207)	(0.0145)	(0.00830)
Inc. empl. status	0.946***	0.927***	0.930***	0.920***	0.929***	0.875***	0.927***	0.890***	0.937***
	(0.0172)	(0.00846)	(0.0238)	(0.00915)	(0.0108)	(0.0188)	(0.0281)	(0.0165)	(0.00907)
Excl. switchers	0.926***	0.925***	0.876***	0.929***	0.929***	0.897***	0.889***	0.908***	0.934***
	(0.0252)	(0.0144)	(0.0558)	(0.0152)	(0.0178)	(0.0325)	(0.0405)	(0.0301)	(0.0144)
Excl. empl. status & switchers	0.907***	0.921***	0.916***	0.917***	0.916***	0.892***	0.866***	0.905***	0.927***
	(0.0190)	(0.0135)	(0.0530)	(0.0142)	(0.0171)	(0.0287)	(0.0285)	(0.0258)	(0.0131)
Observations	5497	12832	1947	10885	7349	3225	2734	4397	11198

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.13: MPC - Persistent Income shocks

	Renter	Homeowner	Outright owner	Mortgagor	Mortgagor - N-HtM	Mortgagor - W-HtM	P-HtM	W-HtM	N-HtM
Transitory component									
$\rho = 0.93$	0.011	0.106***	0.084	0.113***	0.111***	0.136***	-0.031	0.0971**	0.087***
	(0.0422)	(0.0261)	(0.0644)	(0.0286)	(0.0338)	(0.0522)	(0.0689)	(0.0455)	(0.0267)
$\rho = 0.95$	0.017	0.109***	0.084	0.116***	0.113***	0.139***	-0.024	0.103**	0.089***
	(0.0407)	(0.0252)	(0.0625)	(0.0277)	(0.0326)	(0.0508)	(0.0666)	(0.0441)	(0.0259)
$\rho = 0.97$	0.023	0.111***	0.085	0.118***	0.115***	0.143***	-0.018	0.109**	0.092***
	(0.0395)	(0.0246)	(0.0611)	(0.0269)	(0.0317)	(0.0496)	(0.0647)	(0.0430)	(0.0252)
$\rho = 0.99$	0.029	0.114***	0.086	0.122***	0.117***	0.146***	-0.012	0.115***	0.094***
	(0.0385)	(0.0240)	(0.0600)	(0.0264)	(0.0310)	(0.0487)	(0.0632)	(0.0420)	(0.0246)
Persistent component									
$\rho = 0.93$	0.428***	0.169***	0.110	0.180***	0.180***	0.129*	0.498***	0.194***	0.223***
	(0.0556)	(0.0351)	(0.104)	(0.0372)	(0.0452)	(0.0700)	(0.0765)	(0.0607)	(0.0392)
$\rho = 0.95$	0.441***	0.171***	0.113	0.181***	0.187***	0.121*	0.517***	0.186***	0.228***
	(0.0572)	(0.0358)	(0.107)	(0.0379)	(0.0461)	(0.0709)	(0.0784)	(0.0617)	(0.0402)
$\rho = 0.97$	0.451***	0.170***	0.115	0.181***	0.191***	0.111	0.532***	0.177***	0.231***
	(0.0587)	(0.0363)	(0.109)	(0.0383)	(0.0467)	(0.0712)	(0.0799)	(0.0623)	(0.0409)
$\rho = 0.99$	0.458***	0.168***	0.116	0.178***	0.194***	0.1000	0.543***	0.166***	0.230***
	(0.0598)	(0.0365)	(0.109)	(0.0385)	(0.0470)	(0.0712)	(0.0811)	(0.0626)	(0.0412)
Observations	5876	12450	1765	10685	2955	4591	10780		

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.14: Estimated change in consumption following a temporary 25bp cut in monetary policy, based on estimated MPCs from Persistent Shocks

	Mortgagor	Outright owner	Renter
Estimated change in income	757.3	585.3	439.3
Estimated change in non-durable consumption	305.8	-72.3	223.3
Transitory component			
MPC ($\rho = 0.93$)	0.113	0.084	0.011
Change in consumption	85.6	49.2	4.8
MPC ($\rho = 0.95$)	0.116	0.084	0.017
Change in consumption	87.8	49.2	7.5
MPC ($\rho = 0.97$)	0.118	0.085	0.023
Change in consumption	89.4	49.8	10.1
MPC ($\rho = 0.99$)	0.122	0.086	0.029
Change in consumption	92.4	50.3	12.7
Persistent component			
MPC ($\rho = 0.93$)	0.180	0.11	0.428
Change in consumption	136.3	64.4	188.0
MPC ($\rho = 0.95$)	0.181	0.113	0.441
Change in consumption	137.1	66.1	193.7
MPC ($\rho = 0.97$)	0.181	0.115	0.451
Change in consumption	137.1	67.3	198.1
MPC ($\rho = 0.99$)	0.178	0.116	0.458
Change in consumption	134.8	67.9	201.2

The table reports the overall dollar change in income/expenditure over the four-year period following a temporary 25bp cut in monetary policy. The first two rows refer to the estimates reported in Table 1 of Cloyne et al. (2020).