INSTITUT DE HAUTES
ÉTUDES INTERNATIONALES
ET DU DÉVELOPPEMENT
GRADUATE INSTITUTE
OF INTERNATIONAL AND
DEVELOPMENT STUDIES

Essays on New Trends in Financial Intermediation

THESIS
submitted at the Graduate Institute
in fulfilment of the requirements of the
PhD degree in International Economics

by

Edoardo Chiarotti

Thesis N° 1412

Geneva

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Sur le préavis de M. Cédric TILLE, professeur à l'Institut et directeur de thèse, de M. Ugo PANIZZA, professeur à l'Institut et membre interne du jury, et de Mr Arnaud MEHL, Adviser, International Policy Analysis, European Central Bank, Germany, et expert extérieur, la directrice de l'Institut de hautes études internationales et du développement autorise l'impression de la présente thèse sans exprimer par là d'opinion sur son contenu.

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Marie-Laure Salles
Directrice

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RESUME / ABSTRACT

Titre de la thèse / Title of thesis : Essays on New Trends in Financial Intermediation

Résumé en français: Cette thèse examine les grandes tendances qui ont caractérisé le secteur financier au cours des dernières années. Elle se compose de quatre chapitres, qui explorent l'impact des innovations technologiques et des nouvelles politiques des banques centrales, tant dans les économies développées qu'émergentes. Le premier chapitre traite de la manière dont l'avènement d'Internet a permis aux institutions financières d'atteindre plus facilement leurs clients à l'étranger. En estimant un modèle de gravité reposant sur des données concernant les flux financiers internationaux et les câbles Internet sous-marins sur la période 1990-2019, il montre que l'Internet a permis aux banques d'augmenter le montant des prêts et des dépôts auprès des clients étrangers. Le deuxième chapitre se concentre sur les États-Unis et montre que la politique monétaire de la Réserve fédérale a eu d'importants effets de stimulation sur les économies des États américains à la suite de la crise financière mondiale. Le troisième chapitre propose une analyse internationale des nouvelles règles pour les secteurs bancaires introduites par le paquet réglementaire appelé Basel III. Grâce à une difference-in-difference analyse sur les données relatives aux fonds propres réglementaires et aux dividendes sur la période 2010-2019, il montre que les restrictions automatiques sur les distributions de dividendes ont conduit les banques à augmenter leurs fonds propres réglementaires. Enfin, le chapitre 4 propose une analyse sur l'Inde et aborde la politique de démonétisation de la Reserve Bank of India en 2016, et l'effet qu'elle a eu sur les conflits locaux. En utilisant une configuration de régression difference-in-difference généralisée sur des données quotidiennes sur la période 2014-2018, il suggère que les districts avec une pénurie d'argent liquide plus sévère ont enregistré des événements relativement moins violents.

English Summary: This thesis examines some of the major trends that characterised the financial sector in recent years. It does so with four chapters, which explore the impact of technological innovations and new central-bank policies, in both developed and emerging economies. The first chapter addresses how the advent of the internet made it easier for financial institutions to reach clients abroad. By estimating a gravity model with data on international financial flows and submarine internet cables over 1990-2019, it shows that the internet allowed banks to increase the amount of loans and deposits to foreign clients. The second chapter focuses on the United States (US) and shows that the Federal Reserve's monetary policy had large stimulus effects on the economies of US states in the aftermath of the Global Financial Crisis. The third chapter proposes an international analysis of the new rules for the banking sectors introduced by the regulatory package called Basel III. With a difference-in-difference analysis on data on regulatory capital and dividends over 2010-2019, it shows that automatic restrictions on dividend distributions led banks to increase their regulatory capital. Finally, chapter 4 proposes and analysis on India and addresses the policy of demonetization by the Reserve Bank of India in 2016, and the effect that it had on local conflicts. By using a generalised difference-in-differences regression setup on daily data over 2014-2018, it finds that districts with a more severe cash shortage registered relatively less violent events.

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As doctoral students - and in general in academia - we spend a great deal of time in thinking about what is coming next. The next paper, the next grant, the next internship, and the next (first) job. As we are constantly planning ahead, sometimes we can lose the sense of the present, let alone the past. Me for one, I am having a hard time remembering when, in the past 5 years, I took a moment to sit back and think about all that has happened in my doctoral journey. I am really glad that I get the chance of doing it now, before the journey ends, and being thankful to all the people that were by my side and supported me throughout the way.

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Edoardo Chiarotti
Geneva, 31 July 2021.

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Introduction

"*Financial intermediation is a pervasive feature of all of the world's economies*" (Gorton and Winton, 2003). The role of banks is key for economic growth as they provide credit to firms and individuals and allow swift and safe payments for goods and services. In addition, financial intermediaries are a fundamental mean of transmission for monetary policy, as, following shocks in bank reserves and interest rates, they adjust the lending supply to the real economy (Kashyap et al., 1997).

The role and volume of financial intermediation changed significantly over time (Philippon, 2015). In the last ten years alone, the banking sector has undergone major structural transformations that profoundly affected the way banks operate. For example, following the regulatory reforms, banks have enhanced their balance sheets and moved away from both complex and cross-border activities. At the same time, the industry's return on equity has declined, as well as market sentiment (BIS, 2018).

I contribute to this recent literature by proposing an empirical analysis assessing how both technological and policy changes have affected the role of financial intermediaries in recent years, both locally and internationally. I do so through the lenses of three major questions. (i) Did new internet technologies change the way banks go abroad? (ii) Are there major differences in the way banks and the economy react to monetary and prudential policies in the aftermath of the financial crisis? (iii) Can central banks' policies that limit the amount of cash in circulation curb the illegal activities of organized groups?

I address these questions with 4 empirical chapters, which consider banking sectors both across and within countries, and in both developed and emerging economies. In the remaining of this introduction I will briefly summarise each chapter and how they contribute to describing new trends in financial intermediation around the world, with an emphasis on policy implications.

The first chapter, *Banks, Foreign Affiliates and the Internet*, is co-authored with Stela Rubínová and addresses the role of the internet in enabling banks cross-border activities. After the liberalizations of 1980s, banks have been relying on affiliates located in other countries to run their business there (Cerutti et al., 2007). However, in recent years there has been an increase in cross-border lending, especially by banks in Japan, US and Canada

(McCauley et al., 2019). We aim to test whether the advent of internet technologies, like internet banking, played a role in shaping this trend. We do so by building a novel dataset that combines data on submarine cables and banks' cross-border positions. Essentially, these cross-border positions are loan and deposit services that banks provide to clients located in foreign countries.

By using a gravity model on data over 2010-2019, we find that banks in home economies provided significantly more loans and deposits to clients in countries that share more internet connections with the home economy. By contrast, we do not find any significant effect of internet connections on foreign-affiliate activities. Furthermore, we find that having more connections contributes in decreasing the (still relevant) negative impact of distance on cross-border positions. Finally, we find that this internet channel is stronger when clients are in countries with smaller and unstable banking systems.

These findings suggest that investments in submarine cables may facilitate the access of local firms and individuals to credit supplied by foreign banks, and thus support economic growth. However, more cables imply an increase in market shares of banks that are outside the jurisdiction of national regulators, and may therefore require bi-lateral agreements to reduce risks related to excess borrowing.

In the second chapter, *The Local Impact of the Fed in the Aftermath of the Financial Crisis*, I address how new structural changes induced by the Great Financial Crisis of 2007-2009 may change the effectiveness of monetary policy in the United States (US). I do so by focusing on the impact of policies by the Federal Reserve (Fed) on the real economy of single US states. Among other things, the local effect of monetary policy depends on the characteristics of the local house market. For example, Beraja et al. (2019) show that quantitative easing had larger effects on car purchases in areas where property values were high and individuals could refinance their mortgage. I build on this literature by focusing on the more general impact of monetary policy on states' real output and unemployment and how this relationship changed after the financial crisis.

Specifically, I estimate these differences with a Bayesian Global Vector Autoregression (VAR) over two samples, namely 1990-2007 and 2010-2019. The model estimates that, after the crisis, in all states real output and unemployment reacted more to a monetary policy stimulus. In addition, the heterogeneity of states' responses also increased. Interestingly, real output in states with house markets that were most heavily affected by the crisis - like California, Nevada, Arizona and Florida - converges back to equilibrium much faster than before. In the last part of the paper, I explore whether increased differences in house prices can explain the post-crisis differences in output responses. I provide evidence in this sense with a set of cross-sectional regressions that control for state-level characteristics that are relevant for the transmission of monetary.

From a policy perspective, these results suggest that national fiscal policies that address regional inequalities should take house-price dynamics into account. Specifically, a monetary expansion can be complemented by a redistribution of resources from least to most negatively affected areas.

I continue the analysis of new banking policies and their impact on financial intermediaries with a third chapter, *Assessing Basel III: Automatic Distribution Restrictions, Regulatory Capital and Bank Lending*, co-authored with Aakriti Mathur and Aniruddha Rajan. In response to the last financial crisis, the Bank for International Settlements has put forward a new package of regulations, known as Basel III, aimed at increasing the level of capital banks must hold, expressed as a percentage of their risk weighted assets. In addition, banks that breach the new requirements are also subject to automatic restrictions on profit distributions. These automatic restrictions apply at all times and are aimed at refraining banks under stress from distribute profits and therefore avoid risk-shifting behaviours (Acharya et al., 2016).
While these restrictions have many advantages (Schroth, 2021), they could also bear some costs, as banks could hold excessive capital and cut lending to avoid incurring in such restrictions. We test these hypotheses with an empirical methodology that measures banks' concern about automatic restrictions on dividends. We do so by looking at data of past dividends from 2000 for a sample of 65 publicly listed banks across 24 countries. Intuitively, banks that generally pay stable dividends, i.e. smooth dividends, would be more concerned about these restrictions than banks that do not. With a simple difference-in-differences (DID) analysis, we find that dividend-smoothing banks had larger risk-weighted capital ratios after 2016, when dividend restrictions were introduced. We confirm this finding with a local-projection approach that exploits shocks in the threshold at which dividend restrictions apply. Finally, we do not find any such effect on lending, which is good news for policy makers. Possibly, as the policy was phased-in gradually, it allowed banks to build capital organically rather than deleveraging. However, the incentives to deleverage or derisk are likely to be higher during periods of stress, such as the Covid-19 pandemic, when risk-aversion, uncertainty, and market stigma are heightened.

The fourth and final chapter, *Hit them in the Wallet! An Analysis of the Indian Demonetization as a Counter-Insurgency Policy*, is co-authored with Nathalie Monnet. In this chapter, we consider policies aimed at reducing the amount of cash in circulation and their effect on the illegal activities of organised groups. Specifically, we focus on an emerging economy, India, and the policy of demonetization that the Indian government announced in November 2016, which aimed at exchanging small-denomination (but widely-used) banknotes with new ones. However, for reasons outside the control of the Reserve Bank of India, some bank branches received the new notes before others (Chodorow-Reich et al., 2020a). This unintended consequence produced a quasi-random difference in the level of cash in circulation across Indian districts. We exploit this heterogeneity to assess whether districts that experienced a more severe cash shortage also registered a decrease in violent activities related to the Maoists, which are an organised armed group with a developed cash-funding structure (Ramana, 2018).

By using a generalised difference-in-differences regression setup on daily data over 2014-2018, we find that districts with a more severe cash shortage registered relatively less violent events. In addition, we find that Maoists surrendered more in these districts, which is in line with an opportunity-cost channel. Finally, we find that this channel was less strong in districts were Maoists had larger resources to rebuild their finances. Overall, this chapter provides evidence that policies limiting the amount of cash in circulation can curb the illegal activities of organised groups that rely heavily on cash.

In summary, this doctoral thesis contributes to the recent literature studying new trends in the banking sector. Each chapter addresses a different type of structural transformation, from the process of digitization to new prudential regulations, and emphasizes the policy implications.

Chapter 1

Banks, Foreign Affiliates and the Internet

Banks, Foreign Affiliates and the Internet*

This paper is co-authored with Stela Rubínová *.

Abstract

Banks mainly use foreign affiliates and branches to provide financial services to customers located in foreign countries. However, in the last two decades banks' direct cross-border claims have been on the rise. We study whether the advent of internet technologies played a role in this trend. We do so by considering a novel dataset that combines data on countries' internet connections via submarine cables and Locational Banking Statistics by the Bank for International Settlements. Specifically, we focus only on banks' positions that can generate exports of financial services, namely cross-border loans and deposits to and by non banks. Gravity-model estimates for the period 2010-2019 suggest that cable connections significantly boosted these cross-border positions, while they had no effect on foreign-affiliate claims. In addition, while most of the effect on cross-border positions comes through the first connection, we find that multiple connections still contribute in reducing the barrier of distance. We confirm these results by estimating panel gravity models on both the full sample - which dates back to 1990 - and a sub-sample that isolates exogenous connections through routing cables. Finally, we find that the positive effect of cables on cross-border loans and deposits is stronger when clients are located in countries with small and unstable banking sectors.

Keywords: Financial Services, Foreign Affiliates, Internet

JEL classification: G2, F14, L86

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1.1 Introduction

Financial services are among the most internationalized sectors, accounting for the second largest share of international trade in services (WTO, 2019). Historically, most of this trade has been carried out by foreign affiliates located in the importing country. Yet, in the past ten years, cross-border transactions have been driving the growth in financial services' trade in certain markets. In this paper, we assess the role that the advent of the internet played in this trend.

In order to enter foreign markets, firms need to choose whether to keep production at home, and export, or move the production abroad with foreign direct investment (FDI), and sell locally. Serving foreign markets via foreign affiliates has higher fixed costs than cross-border exports but it saves the firm variable trade costs. This implies that the likelihood of serving a market via FDI increases with the size and distance of the destination market and with the size of the exporting firm (Helpman et al., 2004). Oldenski (2012) argues that this choice is also driven by the cost of transmitting information, and specifically by a trade-off concerning two main types of communication cost. On the one hand, firms with a production process that requires substantial *within-firm* communication choose to keep production at home and export. On the other hand, firms in sectors where communication *with customers* is fundamental choose to sell locally via foreign affiliates. Financial services such as retail and business banking are intensive in customer communication which favours local sales through FDI.[1] Other financial services, such as investment banking, require knowledge-intensive non-routine tasks that are costly to communicate on distance and are thus concentrated in a few locations and exported across borders.

Internet technologies facilitate communication and transmission of information. On one hand, the internet makes it easier for the headquarter to directly communicate with clients abroad via internet banking and, in general, to offer their services digitally. On the other hand, digital communication also facilitates information sharing within the bank, possibly expanding the scope of tasks that can be carried out by foreign affiliates.[2] The internet thus lowers the cost of both modes of trade and its impact depends on which type of communication, within-firm or with customer, is more important in a given sector.

We address this question empirically in a gravity framework by estimating the impact of bilateral internet connections on bilateral cross-border and foreign-affiliate positions. For this purpose, we build a novel origin-destination-year dataset merging different data sources

[1] For example, banks find it easier to grant loans to firms that are located in their same geographical area, as proximity facilitates credit scoring, decision and monitoring (e.g. Degryse and Ongena, 2005; Agarwal and Hauswald, 2010).

[2] For example, distance has generally made it difficult for foreign affiliates to share soft information on borrowers with the headquarter, with the results that foreign banks are reluctant to use affiliates to lend to, say, small but sound local firms (e.g. Mian, 2006). Internet may help the sharing of soft information and therefore allow affiliates of foreign banks to lend to sectors that otherwise would not be covered.

on banks' international positions and submarine cables.

To measure banks' cross-border and foreign-affiliates activities, we consider data on banks' international positions collected by the Bank of International Settlements (BIS) as proxies for exports in financial services. While international positions are stocks and include many claims and liabilities that do not generate pure exports - such as securities holding -, the granularity of BIS statistics allows us to isolate the portion of cross-border positions that are likely to generate trade in services. In particular, we use the Locational Banking Statistics (LBS) of the BIS to obtain a measure of cross-border "export-generating" positions by focusing on loans and deposits.[3] We then use a second dataset by the BIS, called Consolidated Banking Statistics (CBS), to obtain a measure of foreign-affiliate claims. While these two datasets are compiled with different principles - location versus nationality -, their combination allows us to obtain relevant proxies for cross-border trade and foreign-affiliate sales that have a much larger and consistent coverage than trade data based on balance-of-payments statistics.[4]

Our main measure of internet connectivity between two countries is based on data on submarine cables. Specifically, we use the underlying data of the Submarine Cable Map by TeleGeography to measure the number of cables connecting two countries, which vary by country pair and year.[5] The advantage of this measure is twofold. First, as it varies across country pairs, we can exploit the gravity model to estimate its correlation with banks' international positions. Second, it allows us to borrow from the literature and use an identification strategy to infer causality based on routing.

For the baseline results, we focus on an unbalanced origin-destination dataset obtained by averaging positions over 2010-2019. [6] The gravity-model estimates suggest that banks provide more loan and deposit services to clients in countries that have more internet connections with the home economy.[7] While most of the effect comes through the first cable connection, we find that having multiple cables help reducing the negative effect of physical distance on cross-border positions. Specifically, we estimate that it takes from 6 to 12 cable connections to completely defy distance.

We base on these baseline results to run a set of robustness checks and tests. Among others,

[3]Specifically, we consider the instrument "Loans and Deposits" for the counter-party sector "Non banks, total". We argue that most of the claims under this classification are loans to non banks, while most of the liabilities under this classification are deposits by non banks. As both of these positions generate trade, we add them up to obtain our measure for export-generating cross-border positions. For more details on this variable, please refer to Section 1.4 on Data.

[4]We show that for countries that have a comprehensive coverage of both types of statistics the two are closely correlated. For example, for the United States, our variable for cross-border and foreign-affiliate positions have a correlation coefficient with variables from the Bureau of Economic Analysis (BEA) of cross-border and foreign-affiliate sales of, respectively, .91 and .96.

[5]The map is available here. The underlying data is available here. To establish when cables become active, we use the date of ready for service. For more information, refer to the Section 1.4 on Data.

[6]The dataset contains a total of 22 origins of and 148 destinations. The 22 origins are Australia, Belgium, Brazil, Canada, Chile, Denmark, Finland, France, Greece, Hong Kong, Ireland, Italy, Macao, Mexico, Netherlands, Philippines, South Africa, South Korea, Spain, Sweden, United Kingdom and United States.

[7]We use both Ordinary Least Squares (OLS) and Poisson Pseudo Maximum Likelihood (PPML) estimators.

we find no strong correlation between cables' connection and claims of foreign affiliates. Intuitively, the type of information foreign affiliates share with their parents do not require fast internet or immediate response, and so the impact of cable connections is not as strong as it is on cross-border client communication (internet banking).[8] Furthermore, we find that the relationship between internet connections and cross-border positions becomes statistically significant at conventional levels after 2000, and that it becomes particularly strong after 2010. This evidence suggests that technologies like internet banking, which became widely used in the past 10 years, made it easier for banks to reach clients abroad.[9]

Gravity models correlating trade in services with measures of internet connectivity between countries can be subject to reverse causality, as countries may decide to invest in fast-internet connection specifically because they trade a lot in services. While this should be less of an issue when considering only a specific type of services like financial services, we test the robustness of our baseline results in two ways. First, we exploit the time dimension of our dataset and we estimate the gravity model on an origin-destination-year panel over 1990-2019. We confirm that the results do not vary when including, among others, origin-destination fixed effects and the lag of cable connections. Second, we consider a sub-sample of countries which received more internet connections only because they happen to be on the shortest sea route of large submarine-cable networks. Specifically, we focus on the cables connecting West-Europe and East-Asia countries (WE-EA), which were mostly financed by telecommunication companies at either end of the cables (Haltenhof, 2019). Given the length of these cables, they had to be connected through routing points across Middle-East and West-Asia countries (ME-WA) with access to the Indian Ocean. We can therefore exploit this quasi-random variation in the number of cables' connections to identify the impact of internet on banks' international positions. We thus estimate the gravity model on a sub-sample of cross-border positions of banks located in WE-EA countries on counterparties located in ME-WA countries over 1990-2019. Our baseline results are confirmed in this subsample, as we find that an increase in routing cables leads to an increase in banks' cross-border positions.

Finally, we examine possible channels driving the results for cross-border positions. First, we find that internet facilitates both cross-border loans and deposits. Second, we find that firms and individuals use the internet to borrow from, and deposit savings at, foreign banks especially when the banking sector at home is small (low assets) and unstable (low Z-Score). These findings can be of interest for policy makers in both developed and developing countries. First, the fact that internet facilitates cross-border lending is relevant for regulators, who care whether loans in their jurisdiction are supplied by either resident banks or by banks located

[8]The literature also identifies trading of financial assets as another process for which fiber-optic cables can make a significance difference (Eichengreen et al., 2016).

[9]Data from Eurostat reports that the percentage of individuals in the European Union using internet banking grew from 28% in 2007 to 60% in 2019. The data is available here.

abroad. Indeed, regulators can supervise foreign subsidiaries located in their jurisdiction, while they cannot regulate banks located in other jurisdictions. As cross-border lending increases, it is more difficult for regulators to monitor lenders and react to the build-up of risks emerging from (excessive) lending. The increase in internet connections could be considered as an early-warning indicator of a surge in cross-border lending, which may be addressed with either bi- or multilateral arrangements. Second, these findings are relevant for policy makers that aim to invest in Information and Communication Technology (ICT) infrastructure. The positive spillover effect of internet cables on cross-border lending is especially relevant for emerging economies, where the local banking system may not have the capacity to meet firms' demand for credit. Investments in cable connections with countries with a more developed banking system may therefore extend the supply of credit to local firms and boost economic growth.

The rest of the paper is organised as follows. Section 1.2 describes the existing literature and the contributions of this paper. Section 1.3 reports stylised facts on banks' cross-border positions and submarine cables. Section 1.4 outlines the data used in the analysis. Section 1.5 describes a conceptual framework outlining how banks can use the internet to both communicate with their clients and foreign affiliates and discusses the gravity model. Section 1.6 presents the baseline results for the effect of internet cables on banks' cross-border positions and robustness checks. Section 1.7 discusses the comparison with foreign-affiliates positions, the non-linear impact of cables, and the timing of the baseline relationship. Section 1.8 addresses the possible endogeneity with panel regressions and sub-sample of routing cables. Section 1.9 discusses the possible channels. Section 1.10 concludes.

1.2 Related literature

Our paper contributes to several strands of the empirical literature studying trends in international banking. A first strand of this literature uses gravity models to explain the cross-country variation in banks' cross-border positions. Since the seminal paper of Portes and Rey (2005), many authors have used a gravity framework to explain differences in banks' international positions (e.g. Aviat and Coeurdacier, 2007; Hellmanzik and Schmitz, 2017; McCauley et al., 2019; Brei and von Peter, 2018). Our paper adds two main contributions to this literature. First, to the best of our knowledge, we are the first to focus on cross-border positions that can generate trade and compare them to foreign-affiliate positions. Second, there is no study that addresses the role played by internet technologies in driving both these international positions in recent years. Among the papers of this recent literature, our paper is closest to Hellmanzik and Schmitz (2017). The authors address the relationship between equity and debt holdings in all sectors and internet hyperlinks between countries.[10] They use a gravity framework for the year 2009 and show that financial integration is higher for countries with larger virtual proximity, especially for equity holdings. They also find stronger results for banks' cross-border holdings, and suggest that "banks have a particular capability to overcome information asymmetries via the internet". Our analysis generally differs from Hellmanzik and Schmitz (2017) as we focus on positions that can generate trade of financial services, and as we are interested to examine the difference between cross-border and foreign-affiliate activities.[11]

A second strand of the literature has used gravity models to explain flows of trade in services, also specifically for financial services and trade through foreign affiliates. For example, Andrenelli et al. (2018) and Benz and Jaax (2020) show that policies restricting trade in services limit activities of foreign affiliates. Our paper lies at the intersection between these two strands, as we use banks' international positions as proxy for trade in financial services in a gravity framework. The data on international positions, coupled with data on internet cables, allows us to cover a much larger time span than standard trade datasets and to use more rigorous identification strategies.

Our paper also contributes to the literature studying how firms and banks operate in foreign markets. Oldenski (2012) points out that the trade-off between within-firm and client communication drives firms' decision on whether to enter a foreign market by cross-border trade or through foreign affiliates. Oldenski argues that banks enter foreign markets mainly through affiliates as in financial services client communication is key. Specifically on banks, Galema and Koetter (2018) focus on a sample of German banks and find that less profitable, more risky and larger banks are more likely to operate via foreign affiliates. Given the importance of foreign affiliates for exports of financial services, authors have studied them extensively, for example by comparing them to local banks and considering differences in regulations and taxes in host countries (Dages et al., 2000; Cerutti et al., 2007; Temesvary, 2018).[12] Other authors also studied global banking channels through the lenses of parent-

[10] They use the Coordinated Portfolio Investment Survey by the International Monetary Fund for measures of bilateral portfolio investment holdings and data by Chung (2011) to measure the number of internet hyperlinks.

[11] More specifically, our paper deviates from Hellmanzik and Schmitz (2017) in four main ways. First, our interest focuses on the financial sector. Second, we are interested in explaining trends in trade in financial services, so we consider only cross-border positions that can generate trade, and we compare them to foreign-affiliate positions. Third, we use the number of submarine cables as our measure for internet connection, which allows us to study the impact of internet through time, from 1990 onwards, while the measure if internet connections across countries based on internet hyperlinks of Hellmanzik and Schmitz (2017) focuses on the year 2009. Fourth, as we are interested in exploring possible mechanism in export-generating positions, we propose a disaggregation between loans and deposits.

[12] For example, Dages et al. (2000) compare foreign-owned banks (foreign affiliates) to local banks in Mexico and Argentina and show that foreign banks supply more and less-volatile lending. Cerutti et al. (2007) differentiate between foreign branches and subsidiaries and show that the first are preferred to the second when foreign markets have higher tax rates and lighter banking regulations. Similarly, Temesvary (2018) finds that US banks use cross-border exports rather than foreign-affiliate sales when regulations in

affiliate interactions and found consistent spillover effects (Anginer et al., 2017; Temesvary et al., 2018).[13] We add to this literature by showing how a decline in communication costs, due to an increase in internet connectivity, shapes the way in which financial services are traded.

Furthermore, we contribute to the literature on lending and distance (e.g. Degryse and Ongena, 2005). For example, in a recent paper Levine et al. (2020) show that the key role of soft information about borrowers makes lending more difficult as lender-borrower distance increases. Mian (2006) reports a similar mechanism for foreign banks in low-income economies, with a case-study on Pakistan.[14] We extend this finding by showing that internet connectivity decreases the impact of distance on the provision of both credit and deposit services.

Finally, we build on the literature addressing the relationship between the internet and trade in services. In one of the first studies in this literature, Freund and Weinhold (2002) find a positive correlation between internet access - measured with the number of internet hosts in a country - and service trade.[15] Other authors focus specifically on measuring internet connections with submarine cables. Eichengreen et al. (2016) finds that fast internet - measured with number of internet cables - boost foreign-exchange transactions in the major financial centers. More generally, Haltenhof (2019) finds that countries with more cable connections trade more in services. These results rely on an identification based on routing, which we will use in this paper.

1.3 Stylised Facts

In banking, client communication is key. Banks have been using foreign affiliates as the main way to run their business abroad. However, in the last two decades the volume of financial services provided across borders has been increasing steadily. To outline this trend, we consider aggregate data from the Locational Banking Statistics (LBS) of the BIS. These statistics include stock values of claims (or liabilities) of banks in home countries on (or held by) counterparties in destination economies. In this paper, we focus on loan and deposit positions for non-bank counterparties, namely firms and individuals.

Figure 1.1 report the aggregate time series for positions with non-bank counterparties. Panel (a) reports claims to non banks, divided by loans and deposits, and other claims. The key

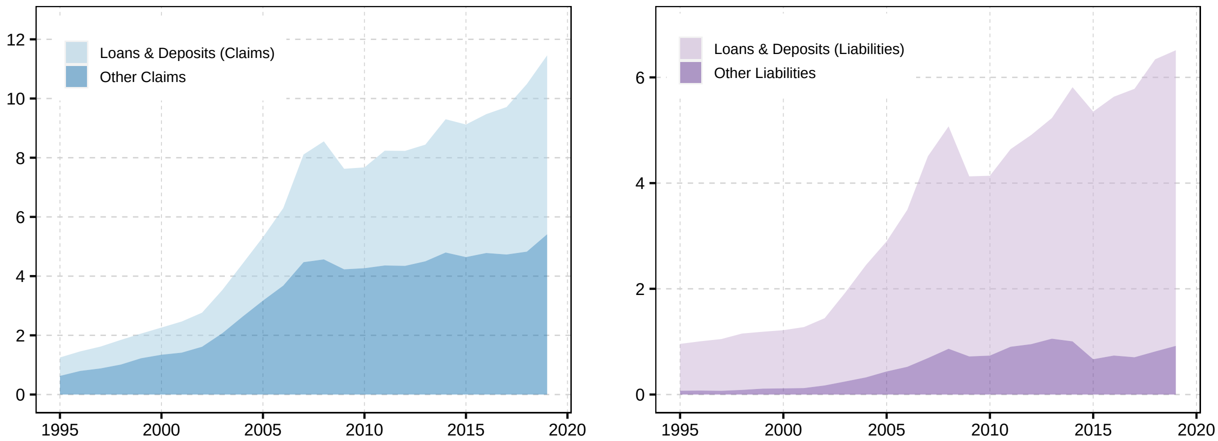
destination economies are stricter.

[13] Anginer et al. (2017) find a positive correlation between parent's and foreign affiliates' default risk. Temesvary et al. (2018) show that US banks adjust their cross-border positions in response to the FED's monetary policy, while the claims of their affiliates respond mainly to monetary-policy changes in host countries.

[14] Mian shows that, as distance makes foreign banks reluctant to lend to soft-information firms, small but sound firms do not have access to foreign credit.

[15] Other papers that draw similar conclusions are, among others, Freund and Weinhold (2004); Choi (2010); Clarke and Wallsten (2006).

Figure 1.1: Positions with Non-Bank Counterparties



(a) Claims to Non Banks

(b) Liabilities by Non Banks

Notes: Figure 1.1 reports time series of export-generating positions, sourced from the Locational Banking Statistics of the BIS. Panel (a) shows series for loans (clear blue) and other claims (dark blue) to non banks. Panel (b) shows series for deposits (clear purple) and other liabilities (dark purple). These aggregates are sums of all country-pair positions in a given year and are expressed in Trillions of US Dollars.

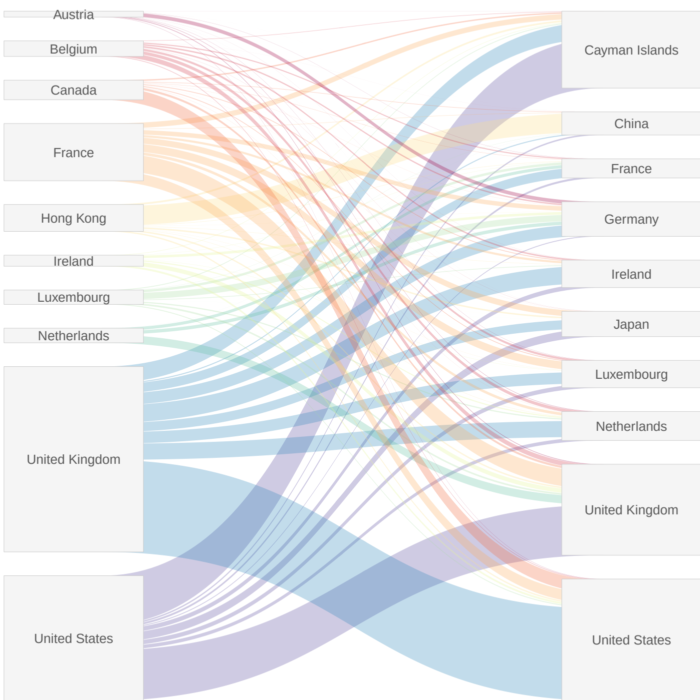
aspect is that loans and deposits to non banks are mainly loans, as firms and individuals do not issue deposit services. These cross-border claims have been increasing largely since 1995. In addition, the growth of other claims has slowed down after the crisis of 2008, which is in line with the literature on the so called financial deglobalisation. One of the reasons behind the post-crisis slowdown of cross-border sales could be the response of banks to stricter banking regulations at home (McCauley et al., 2019).[16] Differently, the positions on loans in panel (a) seems to be less affected by this logic, as they keep increasing at a rapid pace also after the financial crisis. We are interested to study whether internet technologies played a role in determining this trend, as services like internet banking, which developed largely in recent years, can significantly facilitate cross-border lending. Panel (b) reports liabilities by non banks, divided by loans and deposits, and other liabilities. Again, the key aspect is that loan and deposit liabilities by non banks are mostly deposits, as firms and individuals do not issue credit services. These liabilities outline a similar logic than the claims. Indeed, while other liabilities slowed down after 2008, deposits kept increasing. As for lending, internet technologies and home banking can largely facilitate cross-border deposits, as clients can use the internet to open a bank account abroad.

In Figure 1.A.1, we report the time series of positions for all counter parties, which also

[16] McCauley et al. (2019) address this question by focusing on the Consolidated Banking Statistics database by the BIS, which is based on nationality of respondents (rather than residency). The authors show that the the global shrinkage in international positions in the aftermath of the 2008 crisis was actually driven by EU banks, which deleveraged on their foreign positions to restore their capital ratios and comply with EU regulations.

include banks.[17] The figures show that, with these less-specific positions which include also exchanges of financial services between banks, all claims and liabilities slowdown after 2008.[18] This aspect motivates further our choice to focus on positions that are specific to non banks. Importantly, these are the only types of positions in the publicly-available version of the LBS statistics that uniquely identify export-like services for which banks receive either an interest or a fee, namely cross-border loans and deposits. As the baseline measure for our analysis, we consider the sum of these two positions, which we refer to as "export-generating" positions.

Figure 1.2: Network of Export-Generating Positions



Notes: Figure 1.2 reports the trade network of export-generating positions, defined as the sum of loans and deposits to non-banks, collected from the Locational Banking Statistics of the BIS. Country-pair aggregates are computed with post-2010 averages for each country pair. The reported countries are the top-10 origins (left column) and the top-10 destinations (right column) of export-generating positions.

Figure 1.2 outlines the location of the reporting institutions (banks) and counterparties (firms and individuals) for post-2010 averages of these export-generating positions. Left and right columns report the 10 largest, respectively, exporters and importers of related services and the coloured bands are the breakdown of positions by origin and destination.[19] United

[17]These series are less detailed than the series seen above. Indeed, claims on loans and deposits to all banks can also include deposits by banks in home countries with banks in destination countries. Similarly, liabilities on loans and deposits by all banks can also include loans that banks in home economies receive from banks in destination countries

[18]Figure 1.A.2 in Appendix 1.A compares all these times series together in a single graph, in which time series are indexed to 1995.

[19]The larger the width of the bands, the larger the amount of positions.

Kingdom (UK) and United States (US) are by far the largest providers of cross-border loans and deposits, followed by France. UK banks provide a significant amount of loan and deposit services to firms and individuals in the US. In addition, they are also the largest foreign providers of loan and deposit services in France, Germany, Ireland, Japan, Luxembourg and Netherlands. This is no surprise, considering that the banking sector in the UK is quite developed. US banks have a similar network of UK banks, though the average amounts of exported services are lower.

Interestingly, both UK and US banks have large loan and deposit positions with counterparties in the Cayman Islands. The logic of tax arbitrage would suggest that firms in UK and US hold bank accounts at the Cayman Islands for tax arbitrage. These trends highlight the opposite logic, i.e. firms in the Cayman Islands holding accounts at banks in the UK and US. This dynamic is in line with the argument put forward by Coppola et al. (2020), who show that firms have used their affiliates in tax havens to raise capital in foreign markets. What we are possibly observing here are affiliates of, say, US firms located in the Cayman Islands, which hold an account with a US bank. This aspect should not affect the logic we are interested to grasp in our analysis, as internet can enable any firm to connect with banks abroad, regardless of whether it is located in a tax heaven or not.[20]

To estimate this channel, we consider submarine cables as a measure for internet connections at the level of country pairs. Surprisingly, submarine cables are not a new thing. The first transatlantic cable was laid in 1866 to transmit telegraph signals between United States and Europe. Cables evolved with technological process and in 1956 the first coaxial cable made it possible to connect the United States and the United Kingdom with multiple telephone lines. At the end of the 1980s', fiber-optic cables took over. These cables allow much faster connection, as they significantly reduce latency time and increase bandwidth.[21] From 1990 onwards, the number of submarine cables connecting countries grew exponentially.

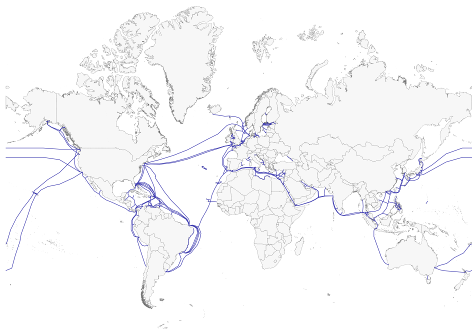
Figure 1.3 outlines the evolution of the submarine cables in the last three decades. In 1990, there were only few fiber-optic cables, mainly connecting the United Kingdom with Ireland and the Netherlands as well as Denmark with Sweden (relatively short cables). The first wave of submarine cables took place in the following ten years and by 2000 submarine cables connected all continents. Among others, the cable FALG Europe-Asia (FEA) connected Western European to East-Asian countries passing through the Suez canal and several Middle-East and East-Asian countries. This cable was financed by Global Cloud Xchange, which mainly aimed at connecting Europe and Asia. As the cable was very long, it had routing points in those countries that happened to be on the shortest sea way between Europe

[20]Figures 1.A.3 and 1.A.4 in Appendix 1.A, breaks down these networks for, respectively, claims and liabilities, with a differentiation between non-bank and all counterparties. When considering all counterparties, positions of UK and US banks have more similar volumes, which suggests that US banks hold a significant amount of bank-to-bank positions.

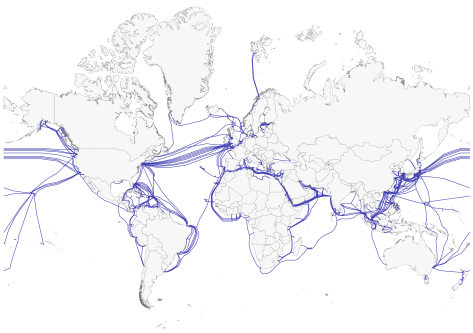
[21]For a detailed history of these cables, refer to Wenzlhuemer (2013) and Eichengreen et al. (2016).

Figure 1.3: Evolution of Submarine Cables

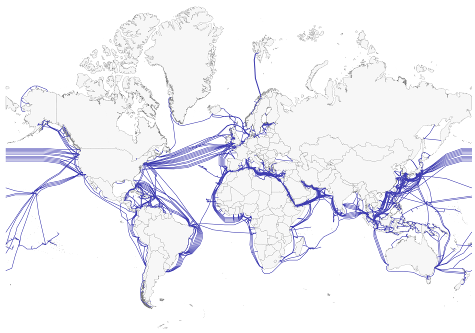
(a) 1990



(b) 2000



(c) 2010



(d) 2020

Notes: Figure 1.3 reports the evolution of submarine cables at different points in time, namely 1990 (panel a), 2000 (panel b), 2010 (panel c) and 2020 (panel d). Both geo-spatial variables and variables on cables' characteristics are sourced from Telegeography. A cable appears in the maps only when it is ready for service. Cables' routs are stylized and do not reflect the actual path taken by the cables.

and Asia. In Section 1.8, we will use the quasi-random connections of routing countries to identify the causal impact of submarine cables on banks' international positions. Finally, panels (c) and (d) show that the number of cables kept growing rapidly after 2000. By 2006, 99% of international communications were passing through submarine cables (Eichengreen et al., 2016). By 2020, there were a total of 478 active submarine cables connecting the majority of countries with sea access around the world. In our analysis, we will exploit this large variation of submarine cables to identify the relationship between internet and cross-border operations of banks in the recent years.

1.4 Data

The literature assessing trends in international banking activities with a gravity framework can be divided in two strands. A first strand considers trade data as the dependent variable of interest. Take a cross-border loan as an example. Trade data would register the interest payment that the borrower in the destination country transfers to the lender in the home economy, namely an export of financial services. Among others, Andrenelli et al. (2018)

and Benz and Jaax (2020) use statistics for Affiliates of Multinational Enterprises (AMNE) by the OECD to measure trade through foreign affiliates. Differently, a second strand of the literature focuses on banks' international claims. Going back to the loan example, international-claim data would register the amount outstanding of the cross-border loan in the asset side of the lender's balance sheet, namely an international claim (stock). For example, Brei and von Peter (2018) use the Locational Banking Statistics (LBS) database by the BIS to measure cross-border financial positions. Similarly, McCauley et al. (2019) use another BIS dataset, called Consolidated Banking Statistics (CBS), and measure banking groups' cross-border claims and claims of their foreign affiliates in host economies. In this paper, we follow the second strand on international claims, but with the aim of considering only the stock variables that can proxy trade in financial services. In other words, we will focus on what we call "export-generating" positions.

Cross-Border Positions. We use the LBS database to obtain a measure for cross-border export-generating positions. The LBS database by the BIS gathers data on cross-border claims and liabilities of banks by residence on an unconsolidated, standalone basis.[22] In the publicly-available version of the LBS at the origin-destination-quarter basis, cross-border claims and liabilities can be disaggregated in type of instruments - all instruments vis-à-vis loans and deposits - and sector of counterparty - all sectors vis-à-vis non banks. We use this disaggregation to build our proxy for cross-border trade based on cross-border claims. Cross-border trade in financial services include, inter alia, payments for services of retail and wholesale banking, such as interest rates on loans and deposits to individuals and corporations, and for investment banking services, such as commission fees for the management of financial assets (e.g. Cerutti et al., 2007). For our main proxy, we focus on retail and wholesale banking and we consider cross-border claims and liabilities for the instrument "Loans and deposits" on the counterparty sector "Non banks, total".[23] For what concerns claims, as individuals and corporations do not provide deposit services (as banks do), it is reasonable to assume that these claims correspond to cross-border loans to individuals and corporations located in destination countries.[24] For what concerns liabilities, as individuals

[22]In a nutshell, reporting banks could be standalone entities located in the reporting country, head offices and subsidiaries of banking groups located in the reporting country, and foreign branches and subsidiaries of a controlling parent that is located outside of the reporting country.

[23]In the publicly-available version of the dataset, this subset is the most disaggregated at the origin-destination-quarter dimension. For example, it is not possible to disaggregate loans and deposits.

[24]Non banks can also include non-bank financial institutions. The reporting guidelines for the BIS international banking statistics define Non-bank financial institutions as "private or public financial institutions, other than banks, engaged primarily in the provision of financial services and activities auxiliary to financial intermediation such as fund management". They include "special purpose vehicles, hedge funds, securities brokers, money market funds, investment funds, pension funds, insurance companies, financial leasing corporations, central clearing counterparties, unit trusts, other financial auxiliaries and other captive financial institutions, and any public financial institutions such as development banks and export credit agencies". The definition of the LBS for "banks" is "deposit-taking corporations, except the central bank" (Article 2.5

and corporations do not grant loans (as banks do), they mostly correspond to the savings of individuals and corporations in destination countries deposited at a bank located in the home country.[25] We therefore sum these two types of claims and liabilities to obtain a measure for export-generating positions, which we use to proxy trends in cross-border trade of financial services. There are two important aspects of this measure. First, while this variable does not include investment banking and loans and deposits to other banks, it excludes all holdings of securities - including holdings of foreign affiliates - and deposits of foreign banks held at the home-country bank, both of which do not generate trade in services for the reporting institution. Second, these positions include claims of resident affiliates of foreign groups, and as such they can include positions that generate re-exports. This is consistent with how cross-border trade in financial services is recorded.

In Section 1.7, we also check the impact of internet on foreign-affiliate positions. We source these positions from the Consolidated Banking Statistics of the BIS. These statistics differ from the LBS in several ways. Above all, they are based on the nationality, rather than location, of the reporting institutions and they do not allow disaggregation between type of counterparty and position. In Appendix 1.B, we describe these data and we discuss how both cross-border and foreign-affiliates positions correlate with standard variables of cross-border exports and foreign-affiliate sales.

Internet Cables. The literature uses different measures for internet connectivity between countries. For example, Hellmanzik and Schmitz (2017) consider a variable for virtual proximity measuring bilateral, inter-domain hyperlinks that internationally connect web pages in the origin country to web pages in the destination country. With a similar logic, Eichengreen et al. (2016) measure countries' fast-internet connectivity with large financial centers with the number of fiber-optic submarine cables between these countries and such financial centers. Submarine cables have also been used in the labour-market literature. For example, Hjort and Poulsen (2019) use the exogenous arrival of submarine cables in Africa to measure the impact of fast internet on local employment. We follow Eichengreen et al. (2016) and focus on the number of submarine cables connecting country pairs.

We obtain data on submarine cables from the underlying dataset of the Submarine Cable Map by TeleGeography. This dataset covers all submarine cables from 1989 to today. The reported variables include cables' length, landing points, ready-for-service year, and owners.[26]

at page 9 of the Reporting guidelines for the BIS international banking statistics). Consequently, "Non banks" should not be able to provide a deposit service. While we cannot disentangle such institutions, we argue that deposits of banks held at non-bank financial institutions should not exist or be a very limited portion of the overall claims. For a more detailed description of the counterparty sectors, refer to Table 2.6 at page 17 and Table 6.1 at page 36 of the Reporting guidelines for the BIS international banking statistics, available here

[25] As for claims, also liabilities include non-bank financial institutions. As only some of these institutions could in principle grant a loan, we argue that the share of liabilities corresponding to loans by non-bank financial institutions in host economies to banks in home economies is low.

[26] The dataset is available on the GitHub page of TeleGeography, here

Using data on lending points and year in which the cable becomes ready for service, we compute an origin-destination-year variable for number of submarine cables linking country pairs. Similarly to Eichengreen et al. (2016), we consider both direct and indirect connections within each cable. Indeed, a cable can have either two or more landing points. Consider the US and the UK. The US could connect to the UK with a simple point-to-point cable (direct connections). However, the US could connect to the UK also with a cable that starts from the US, touches Ireland, and then finally arrives in the UK (indirect connections).[27]

Other Variables and Summary Statistics. For our main controls, we follow the literature predicting trends in banks' international claims with a gravity framework. This literature points out that most of the controls are the same as in standard gravity frameworks (e.g. Portes and Rey, 2005; Aviat and Coeurdacier, 2007; Brei and von Peter, 2018; McCauley et al., 2019). We obtain standard gravity variables from the CEPII dataset. They include distance between countries, whether one of the two countries was a colony of the other, whether two countries share the same language, religion, legal system or currency, and whether two countries have a free trade agreement.

We merge the databases on international positions, submarine cables and gravity controls to obtain an origin-destination-year dataset over 1990-2019. As we are mainly interested in recent trends in trade in financial services, we will focus on the period 2010-2019 for the baseline specification. Specifically, for the baseline estimates we follow most of the literature on gravity models and we use an origin-destination dataset obtained by taking country-pair averages post 2010. [28] Furthermore, in the baseline dataset we drop pairs with contiguous countries, as we assume that contiguous countries would connect via terrestrial (rather than submarine) cables. We also drop landlocked countries, which do not have sea access (like Austria and Switzerland). Finally, for the baseline dataset we exclude the Cayman Islands, which are a tax haven and a large recipient of cross-border banking positions. The resulting baseline-regression dataset is an unbalanced country-pair panel including 22 origins and 148 destinations, for a total of 1,958 observations.[29]

Table 1.1 reports summary statistics for both baseline-regression dataset, with our main variables, and a full dataset reporting also alternative measures for cross-border and foreign-affiliate positions. There are a couple of things worth noting. First, the maximum value for the cross-border positions of over 1 Trillion dollars corresponds to positions of UK banks in the US. In the robustness checks, we will drop this country pair to make sure it is not driving

[27] We do not consider indirect connections that can emerge from different cables, i.e. a connection between the US and the UK formed by one cable that goes from the UK to Ireland and another, different cable that goes from Ireland to the UK.

[28] We report regression results on origin-destination-year datasets in Section 1.8 for endogeneity checks

[29] The 22 origins are Australia, Belgium, Brazil, Canada, Chile, Denmark, Finland, France, Greece, Hong Kong, Ireland, Italy, Macao, Mexico, Netherlands, Philippines, South Africa, South Korea, Spain, Sweden, United Kingdom and United States.

Table 1.1: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Regression Dataset							
LBS CB Exp. Positions (M$)	1,958	2,880	31,765	0	7	528	1,164,105
Cables (Number)	1,958	0	1	0	0	0	12
Distance (Km)	1,958	7,583	4,436	61	3,910	10,175	19,630
Manufacturing Trade (M$)	1,958	1,819	6,059	0	32	1,058	126,462
Full Dataset							
LBS CB Exp. Positions (M$)	3,397	2,839	27,883	0	5	410	1,164,105
LBS CB Tot. Claims (M$)	4,243	5,807	39,297	0	3	520	1,171,876
CBS CB Tot. Claims (M$)	3,258	4,173	25,431	0	3	676	900,918
CBS FA Tot. Claims (M$)	908	11,386	47,372	0	18	3,834	668,734
Cables (Number)	61,256	0	0	0	0	0	12
Distance (Km)	50,884	8,485	4,682	10	4,783	12,004	19,951
Manufacturing Trade (M$)	37,959	364	4,490	0	0	15	400,487

Notes: Table 1.1 reports summary statistics for an origin-destination dataset over 2010-2019. Summary statistics under Regression Dataset report statistics for the dataset used in the baseline regressions, while the ones under Full Dataset report statistics for the unrestricted dataset. LBS CB Exp. Positions is the sum of cross-border loans and deposits from the Locational Banking Statistics (LBS) by the BIS. LBS CB Tot. Claims and CBS CB Tot. Claims are overall cross-border claims from, respectively, the LBS and the Consolidated Banking Statistics (CBS) of the BIS. CBS FA Tot. Claims are the overall foreign-affiliate claims from the CBS database of the BIS. Cables is the number of cables' connections between countries. Distance is the distance between countries. Manufacturing Trade is the flow of trade in manufacturing goods between countries.

the results. Second, the distribution of submarine cables' connections is skewed to the left, as the mean value is zero connections per country pair, with a peak of 13 between Sweden and Denmark. It is expected that the Nordic countries are connected by a large number of cables, as they host a large quantity of servers. Third, there is a difference between the total cross-border claims in the Locational Banking Statistics (mean of 5,807) and in the Consolidated Banking Statistics (mean of 4,173), which is mainly given by the differences in reporting principles, i.e. location vs nationality. We will be using the Consolidated Banking Statistics to source the variable for the total claim of foreign affiliates. Aside from the difference in the reporting principle, the variable has also a lower availability across country pairs (only 908 observations available).

1.5 Conceptual framework and Estimation Method

This section lays down a conceptual framework to think about possible channels in which headquarters may use internet to conduct their business abroad. In addition, it outlines the gravity framework for estimation.

1.5.1 Conceptual framework

No cable connection. The starting point of this framework is a bank targeting an entry in a foreign market that has poor internet connection with the home country. The headquarter (HQ) enters the foreign market with foreign affiliates (FA), as, in banking, communication with the client is fundamental. While there is no issue of client communication (solved with foreign affiliates), there is a potential issue of within-firm communication, as some banking services are difficult to automate (nonroutine). Banking services that can be automated are, for example, deposits.[30] Banking services that can be more difficult to automate are mortgages and loans to corporations - the first stage of credit scoring can be automated, but the decision process requires more skills -, and investment banking - portfolio management is not that automatic. The FA is on the consolidated balance sheet of the HQ, so the HQ cannot allow the FA to have lower standard for services supplied, which can lead to non-performing loans and issues with home-country supervisors. As a result, the HQ will allow the FA to provide routine services, like deposits, but will allow little lending and portfolio management.[31]

Cable connection. From this starting point, we move to a scenario in which a cable connects the home country to the destination economy. On one hand, the HQ can now (partially) meet the aspect of communication with the client through the internet (rather than through the FA). This will allow the HQ to directly reach clients and increase cross-border exports. For example, the HQ can directly take on deposits from clients in destination markets, as they can access the home-banking website of the HQ with fast internet. In addition, it can grant loans to large corporations in host economies, as they can directly use the website of the HQ to manage interest payments.[32] Furthermore, clients in the destination market can use the website of the HQ to place buy and sell orders for investment instruments, therefore boosting the HQ's fees in portfolio management. On the other hand, the HQ can now better meet the issue of within-firm communication related to nonroutine services, as it becomes easier for the FA to share information with the HQ about credit scoring and investment decisions.

Predictions. This framework suggests what we can expect from coefficient estimates of the relationship between internet and both cross-border and foreign-affiliate operations. First, we expect a positive relationship between internet and cross-border activities, as banks can use the internet to directly reach clients in foreign markets. Second, we also expect a positive relationship between internet and foreign-affiliate activities, as internet allows

[30] Usually, clients need to fill in a standard form providing information to open a deposit account

[31] For example, it may be difficult for the FA to share with the distant HQ buy and sell decisions on portfolio management, or credit scores on loans

[32] Also, note that, in general, in order to ask for a loan to a foreign bank, corporations need to open a bank account with that foreign bank. Internet facilitates this step, as corporations - as people - can open the account on the website of the foreign bank.

headquarters to share more information with their affiliates, thus boosting their activities. This being said, we may expect a stronger impact for cross-border than foreign-affiliate operations. Indeed, internet is essential for headquarters to grant loans and deposits directly to customers in foreign markets. In this case, say, the telephone (or emails) is not a substitute, as it is not feasible for the headquarter to manage all clients' operations concerning loans and deposits over the phone (or through a slow website or emails) - these includes money transfers, balance checks, payment of instalments, and so on. On the other hand, (fast) internet is not as essential for the information sharing between the headquarter and affiliates. In this case, at least part of information could be shared also with poor internet connection, via phone calls and emails. For example, banks could use intranet to share information about credit scores. Without fast internet, this process is more complicated, but still feasible. Overall, as there is presumably no or few substitutes to fast internet for client communication, while there can be substitutes for within-firm communication, we may expect the effect of internet on cross-border trade to be stronger than for foreign-affiliate sales.

1.5.2 The Gravity Model

We follow the literature and we use the gravity framework to model banks' cross-border positions (e.g. Portes and Rey, 2005). Specifically, we assume that cross-border positions are proportional to countries' income level and inversely proportional to the trade costs between countries. By using a similar notation to Lendle et al. (2016), exchanges of financial services between countries can be modelled with the following gravity equation:

$$x_{ij} = \frac{y_i y_j}{y_w}\left(\frac{t_{ij}}{P_i \Pi_j}\right)^{\epsilon} \tag{1.1}$$

where x_{ij} is the positions of banks in country i on counterparties in country j. y_i and y_j are total income, in, respectively, exporting country i and importing country j, and y_w is world total income. t_{ij} are trade costs between countries i and j, P_i and Π_j are the multilateral resistance terms in the importing and exporting countries, and ϵ is the trade cost elasticity of bilateral trade.[33] The trade costs t_{ij} and its components can be defined as follows:

$$t_{ij} = D_{ij}^{\alpha_D} M_{ij}^{\alpha_M} e^{(\alpha_C C_{ij} + \boldsymbol{Z}'_{ij}\boldsymbol{\alpha}_Z)} \tag{1.2}$$

As in standard trade models, D_{ij} is the physical distance between country i and j and $\boldsymbol{Z}'_{ij}$ is a vector containing binary variables that measures other barriers to trade, such as whether countries i and j share a language, currency, religion, legal system, have colonial relations or a free trade agreement. We then augment this standard trade equation with two terms. First, we include trade in goods M_{ij}, which enters the equation as a multiplicative component,

[33]For a detail explanation and derivation of these components, see Lendle et al. (2016).

like distance (e.g. Aviat and Coeurdacier, 2007). Second, we include the number of cables' connections C_{ij}, which enters the equation in the exponential argument. The rationale of including this variable in the exponential form rather than in the multiplicative form - like distance and trade in goods - is to allow country-pair observations with zero connections to contribute to the coefficients' estimation.

After substituting 1.2 into 1.1 and taking the logs of both sides, we obtain the following equation:

$$\begin{aligned} ln(x_{ij}) =& ln(y_i) + ln(y_j) - ln(y_w) + \beta_D ln(D_{ij}) + \beta_M ln(M_{ij}) + \\ & \beta_C C_{ij} + \boldsymbol{Z}'_{ij}\boldsymbol{\beta}_Z - \epsilon ln(P_i) - \epsilon ln(\Pi_j) \end{aligned} \tag{1.3}$$

with $\beta_k = \epsilon\alpha_k$, where k is the subscript for the type of trade cost. For estimation, we augment this equation with fixed effects and a stochastic term:

$$ln(x_{ij}) = \beta_D ln(D_{ij}) + \beta_M ln(M_{ij}) + \beta_C C_{ij} + \boldsymbol{Z}'_{ij}\boldsymbol{\beta}_Z + \boldsymbol{\delta}_i + \boldsymbol{\delta}_j + \lambda + u_{ij} \tag{1.4}$$

where $\boldsymbol{\delta}_i$ and $\boldsymbol{\delta}_j$ are, respectively, the origin and destination fixed effects and u_{ij} is a normally-distributed error term.

We estimate coefficients of Equation 1.4 with Ordinary Least Squared (OLS). In addition, we also report estimates obtained with the non-linear Poisson Pseudo Maximum Likelihood (PPML) estimator, which allow for zero-value positions and correct for a possible bias related to heteroskedastic disturbances (Silva and Tenreyro, 2006). The interpretation of coefficients (elasticities) is the same for OLS and PPML estimates.

1.6 Baseline

In this section we present the baseline results for estimates of Equation 1.4, which are computed with both OLS and PPML estimator. In the non-linear model, missing observations of the dependent variables are substituted with zero values, which implies a considerable increase in the sample's size.

Table 1.2 reports the results. We start with a simple log-linear gravity model, by regressing cross-border, export-generating positions on the natural logarithm of distance and standard controls for trade costs. These are dummies which equal one if two countries share colonial relations, language, religion, legal system and a free trade agreement. Generally, the point estimates reflect the findings of the majority of the literature, i.e. gravity plays an important role in explaining variations in cross-border banking claims (e.g. Brei and von Peter, 2018). The amount of claims is lower for countries that are more distant, while it is larger for countries which share colonial relations, language and religion. These results are generally in line with our expectations and the findings of Portes and Rey (2005). For example, the literature points out that banks find it more difficult to lend to distant clients, as it is more difficult to manage both soft and hard information on the borrowers and therefore monitor them (e.g. Brüggemann et al., 2011).

However, one could argue that the large negative effect of distance is counter intuitive, as distance should not matter too much for trade of (weightless) services. Aviat and Coeurdacier (2007) and other authors have reported that the effect of distance on cross-border claims mainly passes through trade in goods. We control for this logic in column 2, in which we include flows of trade in manufacturing goods in the right hand side.[34] In line with findings of Aviat and Coeurdacier (2007), the coefficient on trade is positive and significant and the coefficient of distance is reduced by half, moving from -1.153 in column 1 to -0.745 in column 2.

Table 1.2: Baseline

VARIABLES	(1) ln(CB)	(2) ln(CB)	(3) ln(CB)	(4) ln(CB)	(5) CB	(6) CB	(7) CB	(8) CB
ln(Distance)	-1.153***	-0.745***	-0.676***	-0.715***	-0.683***	-0.357***	-0.192**	-0.414***
	(0.066)	(0.079)	(0.082)	(0.084)	(0.068)	(0.090)	(0.086)	(0.102)
ln(Trade)		0.437***	0.426***	0.419***		0.559***	0.520***	0.471***
		(0.053)	(0.053)	(0.053)		(0.096)	(0.087)	(0.080)
Cables			0.152***	-0.299			0.138***	-0.428***
			(0.031)	(0.194)			(0.023)	(0.134)
ln(Distance) × Cables				0.059**				0.074***
				(0.025)				(0.018)
Colony	0.546**	0.548**	0.569**	0.562**	1.181***	1.070***	0.967***	0.735**
	(0.227)	(0.233)	(0.231)	(0.230)	(0.316)	(0.371)	(0.330)	(0.309)
Language	0.761***	0.557***	0.575***	0.556***	1.548***	1.323***	1.177***	0.937***
	(0.120)	(0.116)	(0.115)	(0.116)	(0.210)	(0.206)	(0.212)	(0.259)
Religion	0.377*	0.330	0.333*	0.359*	0.840**	0.708**	0.610**	0.737**
	(0.207)	(0.203)	(0.201)	(0.202)	(0.343)	(0.337)	(0.300)	(0.315)
Currency	0.143	0.075	0.123	0.114	0.314	0.262	0.385*	-0.012
	(0.176)	(0.169)	(0.173)	(0.172)	(0.230)	(0.222)	(0.199)	(0.206)
Legal	0.239***	0.160*	0.147*	0.165**	-0.057	-0.136	-0.193	-0.138
	(0.084)	(0.084)	(0.083)	(0.084)	(0.143)	(0.131)	(0.118)	(0.124)
FTA	0.286**	0.141	0.136	0.121	-0.070	-0.200	-0.183	-0.278**
	(0.117)	(0.116)	(0.116)	(0.116)	(0.164)	(0.152)	(0.151)	(0.138)
Observations	1,798	1,783	1,783	1,783	3,013	2,970	2,970	2,970
Adjusted R-squared	0.786	0.798	0.800	0.801				
Estimator	OLS	OLS	OLS	OLS	PPML	PPML	PPML	PPML
FE	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.2 reports baseline estimates for the impact of internet on banks' cross-border (CB) export-generating positions. Columns 1-4 and 5-8 report results obtained with, respectively, OLS and PPML estimators. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Trade is flows of trade in manufacturing goods between two countries. Colony, Language, Religion, Currency, Legal and FTA are dummies which equal one if two countries share, respectively, colonial relations, language, religion, currency, legal system and a free trade agreement. Regressions are estimated on a origin-destination dataset obtained by averaging variables over 2010-2019. In all regressions, we exclude bordering and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

[34] We use trade in manufacturing goods and not overall trade to reduce issues of reverse causality, as overall trade includes services in general, and therefore also financial services.

We augment the specification of Column 2 with the number of cable connections between countries. The positive and statistically-significant coefficient of 0.152 suggests that banks provide more cross-border financial services to clients in destinations that share more cable connections with the home country. In addition, when we control for cables, the impact of distance decreases further, with an elasticity that now amounts to -0.676. This evidence suggests that internet may significantly contribute in reducing the role of distance as a barrier for exports of financial services.

We test this hypothesis in column 4, in which we include an interaction term between distance and number of cables. The positive and statistically-significant coefficient of .059 indeed confirms that cables do decrease the negative effect of distance on cross-border exports. By dividing the inverse of the coefficient on distance by this interaction term, we can say that it takes about 12 cables to completely defy distance (0.715/0.059=12.119). This is a large number of connections, considering that, in our sample, only Denmark and Sweden share 12 submarine cables.

In columns 5 to 8 we re-estimate the specifications of columns 1 to 4 with the PPML estimator. Results remain broadly unchanged. However, there are some points worth noting. First, the PPML estimator allows for zero-value cross-border positions.[35] We therefore substitute missing observations with zeros, and as a result the sample size increases from 1,783 to 3,013. Second, and in line with the literature, the PPML regression in column 5 estimates a coefficient on distance that is below unity (e.g. Brei and von Peter, 2018). Third, the PPML regression of columns 7 and 8 estimate a somewhat larger effect of cables. We can see this by considering the interaction coefficient between cables and distance in column 8, which implies that it takes "only" about 6 cables to completely defy distance (0.414/0.074=5.595).

Overall, these results support the hypothesis that internet facilitates banks' direct cross-border operations in destination markets. Moreover, the positive and significant interaction coefficients with distance suggest that the negative impact of distance on cross-border (export-generating) positions decreases as countries are connected with more internet cables. In other words, part of the trade costs associated with distance is due to increased communication costs and internet connections can help reduce such costs. In particular, the models estimate that it takes from 6 to 12 cables to completely defy distance.

We consider columns 3 and 7 as our baseline specifications and we test their robustness in Table 1.3. Results in columns 1-5 and 6-10 are obtained with, respectively, the OLS and the PPML estimator. First, we use a pure export variable as our dependent variables. As mentioned in Appendix 1.B, the USITC statistics include data on cross-border exports of financial and insurance services. Columns 1 and 6 show that the coefficient estimate for cables when using cross-border exports is not statistically significant at conventional levels. The lack of significance could be due to the limitations of export data. For example, with

[35]The dependent variable is no longer log transformed.

export data it is not possible to disaggregate between finance and insurance, or clean data for re-export, or include data post 2016. In different sets of specifications, we find significance at times, depending on whether we include tax havens, use the full dataset, and so on.[36] In general, we find that results are much less robust when we use pure export data compared to data on banks' international positions.

Second, we test the results on cross-border positions with an alternative measure that considers claims to all counterparties from the Locational Banking Statistics of the BIS.[37] The estimates for the cables' coefficient in columns 2 and 7 suggest that results are robust also when we consider this less-precise measure.[38]

Third, we check the robustness of our results by excluding country pairs that could bias the results. Specifically, in columns 3 and 8 we exclude all country pairs that have a tax haven in either origin or destination, as international claims to and from these countries could be unnaturally large.[39] Moreover, in columns 4 and 9 we exclude the two country pairs with United States and United Kingdom, as these two countries share large positions and number of cables. The results show that the findings remain approximately unchanged.

Fourth, and finally, in columns 5 and 10 we include two additional controls used by the literature, namely gaps in GDP per Capita and Tax Rate for corporation (e.g. Andrenelli et al., 2018). The coefficient estimates for cables remain approximately unchanged, while the coefficient estimates for the gaps change with the type of estimator used. The sign of these coefficients cannot be unequivocally interpreted, as we need to apply transformations to be able to include these gaps alongside the origin-destination fixed effects.[40]

1.7 Extensions

Baseline results show that internet cables facilitates cross-border, export-generating positions, thus allowing banks to reach more customers abroad. This is likely the case because internet significantly lowers the costs related to client communication. In this Section, we test further hypotheses that complement our baseline results, namely weaker effects for foreign-affiliate claims, non-linear effects and the impact through time.

[36] Results are available upon request.

[37] Refer to Section 1.4 for an explanation of this alternative measure.

[38] We also test another alternative for cross-border exports, that is sourced from the Consolidated Banking Statistics of the BIS. As explained in the data section, this variable is different from a number of reasons, including the accounting principle of nationality (compared to location). Results with this variable are not statistically significant (not reported). This is expected, as we have shown in Table 1.B.1 in Section 1.B that correlation of the baseline variable with the alternative from the LBS statistics is higher than with the alternative from the CBS statistics. As this latter also correlates significantly less with the export variable from the ITC statistics, these results motivate further our choice of considering the baseline measure from the LBS statistics (rather than the CBS).

[39] We consider tax havens as defined by Oxfam.

[40] Let us consider GDP per capita. We define $k = \frac{GDPPC_d}{GDPPC_o+GDPPC_d}$ and GDPPC Gap $= (1 + (k * ln(k) + (1-k) * ln(1-k)))/ln(2)$.

Table 1.3: Robustness Checks

VARIABLES	(1) ln(CB Exports)	(2) ln(CB Alt.)	(3) ln(CB)	(4) ln(CB)	(5) ln(CB)	(6) CB Exports	(7) CB Alt.	(8) CB	(9) CB	(10) CB
Cables	0.017	0.081**	0.097***	0.147***	0.133***	-0.022	0.095***	0.086***	0.079***	0.136***
	(0.034)	(0.035)	(0.035)	(0.031)	(0.030)	(0.030)	(0.017)	(0.027)	(0.022)	(0.023)
ln(Distance)	-0.585***	-0.821***	-0.616***	-0.678***	-0.630***	-0.635***	-0.142*	0.214	-0.220***	-0.205**
	(0.127)	(0.095)	(0.142)	(0.082)	(0.082)	(0.082)	(0.074)	(0.146)	(0.064)	(0.088)
ln(Trade)	0.563***	0.479***	0.732***	0.427***	0.446***	0.269***	0.497***	1.040***	0.688***	0.508***
	(0.095)	(0.059)	(0.095)	(0.053)	(0.057)	(0.065)	(0.072)	(0.130)	(0.066)	(0.091)
Colony	-1.381*	0.483		0.569**	0.401*	0.189	0.316		0.525**	0.921***
	(0.782)	(0.304)		(0.231)	(0.234)	(0.308)	(0.264)		(0.225)	(0.333)
Language	0.380*	0.413***	0.518***	0.570***	0.640***	0.572***	0.507***	1.306***	0.607***	1.177***
	(0.224)	(0.139)	(0.180)	(0.116)	(0.120)	(0.183)	(0.171)	(0.247)	(0.173)	(0.220)
Religion	0.677***	0.668***	0.657**	0.335*	0.408**	1.103**	1.516***	0.199	0.466	0.627**
	(0.257)	(0.236)	(0.289)	(0.201)	(0.199)	(0.482)	(0.298)	(0.392)	(0.292)	(0.303)
Currency	-0.496**	0.428**	-0.122	0.120	0.089	1.210***	0.431***	0.048	0.082	0.395*
	(0.216)	(0.216)	(0.297)	(0.173)	(0.175)	(0.177)	(0.156)	(0.287)	(0.175)	(0.204)
Legal	0.320**	0.139	0.069	0.145*	0.114	-0.280**	-0.237**	-0.402***	-0.202*	-0.187
	(0.136)	(0.099)	(0.136)	(0.084)	(0.083)	(0.127)	(0.120)	(0.144)	(0.111)	(0.119)
FTA	0.347	0.215	-0.110	0.140	0.177	-0.099	-0.063	-0.393**	0.027	-0.174
	(0.231)	(0.136)	(0.214)	(0.116)	(0.117)	(0.142)	(0.111)	(0.194)	(0.131)	(0.153)
GDPPC Gap					-1.104***					0.198
					(0.280)					(0.741)
CITR Gap					-0.056					0.269***
					(0.058)					(0.095)
Observations	567	1,717	754	1,781	1,639	6,273	3,241	1,382	2,968	2,499
Adjusted R-squared	0.806	0.801	0.827	0.798	0.813					
Check	Exports	CB Alt.	Drop TH	Drop US-UK	Gaps	Exports	CB Alt.	Drop TH	Drop US-UK	Gaps
Estimator	OLS	OLS	OLS	OLS	OLS	PPML	PPML	PPML	PPML	PPML
FE	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.3 reports robustness checks for baseline results in columns 3 and 7 of Table 1.2. Columns 1-5 and 6-10 are obtained with, respectively, OLS and PPML estimators. They report the respective robustness checks: cross-border exports as dependent variable (1 and 6), cross-border positions to all counterparties as dependent variable (2 and 7), exclude tax heavens (3 and 8), exclude US-UK and UK-US country pairs (4 and 9), include GDP Per Capita Gap and Tax-Rate Gap as controls (5 and 10). Cables is the number of cables connecting two countries. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Colony, Language, Religion, Currency, Legal and FTA are dummies which equal one if two countries share, respectively, colonial relations, language, religion, currency, legal system and a free trade agreement. Regressions are estimated on a origin-destination dataset obtained by averaging variables over 2010-2019 and excluding contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

1.7.1 Foreign Affiliates

An increase in internet connections could also lower the costs of within-firm communication, as it allows headquarters to communicate more easily with their foreign affiliates. However, the effect on client communication should be stronger than the effect on within-firm communication. Indeed, while client services like internet banking unequivocally require relatively-fast internet, most of the tasks that a bank's headquarter shares with its affiliates abroad can be coordinated also with alternative means of communication, such as phone calls. We test this hypothesis by comparing the effect of internet on both cross-border and foreign-affiliate positions. We source data on foreign affiliates from the Consolidated Banking Statistics of the BIS and we consider all claims of foreign affiliates to all counterparties. As explained in Section 1.4, there are some important differences between our baseline measure of (export-generating) cross-border positions and this measure for foreign-affiliate positions. Above all, the cross-border variable includes only loan and deposit positions, while the foreign-affiliate variable refers to all claims.[41] With this caveat in mind, we compare the effect of internet connections on cross-border and foreign-affiliate positions by estimating the same type of regressions reported in the baseline section on a limited sub-sample of country pairs, in which observations on both cross-border and foreign-affiliate positions are available.

Table 1.4: Cross Border and Foreign Affiliates

VARIABLES	(1) CB	(2) FA	(3) CB	(4) FA	(5) CB	(6) FA	(7) CB	(8) FA
Panel A: LOG OLS								
ln(Distance)	-1.028***	-1.358***	-0.401***	-0.512*	-0.262*	-0.446	-0.382**	-0.469
	(0.138)	(0.266)	(0.140)	(0.299)	(0.146)	(0.328)	(0.149)	(0.354)
ln(Trade)			0.735***	0.992***	0.725***	0.987***	0.700***	0.982***
			(0.116)	(0.235)	(0.116)	(0.235)	(0.113)	(0.237)
Cables					0.123***	0.059	-0.495***	-0.062
					(0.034)	(0.088)	(0.157)	(0.431)
ln(Distance) × Cables							0.083***	0.016
							(0.022)	(0.053)
Observations	412	412	412	412	412	412	412	412
Adjusted R-squared	0.786	0.532	0.817	0.556	0.822	0.555	0.827	0.554
Panel B: PPML								
ln(Distance)	-0.683***	-0.602***	-0.357***	0.500***	-0.192**	0.503**	-0.414***	0.474**
	(0.068)	(0.220)	(0.090)	(0.194)	(0.086)	(0.197)	(0.102)	(0.205)
ln(Trade)			0.559***	1.626***	0.520***	1.619***	0.471***	1.608***
			(0.096)	(0.183)	(0.087)	(0.183)	(0.080)	(0.182)
Cables					0.138***	0.006	-0.428***	-0.058
					(0.023)	(0.037)	(0.134)	(0.202)
ln(Distance) × Cables							0.074***	0.008
							(0.018)	(0.025)
Observations	3,013	2,616	2,970	2,616	2,970	2,616	2,970	2,616
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.4 reports estimates for the impact of internet on banks' cross-border (CB) and foreign-affiliate (FA) positions. Panel A and B show results obtained with, respectively, OLS and PPML estimator. OLS and PPML estimators use, respectively, the natural logarithm and raw values of dependent variables. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Controls for colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions (coefficient estimates are not reported). Regressions are estimated on a origin-destination dataset obtained by averaging variables over 2010-2019 and excluding contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

A summary of the results is reported in Table 1.4. Panel A and B outline estimates obtained with, respectively, the OLS and the PPML estimators. Odd- and even- numbered columns show estimates when we include, respectively, cross-border and foreign-affiliate positions as the dependent variable of interest. All regressions include the full set of controls and estimates of related coefficients can be found in Tables 1.C.1 and 1.C.2 in Appendix 1.C.

[41] The publicly-available version of the Consolidated Banking Statistics do not allow further disaggregation of claims of foreign affiliates.

The logic of the order of the columns is the same of the one for the baseline table. We start by estimating a simple model with distance. Column 1 shows that the effect of distance on cross-border positions in this sub-sample is in line with the baseline results. Furthermore, column 2 shows that distance plays a large role also in explaining claims of foreign affiliates. However, the explanatory power of distance seems to be lower for foreign-affiliate positions than it is for cross-border positions, as the adjusted R squared for the OLS regressions is lower in column 2 (0.532) than in column 1 (0.786). The fact that the predictive power of standard gravity variables, like distance, is lower for sales of foreign affiliates than it is for direct cross-border sales is in line with the findings of papers comparing different modes of exports (e.g. Andrenelli et al., 2018). Intuitively, distance matters more when banks in the home country need to directly reach clients abroad, while it does not matter much if there is a foreign affiliate in the destination country that communicates with clients and runs the business.[42] In addition, this aspect is in line with the findings of Galema and Koetter (2018), who analyse a sample of German banks and highlight that unobservable bank traits can explain a large share of the variation in foreign affiliate operations. Obviously, in our case these unobservable bank traits must vary across country pairs, as we are controlling for both origin and destination fixed effects. For example, these unobservables can be differences in business models between banking groups that own foreign affiliates in different destinations.[43] Columns 3 and 4 show that, as in the baseline, the effect of distance decreases when we control for manufacturing trade. This reduction is even stronger for foreign-affiliate positions, as the coefficient becomes significantly less negative with OLS (-0.512) and it turns even positive with PPML (0.500).

In columns 5 and 6 we control for internet connections. Column 5 shows that the results on cross-border positions in this sub-sample are in line with the baseline sample, with a lower coefficient on distance and a positive coefficient on cables. Differently, column 6 shows that the positive impact of cables' connections on foreign-affiliate positions is not statistically significant. This result would therefore suggest that internet cables are not essential to decrease costs of within-firm communication between headquarters and affiliates. This could be the case as foreign affiliates may be able to pass on information to the parent concerning credit and investment decisions also without fast internet. Indeed, the decisions on loans and long-term portfolio management do not need immediate response, and alternative communication methods, such as phone calls and emails, may be enough. Furthermore, we may not register any effect as we cannot disentangle between routine and non-routine activities, for example deposits vis-'a-vis short-term portfolio-management operations. While

[42]Note that the change in the sample size between columns 1 and 2 of Panel B is only due to the way fixed effects are handled with the PPML estimator, as some observations are dropped due to collinearity or presence of singletons.

[43]The fixed effects control for all the observable and unobservable country-specific factors determining foreign-affiliate activities, such as profitability, riskiness, size, banking regulation and crises (e.g. Temesvary, 2018).

we cannot rule out this possibility, we argue that the relationship between cables and foreign positions is stronger for cross-border than for foreign-affiliate positions.
Finally, in columns 7 and 8 we confirm this logic by looking at the interactions between distance and cables. While the cables decrease the effect of distance for cross-border positions (positive and significant estimates in column 7), this is not the case for foreign-affiliate claims (positive but insignificant estimates in column 8).

1.7.2 Non-Linearity of Cables

The baseline results showed that cable connections significantly boost banks' lending and deposit services across borders. It is interesting to assess whether most of this effect is driven by the first cable connection or if multiple connections also play a role. In other words, is the effect of cables on cross-border positions non linear? We address this question in Table 1.5. In our baseline sample, most countries have up to 8 cable connections between each other. There are only two countries that over the period 2010-2019 had more than 8 connections between each other, namely Denmark and Sweden - they have up to 12 connections. We therefore estimate all regressions with non-linear terms both in the full sample (odd-numbered columns) and in a reduced sample in which we exclude the two country pairs Denmark-Sweden and Sweden-Denmark (even-numbered columns). In addition, all specifications in Table 1.5 include the usual set of controls, which are not reported here.[44] For this analysis, we report results with the OLS estimators only.
We start by augmenting the baseline regressions with the square of the cable variable. The negative and statistically coefficient of the squared term (-0.020) of column 1 suggests that the positive effect of cables on cross-border positions decreases as the number of cables increases. In addition, the larger estimate of column 2 (-0.053) suggests that this non-linearity is even stronger when we exclude the Denmark-Sweden pairs. In columns 3 and 4 we explore this non linearity with an alternative method that relies on binary variables which equal 1 for country pairs with a specific number of cable connections and 0 otherwise. As we include binary variables for all categories but the countries with 1 cable connection, we can interpret the coefficients of these binary variables as the difference between their identifying group of countries and the group of countries with 1 connection. The negative coefficient for $Cables = 0$ in columns 3 and 4 therefore suggest that banks' cross-border positions are significantly lower between countries with no cable connection compared to countries with one cable connection. On the other hand, the statistically insignificant coefficients of the other binary variables suggest that cross-border positions between countries that share from 2 to 8 cable connections are no different from the ones of countries that share 1 connection. Differently, the positive and statistically significant coefficient of $Cables = 12$ in column 3

[44]Full results are reported in Table 1.C.3

suggest that countries with 12 connections have more cross-border positions than countries with 1 connection. This effect is fully driven by the Denmark-Sweden pairs.

Table 1.5: Non Linearity of Cables

VARIABLES	(1) ln(CB)	(2) ln(CB)	(3) ln(CB)	(4) ln(CB)	(5) ln(CB)	(6) ln(CB)	(7) ln(CB)	(8) ln(CB)
Cables	0.272***	0.397***						
	(0.063)	(0.083)						
Cables^2	-0.020**	-0.053***						
	(0.009)	(0.018)						
Cables = 0			-0.581***	-0.581***				
			(0.100)	(0.100)				
Cables = 2			0.144	0.144				
			(0.166)	(0.166)				
Cables = 3			-0.059	-0.059				
			(0.189)	(0.189)				
Cables = 4			-0.459*	-0.459*				
			(0.262)	(0.262)				
Cables = 5			-0.108	-0.108				
			(0.335)	(0.335)				
Cables = 6			0.479	0.479				
			(0.481)	(0.481)				
Cables = 7			0.298	0.298				
			(0.611)	(0.610)				
Cables = 8			-0.363	-0.364				
			(1.015)	(1.015)				
Cables = 12			1.071***					
			(0.289)					
Cables >= 1					0.596***	0.586***	0.583***	0.579***
					(0.088)	(0.088)	(0.098)	(0.098)
Cables >= 2							0.036	0.021
							(0.125)	(0.126)
ln(Distance)	-0.670***	-0.679***	-0.693***	-0.693***	-0.680***	-0.678***	-0.678***	-0.676***
	(0.082)	(0.081)	(0.082)	(0.082)	(0.080)	(0.080)	(0.081)	(0.081)
ln(Trade)	0.420***	0.409***	0.398***	0.398***	0.403***	0.402***	0.403***	0.402***
	(0.053)	(0.053)	(0.053)	(0.053)	(0.052)	(0.052)	(0.053)	(0.053)
Observations	1,783	1,781	1,783	1,781	1,783	1,781	1,783	1,781
Adjusted R-squared	0.801	0.801	0.802	0.802	0.803	0.802	0.803	0.802
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Specification	Full Sample	Drop DK-SE	Full Sample	Drop DK-SE	Full Sample	Drop DK-SE	Full Sample	Drop DK-SE
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.5 reports regression results testing the non-linearity of cables. Odd- and even- numbered columns show the results for, respectively, the full sample of country pairs and the sub-sample excluding country pairs Denmark-Sweden and Sweden-Denmark (which have 12 connections). Columns 1 and 2 report results for the squared term of cables. Columns 3 and 4 compare categories of number of connections to the baseline category with 1 connection. Columns 5-6 and 7-8 show results with binary variables which equal one for country pairs with at least, respectively, 1 and 2 connections. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Controls for colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions (coefficient estimates are not reported). Regressions are estimated on a origin-destination dataset obtained by averaging variables over 2010-2019 and excluding contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Overall, results of columns 3 and 4 confirm the high non-linearity of the effect of cables on cross-border positions detected in columns 1 and 2, and suggest that most of the effect comes with the first connection. In columns 5 and 6 we test this first-connection hypothesis by including a dummy which compares country pairs with at least 1 connection to country pairs with no connection. When we run this comparison, the model estimates a strong difference in the effect when we move from countries with no cable connections to countries with at least one connection. The slightly more conservative estimate of column 6 is more reliable, as it excludes Denmark-Sweden pairs. Finally, in columns 7 and 8 we augment regressions on columns 5 and 6 with an added dummy that equals 1 for pairs with at least 2 connections. The related coefficients are not statistically significant, which indicates that, after controlling for the impact of having at least 1 cable connection, there is no differential impact of having 2 or more connections.

Overall, results in Table 1.5 show that the effect of cables on banks' cross-border positions is highly non linear, and it passes mostly through the first connection. In other words, once the first connection is in place, extra cables do not imply larger trade in financial services. While this is true for cables per se, Table 1.2 showed that the negative effect of distance disappears only when countries have between 6 and 12 connections. We explore more in details the non-linearity between cables and distance in Table 1.C.4 in Appendix 1.C, where we run regressions similar to Table 1.5. The results confirm that one connection is not enough to significantly reduce the negative effect of distance on banks' cross-border positions. Specifically, they suggest that this negative effect starts decreasing only when countries have at least 5 connections. Taken together, Tables 1.5 and 1.C.4 provide a nuanced picture of the non-linear relationship between cables connections and banks' cross-border positions. While most of the enhancing effect of cables passes through the first connection, extra connections contribute in decreasing the barrier of distance, which still matters significantly in international banking.

1.7.3 Impact over Time

So far we have focused only on aggregated data over 2010-2019, as it is reasonable to believe that fast internet and services like home banking became widely used mainly in recent years. We can test this hypothesis by exploiting the length of the Locational Banking Statistics, which go back to 1977. Data on internet cables from Telegeography are available from 1989, when the first fiber-optic cables were installed. When merging these datasets, we have a reasonable amount of observations from 1995 onwards.[45] We thus run the same baseline regressions of Table 1.2 by year over 1995-2019 on a sub-sample of country-pairs for which observations in 1995 are available.[46] We limit the analysis to this sub-sample in order to make results comparable across years.

Table 1.6 summarises the OLS and PPML estimates for the coefficients on cables by each year. All the regressions include the usual set of controls, including distance and trade. Estimates for all coefficients for selected years are reported in Tables 1.C.5 and 1.C.6 in

[45]This is the case because we use also the OLS estimator, which does not allow zero values in the left hand side. In addition, we are focusing only on loans and deposits to non-banks, which is a high disaggregation, and related data in the LBS by the BIS became available little by little over time.

[46]We thus estimate 60 regressions (30 years times 2, as we estimate both OLS and PPML regressions.)

Table 1.6: Impact by Years

	OLS		PPML	
	(1)	(2)	(3)	(4)
Year	Coeff.	P-Value	Coeff.	P-Value
1995	.013	.931	.035	.755
1996	.059	.496	.048	.487
1997	.179	.025	.155	.027
1998	.137	.053	.158	.012
1999	.200	.018	.103	.060
2000	.140	.015	.166	.001
2001	.112	.032	.097	.002
2002	.118	.035	.086	.020
2003	.118	.032	.103	.001
2004	.188	.001	.140	0
2005	.140	.005	.152	0
2006	.121	.026	.147	0
2007	.162	.011	.152	0
2008	.174	.004	.141	0
2009	.157	.005	.187	0
2010	.167	.002	.207	0
2011	.162	.002	.213	0
2012	.186	0	.172	0
2013	.152	.001	.146	0
2014	.168	0	.149	0
2015	.165	0	.158	0
2016	.159	0	.127	0
2017	.158	0	.130	0
2018	.164	0	.100	.003
2019	.164	0	.120	0

Notes: Table 1.6 reports results for OLS regressions (columns 1-2) and PPML regressions (columns 3-4) estimated by year over 1995-2019. The dependent variable is export-generating positions (log transformed for OLS regressions). Values in columns 1 and 3 are coefficient estimates for the number of cables, while value in columns 2 and 4 are the respective p-values. Controls for distance, manufacturing trade, colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions (coefficient estimates are not reported). All regressions are estimated on a origin-destination sub-sample of country pairs that have information available from 1995 onwards (around 450 country pairs). All regressions exclude contiguous and landlocked countries and standard errors are clustered by country pairs.

Appendix 1.C. Columns 1 and 3 report the coefficient estimates, while columns 2 and 4 report the related p-values. P-values below .1, .05 and .01 indicate statistical significance at the, respectively, at the 10%, 5% and 1% level. The coefficient estimates and p-values for both OLS and PPML estimators for both the years 1995 and 1996 suggest the effect of internet connections on banks' cross-border positions was positive but very low, and therefore not statistically significant at conventional levels. Then both OLS and PPML coefficients start to be statistically significant at the 5% level in 1997. Furthermore, both estimators detect statistical significance at the 1% level from 2004 onwards.[47] Finally, the three-decimal p-values of both estimators can be rounded to zero from 2012 onwards.[48] Overall, if we consider results from both OLS and PPML estimators, the effect of cable

[47]After 2000, PPML coefficients are significant above 1% only in 2002. After 2004, OLS coefficients are significant above 1% only in 2006 and 2007.

[48]This is the case for all years but for 2013 for the OLS estimator and 2018 for the PPML estimator. For the PPML estimator, three-decimal p-values can be rounded to zero already from 2004 onwards.

connections on banks' cross-border positions became quite significant after 2004, and strongly significant from 2012 (or 2014) onwards. This evidence suggests that banks started to use the internet to provide credit and deposit services to clients in foreign countries already in the 2000s. This trend became stronger in recent years, possibly thanks to the development of internet-banking technologies.

1.8 Endogeneity

In principle, gravity models correlating trade in services with measures of internet connectivity between countries can be subject to reverse causality and omitted variable bias. Indeed, countries may decide to invest in fast-internet connection specifically because they trade a lot in services. This should be less of an issue when considering a specific type of services trade, especially since cross-border financial services constitute a very small share of most countries' trade in services. Also considering the large cost of submarine cables, it is less likely that countries would decide to connect via these cables only to facilitate trade in financial services. Moreover, controlling for bilateral trade in goods captures a wide range of factors that could determine both trade in financial services and internet cable connections. Nonetheless, we test the robustness of our baseline results with two identification strategies.

1.8.1 Panel Data and Lags

Our first identification strategy exploits the panel structure of the Locational Banking Statistics to control for a wide range of possible omitted variables and use a lagged value of the number of internet cables to alleviate any potential reverse causality issues. We use a sample of observations on banks' cross-border positions and cables' connections over 1990-2019 and we report the results in Table 1.7. Panel A and Panel B include the coefficient estimates when we use, respectively, contemporaneous and lagged versions of the cables' variable and other time-varying controls.

Columns 1-3 and columns 4-6 show results for, respectively, OLS and PPML estimators. For the latter, we use the extended sample in which we substitute missing observations with zeroes. We differentiate between three different sets of fixed effects, namely origin, destination and year (columns 1 and 4), origin-year and destination-year (columns 2 and 5), and origin-year, destination-year, and origin-destination (columns 3 and 6). With the last set of fixed effects, we identify our results only on within-pair variation over time, and we thus control for potential omitted factors that make countries both more likely to connect and to trade in financial services. So the only potential confounders would need to vary across pairs over time.

When we include cables without lag in Panel A, the OLS results show that cables have a

Table 1.7: Panel

VARIABLES	(1) ln(CB)	(2) ln(CB)	(3) ln(CB)	(4) CB	(5) CB	(6) CB
Panel A: Baseline						
Cables ($l = 0$)	0.172***	0.156***	0.045	0.148***	0.141***	0.114***
	(0.026)	(0.029)	(0.034)	(0.020)	(0.021)	(0.039)
ln(Trade) ($l = 0$)	0.456***	0.499***	0.031	0.485***	0.578***	0.096**
	(0.034)	(0.043)	(0.028)	(0.061)	(0.064)	(0.038)
Currency ($l = 0$)	-0.066	-0.248	0.063	0.351*	0.362*	0.251*
	(0.140)	(0.164)	(0.169)	(0.189)	(0.195)	(0.150)
FTA ($l = 0$)	0.116*	0.155*	-0.040	-0.039	-0.094	-0.093
	(0.069)	(0.092)	(0.083)	(0.103)	(0.119)	(0.109)
ln(Distance)	-0.598***	-0.618***		-0.107	-0.045	
	(0.063)	(0.072)		(0.073)	(0.078)	
Colony	0.521**	0.534**		1.251***	1.107***	
	(0.235)	(0.252)		(0.363)	(0.363)	
Language	0.523***	0.513***		1.211***	1.119***	
	(0.097)	(0.103)		(0.183)	(0.174)	
Religion	0.382**	0.424**		0.820***	0.730**	
	(0.162)	(0.169)		(0.291)	(0.295)	
Legal	0.079	0.057		-0.284***	-0.264***	
	(0.071)	(0.074)		(0.098)	(0.093)	
Observations	20,477	20,282	20,158	66,185	42,947	28,952
Adjusted R-squared	0.778	0.784	0.924			
Panel B: Lag						
Cables ($l = 1$)	0.175***	0.159***	0.060*	0.145***	0.143***	0.126***
	(0.026)	(0.029)	(0.033)	(0.021)	(0.020)	(0.038)
ln(Trade) ($l = 1$)	0.452***	0.478***	0.050*	0.491***	0.572***	0.090**
	(0.034)	(0.042)	(0.027)	(0.062)	(0.065)	(0.040)
Currency ($l = 1$)	-0.061	-0.212	0.040	0.342*	0.352*	0.167
	(0.136)	(0.162)	(0.176)	(0.186)	(0.188)	(0.146)
FTA ($l = 1$)	0.107	0.145	-0.038	-0.057	-0.109	-0.095
	(0.068)	(0.091)	(0.079)	(0.105)	(0.119)	(0.096)
ln(Distance)	-0.618***	-0.644***		-0.122*	-0.058	
	(0.062)	(0.071)		(0.072)	(0.077)	
Colony	0.573**	0.585**		1.220***	1.096***	
	(0.235)	(0.252)		(0.357)	(0.355)	
Language	0.549***	0.542***		1.208***	1.105***	
	(0.095)	(0.101)		(0.189)	(0.181)	
Religion	0.399**	0.435***		0.850***	0.755***	
	(0.160)	(0.167)		(0.287)	(0.288)	
Legal	0.080	0.065		-0.279***	-0.249***	
	(0.070)	(0.074)		(0.098)	(0.094)	
Observations	21,424	21,232	21,120	66,185	44,849	30,243
Adjusted R-squared	0.779	0.784	0.924			
Estimator	OLS	OLS	OLS	PPML	PPML	PPML
FE	o-d-t	ot-dt	ot-dt-od	o-d-t	ot-dt	ot-dt-od

Notes: Table 1.7 reports estimates for the impact of internet on banks' cross-border (CB) export generating positions for a panel of country pairs over 1990-2019. Cables enter with no lag in Panel A, and with one lag in Panel B. Columns 1-3 and 4-6 report results obtained with, respectively, OLS and PPML estimators. Results in columns 1 and 4, 2 and 5, and 3 and 6, are obtained by including, respectively, origin and destination, origin-time and destination-time, and origin-time and destination-time and origin-destination fixed effects. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Colony, Language, Religion, Currency, Legal and FTA are dummies which equal one if two countries share, respectively, colonial relations, language, religion, currency, legal system and a free trade agreement. In all regressions, we exclude bordering and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level.

positive and significant effect on positions (columns 1-2), but this significance goes away when we include the more stringent fixed effects (column 3). On the other hand, the PPML results are robust with all three sets of fixed effects (columns 4-6). Also, note that the coefficient for distance is no longer statistically significant with the PPML estimators with the first two sets of fixed effects.[49].

Panel B show the results when we include the first lag of cables and of the other variables that can vary through time, namely manufacturing trade and the dummy variables indicating whether countries share the same currency and a free trade agreement. Results are broadly in line with Panel A. The effect of lagged cables is slightly larger, as now the OLS estimates with the third set of fixed effects are statistically significant at the 10-% level.

1.8.2 Routing

Our second identification strategy is based on the idea of "routing". Generally, connection networks are financed by investors with an interest in connecting two specific points in space (cities, regions, and so on). As the distance between two specific points maybe large, these networks usually need to pass through locations in between the two points of interest, i.e. so called routing locations. These routing locations will therefore get connected to the network only because they happen to be on the shortest distance between the two points of interest, and not because of a specific interest by the investors. The literature has exploited this random assignment of connectivity in different ways. For example, Fajgelbaum and Redding (2014) study how, among other things, investment in railways can boost local productivity in Argentina in the 19th century. To identify the causal impact, they consider stations at cities that happened to be on the shortest route between larger centers.[50] In this paper, we borrow from Haltenhof (2019), who estimates the impact of submarine cables on trade in services in general and applies the concept of routing to the cable network. Specifically, the author points out that most of the submarine cables connecting West Europe (WE) to East Asia (EA) have been financed by countries in these areas. These cables usually start from Germany, United Kingdom or France, pass through the Suez canal, cross the Indian Ocean and finally arrive in Japan, South Korea or Australia.[51] Given the distance covered, these cables have lending points in Middle-East and West-Asia countries that just happen to be on the cables' route. Haltenhof (2019) identifies 15 of these countries, and consider a subsample of trade flows between these countries (routing countries) and WE and EA countries.[52]

[49]Note that the coefficient of distance and other time-invariant controls cannot be estimated when we include origin-destination fixed effects

[50]The authors focus on the large investments aimed at connecting district centers to highly-populated Spanish colonial towns serving the mining region of Upper Peru. In their context, all cities on the shortest route between district centers and these colonial towns were disproportionately likely to be connected to the railway network, regardless of their unobserved characteristics. Other authors who use similar identification strategies based on routing are Chandra and Thompson (2000), Michaels (2008) and Donaldson (2018).

[51]The cables considered by the author are 4, namely Flag Europe-Asia (FEA), SEA-ME-WE 3, SEA-ME-WE 4 and IMEWE. More information can be found on the website.

[52]These 15 countries are Bangladesh, Djibouti, Egypt, India, Jordan, Lebanon, Malaysia, Morocco, Pakistan, Philippines, Saudi Arabia, South Africa, Thailand, Tunisia, United Arab Emirates. As an alternative subsample, Haltenhof (2019) considers all trade flows between Egypt and WE and EA countries,

Table 1.8: Routing Subsample

VARIABLES	(1) ln(CB)	(2) ln(CB)	(3) CB	(4) CB
Cables Dummy	0.398**		0.314**	
	(0.179)		(0.131)	
Cables Dummy Routing		0.356*		0.322**
		(0.204)		(0.139)
log(Distance)	-1.124***	-1.130***	-0.782***	-0.775***
	(0.183)	(0.185)	(0.128)	(0.127)
ln(Trade)	0.346***	0.355***	0.579***	0.587***
	(0.095)	(0.095)	(0.069)	(0.068)
Colony	-0.456	-0.469	-0.371	-0.367
	(0.326)	(0.319)	(0.248)	(0.248)
Language	0.441***	0.442***	0.806***	0.796***
	(0.169)	(0.168)	(0.140)	(0.142)
Religion	-0.092	-0.074	1.195***	1.176**
	(0.599)	(0.600)	(0.450)	(0.457)
Currency	-0.509	-0.519	-1.440***	-1.440***
	(0.664)	(0.666)	(0.338)	(0.337)
Legal	0.247*	0.245*	0.197**	0.194**
	(0.133)	(0.133)	(0.086)	(0.085)
FTA	-0.174	-0.173	0.222	0.212
	(0.218)	(0.218)	(0.231)	(0.230)
Observations	4,410	4,410	6,574	6,574
Adjusted R-squared	0.778	0.777		
Estimator	OLS	OLS	PPML	PPML
FE	ot-dt	ot-dt	ot-dt	ot-dt

Notes: Table 1.8 reports regression results for a subsample with routing cables. Results are obtained with a subsample of cross-border export generating positions of 15 origin countries in Europe and Eastern Asia, plus Philippines, and 30 destination countries situated on the shortest sea route between Europe and Eastern Asia (from the Suez Canal to the Indian Ocean). For this sub-sample, we consider a country-pair panel over 1990-2019. Columns 1-2 and 3-4 report results obtained with, respectively, OLS and PPML estimators. Cables Dummy is a dummy which equals 0 if country pairs do not share a cable connection and 1 if they do (one cable or more). Cables Dummy Routing is a dummy which equals 0 if country pairs do not share a cable connection with one of the 4 cables FLAG Europe-Asia (FEA), SeaMeWe-3, SeaMeWe-4, IMEWE and 1 if they do. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Colony, Language, Religion, Currency, Legal and FTA are dummies which equal one if two countries share, respectively, colonial relations, language, religion, currency, legal system and a free trade agreement. In all regressions, we exclude bordering and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

We consider a similar strategy to an extended sub-sample of countries in order to maximise the number of observations available in the Locational Banking Statistics. Specifically, we consider the 15 routing countries identified by Haltenhof (2019), excluding South Africa, and we add other 16 countries that are on the shortest sea route between West Europe and East Asia.[53] The final subsample in the Locational Banking Statistics include 15 origin countries and 30 destination countries.[54]

as the Suez canal is generally the passing point of all cables connecting Europe and Asia.

[53]These countries are Algeria, Cambodia, Eritrea, Indonesia, Israel, Libya, Myanmar, Oman, Singapore, Somalia, Sri Lanka, Sudan, Syria, Turkey, Vietnam and Yemen.

[54]Among the 15 countries of origin, there are 14 countries in the between WE and EA plus Philippines. These 14 WE-EA countries are Australia, Belgium, Denmark, Finland, France, Hong Kong, Ireland, Italy, South Korea, Macao, Netherlands, Philippines, Spain, Sweden and United Kingdom. When Philippines,

This analysis has two main other differences with the baseline. First, in order to maximise the sample size and variation of cables' connections, we run regressions on a panel, with origin-time and destination-time fixed effects.[55] Second, in order to give power to our explanatory variable for cables, we use dummies which equal 1 for countries with at least 1 cable and zero otherwise. Furthermore, we use an alternative for this variable which equals 1 for countries with at least one connection with the 4 cables mentioned by Haltenhof (2019) and zero otherwise. We do so to isolate the effects of the cables that are most likely routing cables for the considered routing countries.

Table 1.8 reports the results. Columns 1-2 and 3-4 show estimates obtained with, respectively, the OLS and the PPML estimator. The coefficient estimates for both versions of cables' dummies are positive and significant. In addition, the size of coefficients is generally larger than in the full-sample regressions reported in columns 2 and 5 of panel regressions in Table 1.7. The coefficients of the rest of the controls are generally in line with these baseline panel regressions, with the difference that the negative coefficient on currency is now also statistically significant. Overall, we estimate a somewhat larger effect of connections on banks' cross-border positions when we consider a sub-sample with routing countries and routing cables.

1.9 Channels

In this Section we examine potential mechanisms that could partially drive the baseline results on cross-border positions. Specifically, we shall consider cross-border loans and deposits separately. One of the many advantages of using the LBS statistics is that we can disentangle these two types of export-generating positions, which would not be possible with any other publicly available dataset on either international positions or exports. As explained in Section 1.4, we do this disaggregation by focusing only on loan and deposit positions to non-banks. We therefore assume that, under this classification, claims are mostly loans to foreign households and corporations, and liabilities are mostly deposits by foreign households and corporations.[56] We can thus estimate the gravity model with cross-border positions on loans and deposits as dependent variables.

Table 1.9 reports the results. For simplicity, we report only the results obtained with the PPML estimator, while the results obtained with the OLS are reported in Appendix 1.D. Columns 1 and 2 show that loans and deposits are larger between countries that are better

which is a routing sample, is the origin, the destinations are these 14 WE-EA countries only. Finally, all the 30 destinations are routing countries (Philippines is the only routing country to have data available as origin).

[55] There is not enough variation in this smaller sample to include origin-time, destination-time and origin-destination fixed effects at the same time.

[56] We exclude all bank-to-bank positions. If we included these positions, claims could include deposits of home banks at foreign banks, and liabilities could include loans by foreign banks to home banks.

Table 1.9: Channels

VARIABLES	(1) Loans	(2) Deposits	(3) Loans	(4) Deposits	(5) Loans	(6) Deposits
Cables	0.155***	0.098***	0.149***	0.087***	0.477***	0.448***
	(0.021)	(0.030)	(0.023)	(0.033)	(0.113)	(0.115)
Cables × Emerging Dest.			0.052	0.109*	-0.017	-0.015
			(0.047)	(0.058)	(0.057)	(0.072)
Cables × Assets Dest.					-0.001***	-0.002**
					(0.001)	(0.001)
Cables × Z-Score Dest.					-0.008***	-0.010***
					(0.003)	(0.003)
ln(Distance)	-0.151	-0.273***	-0.154	-0.287***	-0.113	-0.309***
	(0.097)	(0.103)	(0.095)	(0.104)	(0.103)	(0.098)
ln(Trade)	0.537***	0.383***	0.531***	0.354***	0.610***	0.427***
	(0.088)	(0.114)	(0.089)	(0.119)	(0.089)	(0.108)
Colony	0.964***	1.089***	1.017***	1.158***	0.937***	0.933**
	(0.339)	(0.354)	(0.347)	(0.366)	(0.340)	(0.368)
Language	1.269***	0.967***	1.274***	1.004***	1.229***	0.945***
	(0.216)	(0.205)	(0.214)	(0.209)	(0.204)	(0.229)
Religion	0.663**	0.900**	0.702**	0.955**	0.693**	0.847*
	(0.307)	(0.426)	(0.307)	(0.432)	(0.304)	(0.444)
Currency	0.075	0.723***	0.043	0.674***	0.306	1.036***
	(0.189)	(0.227)	(0.193)	(0.237)	(0.210)	(0.256)
Legal	-0.233*	-0.250*	-0.222*	-0.227*	-0.231*	-0.276*
	(0.124)	(0.134)	(0.120)	(0.133)	(0.121)	(0.142)
FTA	-0.215	-0.200	-0.225	-0.199	-0.256	-0.401**
	(0.167)	(0.181)	(0.166)	(0.178)	(0.182)	(0.189)
Observations	2,970	2,970	2,970	2,970	2,800	2,800
FE	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.9 reports estimates for the channels of the impact of internet on banks' cross-border loans (odd-numbered columns) and deposits (even-numbered columns) to non banks. All results are obtained with the PPML estimator using an origin-destination dataset, obtained by averaging variables over 2010-2019. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Emerging Dest. is a dummy which equals 1 when destination countries are emerging economies. Assets Dest. is the total amounts of assets of the banking sector in destination economies (size of the banking sector). Z-Score is the Z-score of the banking sector in destination countries, defined as (ROA+(equity/assets))/sd(ROA) (stability of the banking sector). Trade is flows of manufacturing trade between two countries. Colony, Language, Religion, Currency, Legal and FTA are dummies which equal one if two countries share, respectively, colonial relations, language, religion, currency, legal system and a free trade agreement. In all regressions, we exclude bordering and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

connected through internet cables. Intuitively, individuals and corporations use the internet to manage both accounts (deposits) and credit lines (loans) with foreign banks. Furthermore, the coefficient on distance for loans is no longer statistically significant at conventional levels. In columns 3 and 4 we address whether the effect is stronger for clients located in emerging economies. For both types of positions we could expect a "North-South" effect. For example, individuals in emerging countries may prefer to deposit their savings at banks in developed economies as they are generally safer than local banks. On the contrary, individuals in developed economies do not necessarily need to do so, as their home banking system is sound. Similarly, large corporations in emerging markets may prefer to get a loan from a foreign bank located in developed economies because interest rates in these economies are lower than the local ones. This arbitrage is not possible for corporations in developed economies, as they would get approximately the same rates they get at home. However, it is also true that banks located in developed economies may be more inclined to lend to large corporations in other developed economies (North-North), rather than in emerging markets (North-South), which may have different regulations on corporations. For example, US banks may find it easier to lend to large UK corporations, rather than large corporations in the Philippines, because UK loans are more similar to American laws and there is less uncertainty on monitoring. The aspect of monitoring may not be as important for deposits, as in this case banks are the borrower rather than the creditor. Overall, the impact of internet on both cross-border loans and deposits may be be stronger for North-South positions, especially for deposits.

Among the 16 countries of origin that report this type of data to the BIS, at least 14 of them can be defined as advanced economies. On the other hand, the destination countries include both developed and emerging economies. We can therefore use a dummy which equals 1 for destinations that are emerging economies to capture (mostly) North-North and North-South dynamics.[57] Columns 3 and 4 report estimation results for model augmented with the interaction term between this dummy and cables. First of all, the coefficients for cables as individual variables estimate the impact for North-North observations. The impact is positive and significant for both loans (.149) and deposits (.087). This evidence suggests that corporations (and individuals) in developed countries use the internet to manage credit lines with foreign banks from other developed economies. In this case, the value for North-North lending may come from loans in foreign currencies. At the same time, individuals (and corporations) in developed economies use the internet to open deposits with foreign banks in other developed economies. This logic could be in line with a story on tax heavens. For example, individuals and firms in developed countries could use the internet to open a bank account in Malta and deposit savings there to avoid taxes.

Second, the coefficients for the interaction terms estimate whether the impact of internet on loans and deposits is larger for North-South observations than it is for North-North observations. The positive but insignificant coefficient of .052 in column 3 indicates that the relationship is not stronger for North-South lending positions. This result may be due to the two discussed opposing channels. On the one hand, banks in developed countries use the internet to gain market shares in emerging economies through direct lending. On the other hand, interactions via the internet may not be enough to overcome monitoring concerns in North-South lending relationships. Furthermore, it could also be that the

[57] South-North and South-South observations are very few.

emerging-market dummy is not measuring some important characteristics of the banking sector in the destination country.

Considering deposits, the positive and significant coefficient in column 4 of 0.109 suggests that individuals in emerging countries use the internet to open accounts with foreign banks located in developed countries more than individuals in developed countries. This finding is in line with our expectations. Individuals in emerging countries prefer to deposit their money at larger and sounder banks in developed countries, and use the internet to open these accounts.

To test the more specific channels on the size and stability of the banking sectors, in columns 5 and 6 we augment these regressions with the measures for overall assets and Z-Score of banks in destination countries. The amount of assets is a proxy for the size of the banking sector. The Z-Score, defined as the sum of Return on Assets (ROA) and the ratio of Equity over Assets over the standard deviation of ROA, is a measure for the stability of the banking sector. High values means that the banking sector is more stable, as returns are large and not volatile, and equity levels are high. The coefficient estimates for both interaction terms are negative and statistically significant, and this is the case for both loan and deposit positions. It follows that the effect of cable connections on both cross-border loans and deposits is lower when the banking sector in destination countries is larger and more stable. At the same time, the coefficients for the interaction terms with the emerging-economy dummy become statistically insignificant. Overall, most of the effect detected for emerging markets is passing through the characteristics of the banking sectors in destination economies. Firms and individuals in countries with small and unstable banking sectors use the internet to open lines of credit and deposits with banks abroad, and they do it significantly more than firms and individuals in countries with large and stable banking sectors.[58]

1.10 Conclusions

Trade in financial services has been so far dominated by sales of foreign affiliates, but cross border trade is catching up. In this paper we show that the expansion of internet connectivity and the ensuing decline in communication costs has contributed to this trend.

Our analysis is based on data collected by the BIS on banks' international positions. Data on bilateral trade in services have notoriously poor coverage and are reported with a substantial lag. We propose that a subset of banks' international positions is a good proxy for exports of financial services and therefore superior because of its broad coverage and timeliness.

In particular, we show that cable connections across countries boost banks' cross-border positions. On the other hand, we do not find any such relationship for positions of foreign

[58]In Table 1.D.1 in Appendix 1.D, we show that when we use the OLS estimators results remain broadly unchanged, but for the coefficients on the Z-Score, which lose significance.

affiliates. These results are in line with previous literature which argued that financial services are especially intensive in client communication and therefore would predict that a decline in communication costs boosts cross-border trade more than trade through foreign affiliates. In addition, we show that the positive effect of cables on banks' cross-border positions comes mainly through the first cable connection. However, we find that there are still positive spillovers in having multiple connections, as the negative effect of distance decreases with the number of cables.

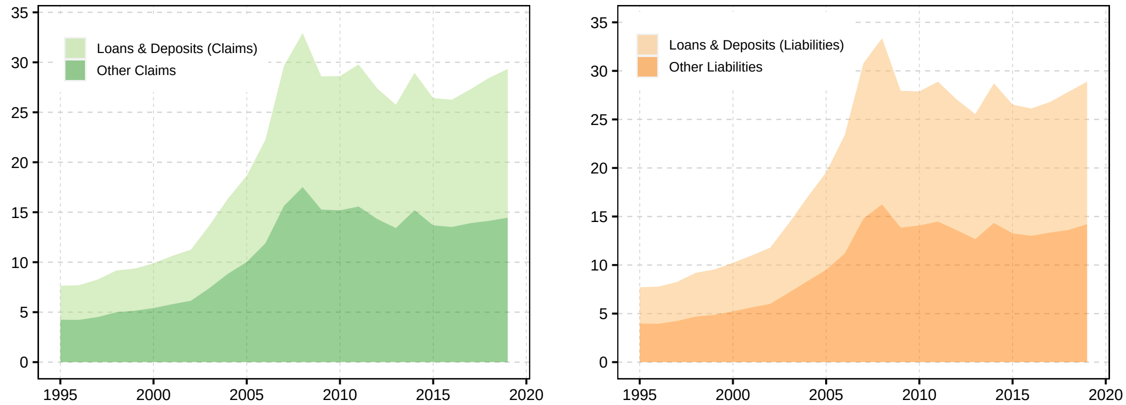
We also show that our results hold for both loans and deposits. In addition, we find that firms and individuals in developed countries use the internet to access credit and deposit services of banks located in other developed countries (North-North). Moreover, we find that internet enables cross-border positions especially when counterparties are located in economies with underdeveloped and unstable financial sector. Intuitively, firms and individuals in countries with small and unreliable banking sectors use the internet to borrow from, and deposit their savings by, large foreign banks.

Our findings complement the literature that has focused on banking regulation as one of the drivers of banks' cross-border lending after the great financial crisis (e.g. Aiyar et al., 2014; Berrospide et al., 2016; Figuet et al., 2015; Bremus and Fratzscher, 2015).[59] While stricter regulations might have constrained cross-border lending, the increase in internet connections favoured it. These findings highlight a potential trade-off for policy makers, especially in emerging economies. On one hand, investments in submarine cables may facilitate the access of local firms and individuals to credit supplied by foreign banks, and thus support economic growth. On the other hand, more cables means more lending by banks that are outside the jurisdiction of national regulators, and may therefore require bi-lateral agreements to reduce risks related to excess borrowing.

[59] For a review of the literature, see for example Buch and Goldberg (2016).

Appendix 1.A Stylised Facts - Further Disaggregations

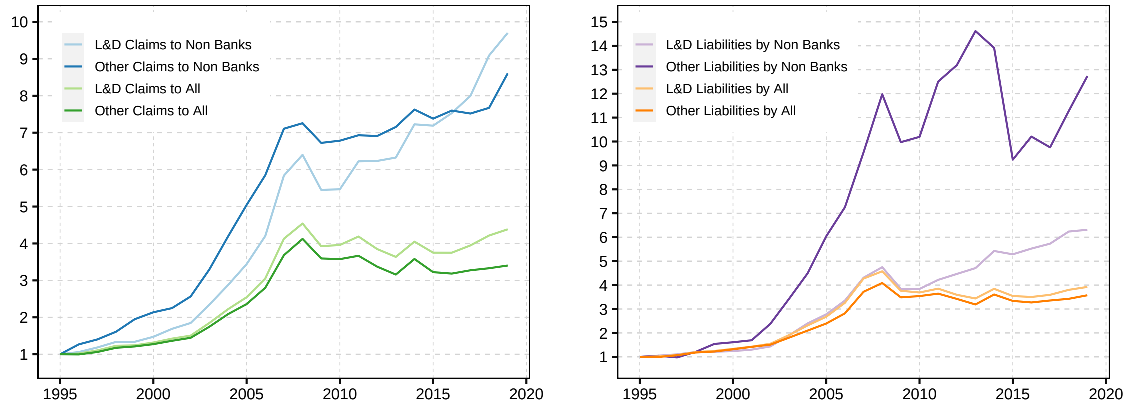
Figure 1.A.1: All Counterparties



(a) Claims (b) Liabilities

Notes: Figure 1.A.1 reports time series of cross-border positions to all counterparties, sourced from the Locational Banking Statistics of the BIS. Panel (a) shows series for loans (clear green) and other claims (dark green). Panel (b) shows series for deposits (clear orange) and other liabilities (dark orange). These aggregates are sums of all country-pair positions in a given year and are expressed in Trillions of US Dollars.

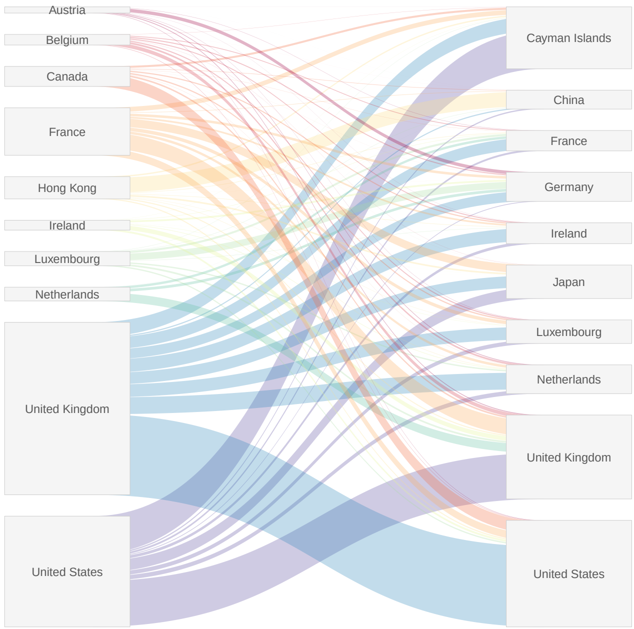
Figure 1.A.2: Indexed Time Series



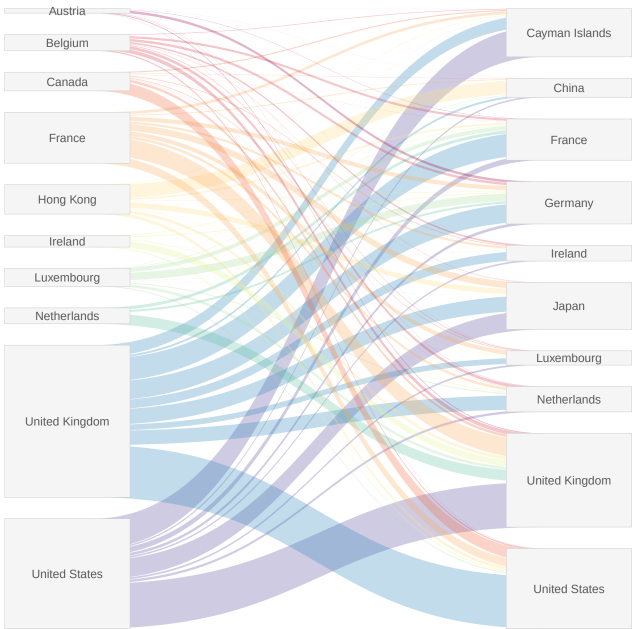
(a) Claims (b) Liabilities

Notes: Figure 1.A.2 reports the evolution of cross-border positions to non banks and all counterparties, sourced from the Locational Banking Statistics of the BIS. The series are indexed to 1995 (the value in 1995 for all series equals 1). Panel (a) and (b) report results for, respectively, claims and liabilities. These aggregates are sums of all country-pair positions in a given year and are expressed in Trillions of US Dollars.

Figure 1.A.3: Network of Export-Generating Positions - Loans



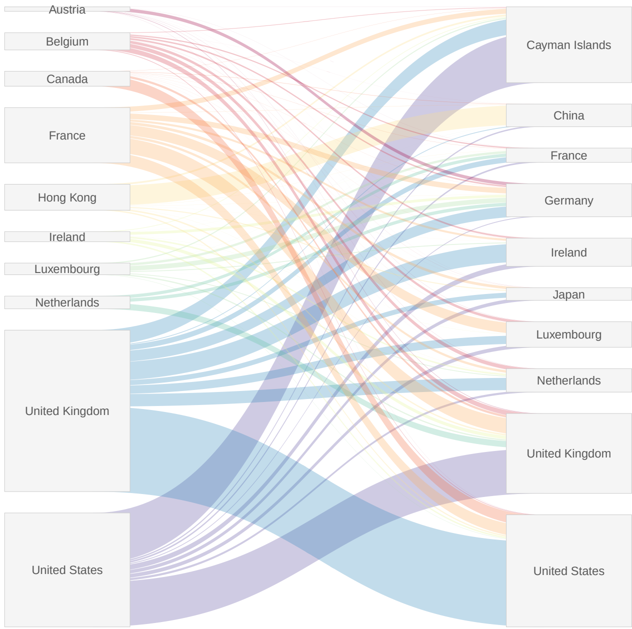
(a) Non-Bank Counterparty



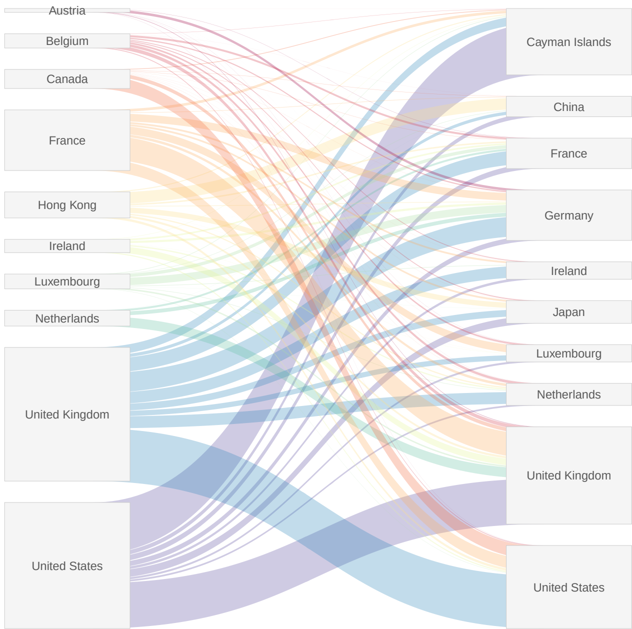
(b) All Counterparties

Notes: Figure 1.A.3 reports the trade network of loan positions, collected from the Locational Banking Statistics of the BIS. Panel (a) and (b) refer to loans to, respectively, non-bank and all counterparties. Country-pair aggregates are computed with post-2010 averages for each country pair. The reported countries are the top-10 origins (left column) and the top-10 destinations (right column) of loan positions (lenders are in origins and borrowers are in destinations).

Figure 1.A.4: Network of Export-Generating Positions - Deposits



(a) Non-Bank Counterparty



(b) All Counterparties

Notes: Figure 1.A.4 reports the trade network of deposit positions, collected from the Locational Banking Statistics of the BIS. Panel (a) and (b) refer to deposits by, respectively, non-bank and all counterparties. Country-pair aggregates are computed with post-2010 averages for each country pair. The reported countries are the top-10 origins (left column) and the top-10 destinations (right column) of deposit positions (deposit providers are in origins and deposit holders are in destinations).

Appendix 1.B International Banking Positions and Exports of Financial Services

Foreign-Affiliate Positions. We use the Consolidated Banking Statistics (CBS) database by the BIS to obtain a measure for foreign-affiliate export-generating positions. The CBS are compiled according to the nationality of banks (rather than location) on a worldwide consolidated group basis. The parent is therefore the only reporting entity for a banking group. It reports both its own claims in the reporting country and in host countries, and the claims of its branches and subsidiaries in the reporting country and in the host countries. Unlike the LBS database, the publicly available version of the CBS does not distinguish claims and liabilities for type of instruments and sector of counterparties. However, the CBS database does differentiate claims according to the nationality of the counterparty.[60] As a result, we can consider claims of foreign affiliates in the host economy on host-economy counterparties as a measure for foreign-affiliate activities. Such measure excludes foreign affiliate positions that generate cross-border trade and corresponds to the WTO definition of exports through commercial presence. The downside of this variable is that it can include positions that do not generate trade, such as foreign affiliates' deposits held at other local banks, holdings of debt securities and equity issued by local entities, and derivatives with underlying assets of local entities. Also for this reason, in what follows we test how this variable correlates with measures of foreign-affiliate sales from trade-based datasets, such as the AMNE database by the OECD and the MOFA statistics by the BEA.

Correlation with Trade Variables. To test how well our measures for cross-border and foreign-affiliate positions can proxy trends in trade in financial services, we compare them with trade-based variables of three different datasets. In trade terminology, trade in services can be disaggregated into four modes: cross-border supply (mode 1), consumption abroad (mode 2), commercial presence (mode 3) and presence of natural persons (mode 4). With the variables for cross-border and foreign-affiliate claims discussed above, we aim to proxy, respectively, mode 1 (cross border) and mode 3 (commercial presence) trade. As a start, we collect data on exports of financial services of US financial institutions from the BEA statistics. For cross-border exports of financial services - also known as balance-of-payment (BoP) exports -, we rely on BEA's statistics for Trade in Services for Finance and Insurance Services.[61] By definition, this trade variable includes all modes of trade but mode 3 (exports via foreign affiliates). Since mode 2 and 4 play a negligible role in trade in financial services, we consider this variable as an indicator for cross-border exports of

[60] This version of the dataset is called CBSI. An alternative version also differentiates according to the nationality of the guarantor of the claim in question, i.e. so called CBSG dataset.

[61] Specifically, we use Table 2.2, with destination and sector disaggregation.

financial services. Concerning foreign-affiliate sales of financial services, we consider the BEA's statistics for Majority Owned Foreign Affiliate (MOFA). These statistics cover all finance- and insurance-sector transactions between majority-owned foreign affiliates of US financial institutions and residents in the host country, where majority ownership is defined as the combined ownership of all U.S. parents exceeding 50 percent.[62]

While the BEA statistics have good time and host-country coverage, and exclude re-exports, they are available only for the U.S. as the exporting economy. To extend the coverage on exporting countries, we use a database on cross-border trade published by the USITC and a database on foreign-affiliate sales from the OECD. The USITC's International Trade and Production Database for Estimation (ITPD-E) provides a consistent global coverage of bilateral cross-border and domestic trade in services from 2005 to 2016.[63] The Affiliates of Multinational Enterprises (AMNE) database by the OECD covers foreign affiliate sales of financial and insurance services for multiple exporting countries, with a disaggregation by destination and year. A drawback of this data source is that it covers all foreign affiliate sales, including cross-border exports.

We use these trade-based variables to test whether our position-based variables from BIS statistics are good proxies for trade in financial services.

Table 1.B.1: International Positions vs Export Variables

	(1) BEA CB Exp.	(2) ITC CB Exp.	(3) BEA FA Exp.	(4) OECD FA Exp. (TO)	(5) OECD FA Exp. (GO)
LBS CB Baseline	0.912	0.817			
LBS CB Tot. Claims	0.909	0.786			
CBS CB Tot. Claims	0.810	0.605			
CBS FA Tot. Claims			0.957	0.797	0.739
Exporters	US	ALL	US	ALL	ALL

Notes: Table 1.B.1 reports correlation coefficients between variables for international positions and export variables, computed with an origin-destination-time dataset over 2010-2016, which excludes tax havens. LBS CB Baseline is the sum of cross-border loans and deposits from the Locational Banking Statistics (LBS) by the BIS. LBS CB Tot. Claims and CBS CB Tot. Claims are overall cross-border claims from, respectively, the LBS and the Consolidated Banking Statistics (CBS) of the BIS. BEA CB Exp. and BEA FA Exp. are cross-border exports and foreign-affiliates sales of financial and insurance services from BEA statistics for the US. USITC CB Exp. is cross-border trade in financial and insurance services from the USITC database. OECD FA Exp. (TO) and OECD FA Exp. (GO) are, respectively, turnover and gross output of foreign affiliates from the OECD AMNE database.

Table 1.B.1 reports the correlations across these variables. First, we correlate different versions of cross-border claims by the BIS (first three rows) with variables of cross-border trade by the BEA (first column) and USITC (second column). The first variable ("LBS CB Baseline") is our baseline measure for cross-border (export-generating) positions, namely the sum of cross-border loans and deposits from the Locational Banking Statistics (LBS)

[62]Specifically, we use Table II.E7, which exclude re-exports.

[63]In principle, the database starts in 2000 but the country coverage for services trade is very poor until 2005. Moreover, the coverage has improved substantially only since 2010.

by the BIS. This variable correlates well with the export statistics by the BEA (US only) and the USITC, with correlation coefficients of, respectively, 0.912 and 0.817. We then consider an alternative variable for cross-border positions, computed with overall cross-border claims from the LBS database ("LBS CB Tot. Claims"). We expect the correlation with trade-based variables to be lower because this variable includes claims that may not generate trade in financial services. The correlation coefficients are indeed slightly lower, namely 0.909 and 0.786. Furthermore, we consider a second alternative variable for cross-border positions, obtained from the Consolidated Banking Statistics (CBS) of the BIS ("CBS CB Tot. Claims"). As Table 1.B.1 shows, this variable correlates less with the trade-based variables.

Second, we correlate data on foreign-affiliate positions (last row) with data on foreign-affiliate sales by the BEA (third column) and OECD (fourth and fifth columns). This variable on foreign-affiliate positions comes from the CBS database by the BIS, and reports claims of foreign affiliates on host-country residents ("CBS FA Tot. Claims"). The coefficient of 0.957 reported in column 3 of Table 1.B.1 shows that this variable correlates well with the variable for foreign-affiliate sales reported by the BEA (US only). Finally, the coefficients of 0.797 and 0.739 in columns 5 and 6 suggest a lower correlation with the OECD data on a wider range of exporters - a discrepancy that could be driven by the inclusion of re-exports in the OECD data. Overall, Table 1.B.1 shows that the baseline variables for cross-border and foreign-affiliate positions correlate very well with the best-quality data on cross-border and foreign-affiliate exports of financial services.

Appendix 1.C Further Checks - Full Tables

Table 1.C.1: Cross Border and Foreign Affiliates - OLS Full

VARIABLES	(1) ln(CB)	(2) ln(FA)	(3) ln(CB)	(4) ln(FA)	(5) ln(CB)	(6) ln(FA)	(7) ln(CB)	(8) ln(FA)
ln(Distance)	-1.028***	-1.358***	-0.401***	-0.512*	-0.262*	-0.446	-0.382**	-0.469
	(0.138)	(0.266)	(0.140)	(0.299)	(0.146)	(0.328)	(0.149)	(0.354)
ln(Trade)			0.735***	0.992***	0.725***	0.987***	0.700***	0.982***
			(0.116)	(0.235)	(0.116)	(0.235)	(0.113)	(0.237)
Cables					0.123***	0.059	-0.495***	-0.062
					(0.034)	(0.088)	(0.157)	(0.431)
ln(Distance) $\times$ Cables							0.083***	0.016
							(0.022)	(0.053)
Language	1.170***	2.754***	0.664***	2.073***	0.637***	2.060***	0.569***	2.046***
	(0.192)	(0.503)	(0.188)	(0.534)	(0.185)	(0.535)	(0.184)	(0.540)
Religion	1.222***	2.862***	0.973***	2.526***	0.938***	2.509***	0.993***	2.520***
	(0.332)	(0.883)	(0.333)	(0.852)	(0.326)	(0.850)	(0.328)	(0.852)
Currency	-0.301	-0.464	-0.473**	-0.697	-0.402*	-0.663	-0.513**	-0.685
	(0.211)	(0.631)	(0.208)	(0.632)	(0.208)	(0.641)	(0.207)	(0.654)
Legal	0.020	-0.450	-0.091	-0.599*	-0.114	-0.610*	-0.059	-0.599*
	(0.144)	(0.342)	(0.130)	(0.341)	(0.131)	(0.343)	(0.130)	(0.346)
FTA	0.446*	0.814*	0.115	0.367	0.167	0.392	0.115	0.382
	(0.250)	(0.471)	(0.228)	(0.513)	(0.231)	(0.516)	(0.238)	(0.518)
Observations	412	412	412	412	412	412	412	412
Adjusted R-squared	0.786	0.532	0.817	0.556	0.822	0.555	0.827	0.554
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
FE	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.C.1 reports OLS estimates for the impact of internet on banks' cross-border (CB) and foreign-affiliate (FA) positions. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Controls for colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions. Regressions are estimated on a origin-destination dataset obtained by averaging variables over 2010-2019 and excluding contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 1.C.2: Cross Border and Foreign Affiliates - PPML Full

VARIABLES	(1) CB	(2) FA	(3) CB	(4) FA	(5) CB	(6) FA	(7) CB	(8) FA
ln(Distance)	-0.683***	-0.602***	-0.357***	0.500***	-0.192**	0.503**	-0.414***	0.474**
	(0.068)	(0.220)	(0.090)	(0.194)	(0.086)	(0.197)	(0.102)	(0.205)
ln(Trade)			0.559***	1.626***	0.520***	1.619***	0.471***	1.608***
			(0.096)	(0.183)	(0.087)	(0.183)	(0.080)	(0.182)
Cables					0.138***	0.006	-0.428***	-0.058
					(0.023)	(0.037)	(0.134)	(0.202)
ln(Distance) $\times$ Cables							0.074***	0.008
							(0.018)	(0.025)
Colony	1.181***	-1.105	1.070***	-1.084	0.967***	-1.101	0.735**	-1.147
	(0.316)	(0.870)	(0.371)	(0.992)	(0.330)	(0.994)	(0.309)	(0.992)
Language	1.548***	1.035***	1.323***	0.359	1.177***	0.360	0.937***	0.353
	(0.210)	(0.389)	(0.206)	(0.340)	(0.212)	(0.340)	(0.259)	(0.341)
Religion	0.840**	3.668***	0.708**	3.061***	0.610**	3.062***	0.737**	3.095***
	(0.343)	(0.698)	(0.337)	(0.626)	(0.300)	(0.627)	(0.315)	(0.645)
Currency	0.314	0.355	0.262	-0.485	0.385*	-0.485	-0.012	-0.509
	(0.230)	(0.624)	(0.222)	(0.595)	(0.199)	(0.594)	(0.206)	(0.606)
Legal	-0.057	-0.142	-0.136	-0.351	-0.193	-0.357	-0.138	-0.352
	(0.143)	(0.280)	(0.131)	(0.215)	(0.118)	(0.223)	(0.124)	(0.225)
FTA	-0.070	0.284	-0.200	-0.082	-0.183	-0.079	-0.278**	-0.096
	(0.164)	(0.338)	(0.152)	(0.202)	(0.151)	(0.200)	(0.138)	(0.207)
Observations	3,013	2,616	2,970	2,616	2,970	2,616	2,970	2,616
Estimator	PPML	PPML	PPML	PPML	PPML	PPML	PPML	PPML
FE	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.C.2 reports PPML estimates for the impact of internet on banks' cross-border (CB) and foreign-affiliate (FA) positions. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Controls for colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions. Regressions are estimated on a origin-destination dataset obtained by averaging variables over 2010-2019 and excluding contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 1.C.3: Non Linearities - Full

VARIABLES	(1) ln(CB)	(2) ln(CB)	(3) ln(CB)	(4) ln(CB)	(5) ln(CB)	(6) ln(CB)	(7) ln(CB)	(8) ln(CB)
Cables	0.272***	0.397***						
	(0.063)	(0.083)						
Cables × Cables	-0.020**	-0.053***						
	(0.009)	(0.018)						
Cables = 0			-0.581***	-0.581***				
			(0.100)	(0.100)				
Cables = 2			0.144	0.144				
			(0.166)	(0.166)				
Cables = 3			-0.059	-0.059				
			(0.189)	(0.189)				
Cables = 4			-0.459*	-0.459*				
			(0.262)	(0.262)				
Cables = 5			-0.108	-0.108				
			(0.335)	(0.335)				
Cables = 6			0.479	0.479				
			(0.481)	(0.481)				
Cables = 7			0.298	0.298				
			(0.611)	(0.610)				
Cables = 8			-0.363	-0.364				
			(1.015)	(1.015)				
Cables = 12			1.071***					
			(0.289)					
Cables >= 1					0.596***	0.586***	0.583***	0.579***
					(0.088)	(0.088)	(0.098)	(0.098)
Cables >= 2							0.036	0.021
							(0.125)	(0.126)
ln(Distance)	-0.670***	-0.679***	-0.693***	-0.693***	-0.680***	-0.678***	-0.678***	-0.676***
	(0.082)	(0.081)	(0.082)	(0.082)	(0.080)	(0.080)	(0.081)	(0.081)
ln(Trade)	0.420***	0.409***	0.398***	0.398***	0.403***	0.402***	0.403***	0.402***
	(0.053)	(0.053)	(0.053)	(0.053)	(0.052)	(0.052)	(0.053)	(0.053)
Colony	0.569**	0.578**	0.589**	0.589**	0.582**	0.586**	0.582**	0.586**
	(0.230)	(0.229)	(0.228)	(0.228)	(0.228)	(0.228)	(0.228)	(0.228)
Language	0.566***	0.567***	0.557***	0.557***	0.556***	0.562***	0.557***	0.563***
	(0.115)	(0.115)	(0.116)	(0.116)	(0.115)	(0.115)	(0.115)	(0.115)
Religion	0.366*	0.332	0.309	0.309	0.339*	0.318	0.340*	0.319
	(0.202)	(0.203)	(0.204)	(0.204)	(0.201)	(0.203)	(0.201)	(0.203)
Currency	0.135	0.149	0.169	0.169	0.181	0.184	0.181	0.184
	(0.174)	(0.174)	(0.173)	(0.173)	(0.172)	(0.172)	(0.172)	(0.172)
Legal	0.160*	0.157*	0.140*	0.140*	0.153*	0.146*	0.154*	0.146*
	(0.083)	(0.083)	(0.083)	(0.083)	(0.082)	(0.082)	(0.082)	(0.082)
FTA	0.133	0.142	0.144	0.144	0.148	0.150	0.147	0.149
	(0.116)	(0.115)	(0.115)	(0.115)	(0.115)	(0.115)	(0.115)	(0.115)
Observations	1,783	1,781	1,783	1,781	1,783	1,781	1,783	1,781
Adjusted R-squared	0.801	0.801	0.802	0.802	0.803	0.802	0.803	0.802
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Specification	Full Sample	Drop DK-SE	Full Sample	Drop DK-SE	Full Sample	Drop DK-SE	Full Sample	Drop DK-SE
FE	o-d	o-d	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.C.3 reports regression results testing the non-linearity of cables. Odd-numbered and even-numbered columns show the results for, respectively, the full sample of country pairs and the sub-sample excluding country pairs Denmark-Sweden and Sweden-Denmark (which have 12 connections). Columns 1 and 2 report results for the squared term of cables. Columns 3 and 4 compare categories of number of connections to the baseline category with 1 connection. Columns 5-6 and 7-8 show results dummies which equal one for country pairs with at least, respectively, 1 and 2 connections. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Controls for colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions. Regressions are estimated on a origin-destination dataset obtained by averaging variables over 2010-2019 and excluding contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 1.C.4: Non Linearities - Distance

VARIABLES	(1) ln(CB)	(2) ln(CB)	(3) ln(CB)	(4) ln(CB)	(5) ln(CB)	(6) ln(CB)
Cables = 0	-1.514	-1.514				
	(0.983)	(0.983)				
Cables = 2	1.162	1.163				
	(1.464)	(1.464)				
Cables = 3	-2.077	-2.077				
	(2.585)	(2.584)				
Cables = 4	-0.215	-0.215				
	(2.071)	(2.071)				
Cables = 5	-5.475***	-5.475***				
	(1.380)	(1.380)				
Cables = 6	-18.721	-18.721				
	(12.373)	(12.370)				
Cables = 7	-9.492***	-9.492***				
	(2.728)	(2.727)				
Cables = 8	-18.807***	-18.809***				
	(3.719)	(3.718)				
Cables = 12	0.847**					
	(0.363)					
Cables = 0 × ln(Distance)	0.110	0.109				
	(0.114)	(0.114)				
Cables = 2 × ln(Distance)	-0.125	-0.125				
	(0.174)	(0.174)				
Cables = 3 × ln(Distance)	0.242	0.242				
	(0.315)	(0.314)				
Cables = 4 × ln(Distance)	-0.041	-0.041				
	(0.250)	(0.250)				
Cables = 5 × ln(Distance)	0.713***	0.713***				
	(0.191)	(0.191)				
Cables = 6 × ln(Distance)	2.289	2.289				
	(1.440)	(1.439)				
Cables = 7 × ln(Distance)	1.212***	1.212***				
	(0.334)	(0.334)				
Cables = 8 × ln(Distance)	2.395***	2.395***				
	(0.468)	(0.468)				
Cables = 12 × ln(Distance)	0.000					
	(0.000)					
Cables >= 1			0.796	0.651	1.292*	1.303*
			(0.727)	(0.739)	(0.778)	(0.781)
Cables >= 1 × ln(Distance)			-0.024	-0.008	-0.083	-0.085
			(0.085)	(0.087)	(0.091)	(0.092)
Cables >= 5					-4.704***	-6.322***
					(1.386)	(1.126)
Cables >= 5 × ln(Distance)					0.634***	0.823***
					(0.177)	(0.147)
ln(Trade)	0.386***	0.386***	0.403***	0.402***	0.399***	0.393***
	(0.053)	(0.053)	(0.053)	(0.053)	(0.053)	(0.053)
ln(Distance)	-0.810***	-0.810***	-0.673***	-0.675***	-0.678***	-0.687***
	(0.125)	(0.125)	(0.088)	(0.088)	(0.088)	(0.088)
Colony	0.579**	0.579**	0.584**	0.586**	0.581**	0.586**
	(0.229)	(0.229)	(0.228)	(0.228)	(0.229)	(0.228)
Language	0.563***	0.563***	0.558***	0.563***	0.553***	0.559***
	(0.117)	(0.117)	(0.116)	(0.116)	(0.115)	(0.116)
Religion	0.314	0.314	0.338*	0.318	0.361*	0.328
	(0.204)	(0.204)	(0.201)	(0.203)	(0.202)	(0.203)
Currency	0.168	0.168	0.182	0.184	0.149	0.144
	(0.175)	(0.175)	(0.172)	(0.172)	(0.174)	(0.174)
Legal	0.147*	0.148*	0.151*	0.145*	0.155*	0.145*
	(0.083)	(0.083)	(0.083)	(0.083)	(0.082)	(0.082)
FTA	0.136	0.136	0.148	0.150	0.140	0.142
	(0.116)	(0.116)	(0.115)	(0.115)	(0.114)	(0.114)
Observations	1,783	1,781	1,783	1,781	1,783	1,781
Adjusted R-squared	0.803	0.802	0.803	0.802	0.803	0.803
Specification	Full Sample	Drop DK-SE	Full Sample	Drop DK-SE	Full Sample	Drop DK-SE
Estimator	OLS	OLS	OLS	OLS	OLS	OLS
FE	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.C.4 reports regression results testing the non-linearity of cables. Odd-numbered and even-numbered columns show the results for, respectively, the full sample of country pairs and the sub-sample excluding country pairs Denmark-Sweden and Sweden-Denmark (which have 12 connections). Columns 1 and 2 compare categories of number of connections to the baseline category with 1 connection. Columns 5-6 and 7-8 show results dummies which equal one for country pairs with at least, respectively, 1 and 2 connections. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Controls for colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions. Regressions are estimated on a origin-destination dataset obtained by averaging variables over 2010-2019 and excluding contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 1.C.5: Impact through Time - Full Results for Selected Years (OLS)

YEARS	(1) 1995	(2) 2000	(3) 2005	(4) 2010	(5) 2015	(6) 2019
Cables	0.013	0.140**	0.140***	0.167***	0.165***	0.164***
	(0.150)	(0.058)	(0.049)	(0.054)	(0.046)	(0.033)
ln(Distance)	-0.606***	-0.388*	-0.081	-0.569***	-0.539**	-0.191
	(0.200)	(0.206)	(0.212)	(0.206)	(0.227)	(0.191)
ln(Trade)	0.672***	0.598***	0.883***	0.506***	0.520***	0.670***
	(0.126)	(0.165)	(0.124)	(0.107)	(0.118)	(0.096)
Language	0.552**	0.788***	0.525**	0.731***	0.614***	0.316
	(0.250)	(0.249)	(0.231)	(0.232)	(0.229)	(0.216)
Religion	0.484	1.126***	0.779**	0.291	0.493	0.323
	(0.343)	(0.340)	(0.358)	(0.311)	(0.341)	(0.324)
Currency	-1.448***	-0.440	-0.473*	-0.501**	-0.407	-0.207
	(0.484)	(0.277)	(0.274)	(0.250)	(0.296)	(0.234)
Legal	-0.007	-0.211	-0.212	-0.077	-0.145	-0.014
	(0.153)	(0.147)	(0.150)	(0.142)	(0.159)	(0.150)
FTA	-0.106	0.148	-0.154	0.058	0.295	0.546*
	(0.282)	(0.257)	(0.286)	(0.273)	(0.335)	(0.294)
Observations	456	437	445	433	429	427
Adjusted R-squared	0.786	0.800	0.812	0.790	0.817	0.852
FE	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.C.5 reports results for OLS regressions estimated for years 1995, 2000, 2005, 2010, 2015 and 2019. The dependent variable is the natural logarithm of export-generating positions. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Controls for distance, manufacturing trade, colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions. All regressions are estimated on a origin-destination sub-sample of country pairs that have information available from 1995 onwards (around 450 country pairs) and exclude contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 1.C.6: Impact through Time - Full Results for Selected Years (PPML)

YEARS	(1) 1995	(2) 2000	(3) 2005	(4) 2010	(5) 2015	(6) 2019
Cables	0.035	0.166***	0.152***	0.207***	0.158***	0.120***
	(0.112)	(0.051)	(0.031)	(0.024)	(0.028)	(0.031)
ln(Distance)	-0.146	-0.082	0.337**	0.486***	0.210**	0.271**
	(0.211)	(0.120)	(0.136)	(0.124)	(0.090)	(0.109)
ln(Trade)	0.731***	0.601***	0.772***	0.694***	0.635***	0.616***
	(0.121)	(0.091)	(0.107)	(0.080)	(0.091)	(0.101)
Language	0.436*	0.731***	0.094	0.969***	1.537***	0.610
	(0.243)	(0.249)	(0.330)	(0.258)	(0.375)	(0.402)
Religion	0.269	1.211***	0.035	-0.210	0.530*	0.505
	(0.356)	(0.436)	(0.535)	(0.347)	(0.319)	(0.443)
Currency	-0.265	0.419*	0.517*	0.728***	0.224	0.272
	(0.415)	(0.235)	(0.304)	(0.195)	(0.203)	(0.218)
Legal	0.048	-0.366**	0.008	-0.140	-0.551***	-0.089
	(0.135)	(0.173)	(0.216)	(0.123)	(0.145)	(0.167)
FTA	0.388	-0.245	-0.612**	0.147	-0.031	0.243
	(0.254)	(0.298)	(0.262)	(0.152)	(0.147)	(0.202)
Observations	979	974	977	969	976	978
FE	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.C.6 reports results for PPML regressions estimated for years 1995, 2000, 2005, 2010, 2015 and 2019. The dependent variable is export-generating positions. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Trade is flows of manufacturing trade between two countries. Controls for distance, manufacturing trade, colonial relations, language, religion, currency, legal system and a free trade agreement are included in all regressions. All regressions are estimated on a origin-destination sub-sample of country pairs that have information available from 1995 onwards (around 970 country pairs) and exclude contiguous and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix 1.D Channels - OLS Table

Table 1.D.1

VARIABLES	(1) ln(Loans)	(2) ln(Deposits)	(3) ln(Loans)	(4) ln(Deposits)	(5) ln(Loans)	(6) ln(Deposits)
ln(Distance)	-0.691***	-0.623***	-0.694***	-0.628***	-0.667***	-0.623***
	(0.095)	(0.089)	(0.095)	(0.089)	(0.096)	(0.091)
Colony	0.637*	0.549*	0.641*	0.557*	0.665**	0.564*
	(0.329)	(0.284)	(0.330)	(0.284)	(0.338)	(0.292)
Language	0.547***	0.615***	0.546***	0.613***	0.484***	0.564***
	(0.138)	(0.125)	(0.138)	(0.125)	(0.140)	(0.129)
Religion	0.151	0.639***	0.168	0.674***	0.189	0.697***
	(0.234)	(0.207)	(0.236)	(0.207)	(0.235)	(0.208)
Currency	-0.013	0.039	-0.013	0.037	0.026	0.071
	(0.197)	(0.177)	(0.197)	(0.176)	(0.197)	(0.182)
Legal	0.047	0.125	0.051	0.135	0.044	0.138
	(0.103)	(0.087)	(0.103)	(0.088)	(0.104)	(0.089)
FTA	0.206	0.084	0.203	0.077	0.215	0.015
	(0.142)	(0.129)	(0.142)	(0.129)	(0.144)	(0.130)
ln(Trade)	0.488***	0.450***	0.484***	0.442***	0.528***	0.466***
	(0.060)	(0.066)	(0.060)	(0.066)	(0.064)	(0.069)
Cables	0.169***	0.121***	0.139***	0.060*	0.353***	0.251**
	(0.036)	(0.029)	(0.047)	(0.033)	(0.120)	(0.117)
Cables × Emerging Dest.			0.066	0.137**	-0.011	0.028
			(0.066)	(0.054)	(0.067)	(0.072)
Cables × Assets Dest.					-0.001*	-0.002**
					(0.001)	(0.001)
Cables × Z-Score Dest.					-0.004	0.002
					(0.004)	(0.003)
Observations	1,627	1,627	1,627	1,627	1,580	1,580
Adjusted R-squared	0.740	0.800	0.740	0.801	0.741	0.803
FE	o-d	o-d	o-d	o-d	o-d	o-d

Notes: Table 1.D.1 reports estimates for the channels of the impact of internet on banks' cross-border loans (odd-numbered columns) and deposits (even-numbered columns) to non banks. All results are obtained with the OLS estimator using an origin-destination dataset, obtained by averaging variables over 2010-2019. Cables is the number of cables connecting two countries. Distance is the distance between two countries. Emerging Dest. is a dummy which equals 1 when destination countries are emerging economies. Assets Dest. is the total amounts of assets of the banking sector in destination economies (size of the banking sector). Z-Score is the Z-score of the banking sector in destination countries, defined as (ROA+(equity/assets))/sd(ROA) (stability of the banking sector). Trade is flows of manufacturing trade between two countries. Colony, Language, Religion, Currency, Legal and FTA are dummies which equal one if two countries share, respectively, colonial relations, language, religion, currency, legal system and a free trade agreement. In all regressions, we exclude bordering and landlocked countries. Robust standard errors are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Chapter 2

The Local Impact of the FED in the Aftermath of the Financial Crisis

The Local Impact of the FED in the Aftermath of the Financial Crisis*

Abstract

This paper studies the local impact of the Federal Reserve's monetary policy before and after the financial crisis of 2008. To do so, I use a Bayesian Global vector autoregression (VAR) with state equations and a national block. I estimate it over two different periods, namely 1990-2007 and 2010-2019, by using monthly data on states' real economic activity and unemployment. The model estimates two key differences in states' reactions to an expansionary monetary-policy shock between the pre and post-crisis periods. First, the average reactions of states' responses is larger in the aftermath of the crisis. Second, such reactions are also more heterogeneous across states. Specifically, states like California, Nevada and Florida converge back to equilibrium much faster than before the crisis. As house markets in these states are also the ones most severely affected by the crisis, I explore whether differences in house prices can explain these trends. With a cross-sectional regression analysis on stacked samples, I find that house prices correlate with states' responses more in the post-crisis sample, and that this difference is statistically significant.

Keywords: FED, Financial Crisis, Regional Impact, House Prices

JEL classification: E52, E58, E65

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2.1 Introduction

In the United States, monetary policy has a local dimension (Carlino and DeFina, 1999). This is likely the case because state-specific characteristics, such as labour and house market conditions, can determine the impact of monetary measures on states' economies (e.g. Fratantoni and Schuh, 2003). Such state-specific characteristics are not necessarily fixed in time and can change in the aftermath of large economic shocks, also heterogeneously across states. For example, during the financial crisis of 2008 house prices collapsed in California, Nevada and Florida, while registered only mild decreases in central states like Kansas and Missouri (Wang, 2019). As a result, the last financial crisis might have changed the way single states react to monetary policy in the United States.

From 2010, the Federal Reserve (Fed) has implemented large measures to stimulate the economy, known as Quantitative Easing (QE).[1] A recent literature has shown how the beneficial effect of QE on borrowing and consumption was lower in areas where house markets were most negatively affected by the crisis (e.g. Beraja et al., 2019; Di Maggio et al., 2020). In a nutshell, this happens because households in areas with low house prices cannot use either home-equity credit lines or savings from mortgage refinancing to increase spending (e.g. Mian and Sufi, 2014; Bhutta and Keys, 2016; Chen et al., 2020).[2]

In light of this evidence, this paper aims to estimate if and how the real effects of monetary policy on states' economies have changed in the aftermath of the last financial crisis. I do so by considering monthly state-level data on real output and unemployment provided by the Fed of Philadelphia and the Bureau of Labor Statistics. As this data is available from 1990 onwards, I can run separate analyses for the periods before and after the crisis. In summary, I find that the positive effects of a monetary stimulus on real output and unemployment are significantly reduced after the crisis in states where house prices decreased more, such as California, Nevada and Florida. Interestingly, these were also the states that reacted better to a monetary stimulus before the financial crisis. I provide evidence that these results are driven by the amplification mechanism of house-equity extraction by households. While before the crisis residents in, say, California could respond to a cut in rates by extracting equity from their high-value properties and spend more, they could no longer do so after 2008.

More in details, in the first part of the paper I estimate states' responses to a shock in the Federal Funds Rate before and after the crisis. I do so with a Bayesian Global Vector Autoregression (GVAR) estimated with monthly data over two samples, namely 1990-2007 and 2010-2019. This model setup has some clear advantages.

[1] For a detail explanation of the different measures and their timing, see Bernanke (2020).

[2] The activity of equity extraction induced by monetary policy is referred to as the house-equity channel - or house-price channel - of monetary policy.

First, it allows separate blocks of equations for the Fed and the states. The Fed block includes measures for national inflation, policy rate and monetary variables, while the states' block contains measures for state-level real output, such as real economic activity (REA) and unemployment. In this paper, the policy rate is the Shadow Federal Funds Rate (FFR) computed by Wu and Xia (2016), which approximates well monetary-policy measures also in times of Zero Lower Bound. Real output is measured with the Coincident Index constructed by the Fed of Philadelphia, which is a composite index for REA computed for each state via a dynamic single-factor model including nonfarm payroll employment, average hours worked in manufacturing by production workers, the unemployment rate, and wage and salary disbursements (Crone and Clayton-Matthews, 2005). Each block is augmented with aggregates of the other block, which enter the block's equations as weakly exogenous variables.[3] For example, the aggregate of states' real economic activity enters the Fed's Taylor Rule, while the policy rate enters the output equation of the states. In addition, the states' equations are augmented with aggregates of real variables of bordering states, which control for cross-state spatial spillovers. Second, the Bayesian setup reduces the issues of overfitting that characterise VAR models and issues of small-sample estimation.[4]

I use this model to estimate the responses of states' REA and unemployment to an expansionary monetary-policy shock, in both pre- and post-crisis samples. These two sets of responses differ in two main ways. First, the model estimates a larger (on-impact) effect of monetary policy across all states in the post-crisis sample. This larger estimated effect could be due to several factors. For example, after the crisis the Fed has committed to zero rates for a prolonged period of time. An unexpected deviation from that commitment could have produced larger effects than conventional monetary policy in the pre-crisis period Feldkircher and Huber (2018).[5] Second, the model shows that also the heterogeneity of states' responses increase in the post-crisis sample.

In the second part of the paper I explore whether this increase in heterogeneity maybe due to larger cross-state differences in house prices. Intuitively, with homogeneous house markets, states' responses would be similar. Differently, as house-price differences increase, states' responses would diverge.[6] I explore this hypothesis by considering the impulse-response functions of the states more affected by the house-market crisis, namely California, Nevada, Arizona and Florida. I find that in the post-crisis sample the responses of these states

[3]This is the same type of model proposed by, among others, Georgiadis (2015) for the EU and Fischer et al. (2021) for the US.

[4]In this paper, I use a non-conjugate Minnesota prior.

[5]Alternatively, I explore the hypothesis that states register larger expansionary effects because the crisis pushed their economies back to the initial phase of the business cycle.

[6]More specifically, the increase in heterogeneity would depend also on the larger estimated effect of QE. If the effect estimated effect of QE is generally larger, states with unchanged prices would perform better than in the past. In this case, the distance between such states (performing well) and states with house-market crisis (performing badly) would increase, and therefore heterogeneity would be higher. In all, it is a trade-off between the estimated effectiveness of QE and the degree of the house-market crisis in the affected states.

converge back to equilibrium much faster than in the pre-crisis sample. More formally, I test this hypothesis by regressing states' responses in REA and unemployment to a monetary policy shock on differences in house prices and controls for other channels of monetary policy. I find that house prices correlate more with states' responses in the post-crisis sample. When I estimate regressions on stacked samples, I also find that this post-crisis difference is statistically significant.

These findings are broadly in line with papers that focus on micro-level data on lending. Specifically, Beraja et al. (2019) show that QE has boosted mortgage refinancing and car purchases, but less so in cities like Las Vegas, where the house-market crisis was severe. Beraja et al. (2019) also show that these dynamics did not take place after the crisis of 2001, as house prices remained stable. Similarly, Di Maggio et al. (2020) focus on QE-eligible conforming mortgages and show that especially the first round of quantitative easing led to large refinancing and increases in consumption. My results show that the findings of these authors can be extended to "aggregate" state economies, as the impact of monetary policy on states' real output and unemployment changed after the crisis. The policy implications of these findings are twofold. First, they are relevant for future implementation of QE programs. As mentioned by Beraja et al. (2019), the Fed does not currently have a mandate to reduce state inequalities. However, focusing asset purchases in the areas that are most affected by negative exogenous shocks could improve the overall effectiveness of monetary policy.[7] Second, these findings can be of interest for national fiscal policies that address regional business cycles. For example, states like Texas did not see large falls in house prices over 2005-2013, and they reacted well to monetary expansions after the crisis. On the other hand, the house markets in other states, like California, were hardly hit, and monetary policy did not produce the expected stimulus. A fiscal transfer from the first to the second could ease these disparities.

The rest of the paper is organized as follows. Section 2.2 briefly summarizes the related literature and lays down the contributions. Section 2.3 illustrates the Global VAR model and its Bayesian setup. Section 2.4 explains the data used for estimation and related sources. Section 2.6 and 2.7 reports the results of the model for, respectively, the Fed and the states. Section 2.8 addresses the cross-sectional heterogeneity with a regression analysis. Section 2.9 concludes.

2.2 Related Literature

This paper contributes to the general literature on the regional impact of the Fed. The first authors to analyse the local dimension of monetary policy in the US are Carlino and DeFina

[7] A similar point is made by Beckworth (2010) for conventional monetary policy and Optimal Currency Area.

(1998, 1999); Carlino et al. (1999). They use a state-by-state VAR and find that the states in the Great Lakes see the largest losses in real personal income. These results are confirmed by Crone (2005), who uses more aggregated data and also reports that the Energy Belt - i.e. an area that includes the South West and the Rocky Mountains' regions - is the one least impacted by the Fed's monetary policy. Similar results with region-level data are also found by Kouparitsas (2001) and Owyang and Wall (2009). Furthermore, Beckworth (2010) proposes the most recent analysis on the regional spillovers of the Fed's policy. Beckworth estimates a near-VAR model for the period 1983-08 and confirms the results of the other authors. In addition, he studies possible explanations of the cross-state differences and find that the criteria that define an Optimal Currency Area drive such differences. Finally, Fratantoni and Schuh (2003) focus on regional differences in house prices. They estimate a heterogeneous-agent VAR model with data on U.S. regions over 1986-1996 and show that coastal housing boom can influence the effectiveness of monetary policy. This paper builds on this literature to propose an analysis of the local impact of monetary policy after the crisis of 2008.

This paper also contributes to the literature assessing the impact of unconventional monetary policy. There is currently a debate in the empirical literature studying the Fed's rounds of QE and forward guidance. On the one hand, some authors report that the QE and forward guidance of the Fed were not as effective as conventional monetary policy (Engen et al., 2015; Gust et al., 2017; Eberly et al., 2019). On the other hand, other authors report that unconventional monetary policy was at least as effective as standard monetary policy (Swanson and Williams, 2014; Debortoli et al., 2020). Bernanke (2020) summarises this literature and argues that QE and forward guidance can provide considerable policy space at the Zero Lower Bound. In addition, Wu and Xia (2016) argue that QE could have had even stronger expansionary effects on unemployment. I build upon these results and I estimate the role played by monetary policy in driving states' real economy in a period of ZLB.

A new literature has addressed the effect of QE on local economies by relying on micro-level loan data. Beraja et al. (2019) find that QE has led to more mortgage refinancing and consumption, but less so in regions with low house prices. Di Maggio et al. (2020) use the same data to show that this was the case especially for the first round of QE, compared to the third round. Further evidence for the house-price channel is found by Mian and Sufi (2014), Bhutta and Keys (2016), Chen et al. (2020) and Di Maggio et al. (2017). Moreover, Alpanda and Zubairy (2019) use time-series, state-dependent local projections on a pre-2007 sample to show that the house-price channel works less when households' debt it high. Differently from these authors, I focus on state-level variables, I compare results in pre- and post-crisis samples, and I use a Bayesian GVAR model.

Furthermore, a new literature has used VAR models to estimate the impact of QE in Europe. For example, Boeckx et al. (2017) find that the expansionary effects of unconventional monetary policy are smaller in countries that have been more affected by the financial crisis. Burriel and Galesi (2018) find similar conclusions for countries with weak banking systems. To the extent of my knowledge, no study has estimated the local impact of the Fed in the aftermath of the financial crisis with VAR models.

Finally, this paper borrows from the literature that estimate the impact of an aggregate shock on lower-level economies. Authors use different estimation methods, namely panel VAR (Ciccarelli et al., 2013), Local Projections (Furceri et al., 2019), Restricted VAR (Beckworth, 2010; Boeckx et al., 2017), Global VAR (Georgiadis, 2015; Burriel and Galesi, 2018; Fischer et al., 2021) and Factor augmented VAR (Barigozzi et al., 2014; Potjagailo, 2017; Corsetti et al., 2018). This paper uses a Global Bayesian VAR, similar to Fischer et al. (2021).

2.3 Econometric Model

2.3.1 Global VAR

I estimate a Global VAR, which includes two blocks of equations, one for the Fed and one for the states. These two blocks can be thought of as VAR models with exogenous variables. Using the notation from Georgiadis (2015), the simplified version of the Fed and state blocks can be written as follows:

$$x_t^{(F)} = a^{(F)} + \Phi_1^{(F,F)} x_{t-1}^{(F)} + \Gamma_0^{(F,S)} x_t^{*(F,S)} + \epsilon_t^{(F)} \tag{2.1}$$

$$x_{it}^{(S)} = a_i^{(S)} + \Phi_{i1}^{(S,S)} x_{it-1}^{(S)} + \Gamma_{i0}^{(S,S)} x_{it}^{*(S,S)} + \Gamma_{i0}^{(S,F)} x_{it}^{*(S,F)} + \epsilon_{it}^{(S)} \tag{2.2}$$

for the Fed "F" and state "S".

Let us first consider Equation 2.1, which represents the set of equations for the Fed, including the Taylor Rule. These reduced form equations model the behaviour of k_F endogenous variables, contained in the vector $x_t^{(F)}$. As in a standard VAR, the first lag of the endogenous variables enter the right-hand side in $x_{t-1}^{(F)}$. In addition, aggregates of state-level variables enter the equations of the Fed through the (k_F^*)-dimensional vector $x_t^{*(F,S)}$. This vector is defined as weighted averages of state-level endogenous variables, i.e. $x_t^{*(F,S)} = \sum_{i=1}^N \omega_i^{(F)} x_{it}^{(S)}$, where the weights are states' contribution to the national GDP. Finally, the vector $a^{(F)}$ contains the constant terms and $\epsilon_t^{(F)} \sim \mathcal{N}(0_{k_F}, \Sigma_{\epsilon,F})$ is a white noise process with variance-covariance $\Sigma_{\epsilon,F}$.

The set of equations for the states follow a similar logic. The (k_i)-dimensional vector $x_{it}^{(S)}$ is regressed on its first lag $x_{it-1}^{(S)}$ as in a standard VAR. In addition, the (k_i^*)-dimensional vector $x_{it}^{*(S,S)}$ contains aggregates of state-level variables in bordering states, and represents the cross-state spillovers. Specifically, $x_{it}^{*(S,S)}$ is defined as $x_{it}^{*(S,S)} = \sum_{i=1}^N \omega_i^{(S)} x_{it}^{(S)}$, where $\omega_i^{(S)}$ are weights which equal 1 for bordering states and 0 otherwise. In addition, Equation 2.2

has a third added term, namely $x_{it}^{*(S,F)}$, which represents the impact of the Fed's policy on the states' economies. Indeed, in this model, $x_{it}^{*(S,F)}$ always equals $x_t^{(F)}$.[8] Finally, the vector $a^{(S)}$ contains the constant terms and $\epsilon_t^{(S)} \sim \mathcal{N}(0_{k_i}, \Sigma_{\epsilon,i})$ is a white noise process with variance-covariance $\Sigma_{\epsilon,i}$.

In the baseline model, the Fed's endogenous variable will be the Consumer Price Index, the Commodity Price Index, the Federal Funds Rate (FFR), and the natural logarithm of total reserves and monetary base ($k_F = 5$), while the state-level endogenous variables will be the Coincident Index for real economic activity (REA) and the unemployment rate ($k_F^* = k_i = k_i^* = 2$). Intuitively, in the simplified representation of the model outlined above, the weighted averages of states REA and unemployment enter the equations of the Fed through $x_t^{*(F,S)}$. Similarly, the weighted averages of bordering states' REA and unemployment enter a state's equations through $x_{it}^{*(S,S)}$, while the Fed's monetary variables and prices enter a state's equations through $x_{it}^{*(S,F)} = x_t^{(F)}$.

I will now briefly describe the solution of the global model. As shown in Georgiadis (2015), the two blocks of equations in 2.1 and 2.2, can be considered two different cross-sectional types, each including, respectively, one cross-sectional unit for the Fed and N cross-sectional units for the states. In general terms, $\mathcal{N}$ different cross-sectional types collected in the set $\mathbb{N}$ with each featuring $n^{(J)}$ can be written as follows:

$$x_{it}^{(J)} = a_i^{(J)} + \Phi_{i1}^{(J)} x_{it-1}^{(J)} + \sum_{S \in N, S \neq J} \Gamma_{i0}^{J,S} x_{it}^{*(J,S)} + \epsilon_{it}^{(J)} \tag{2.3}$$

for $i = 1, ..., n^{(J)}$ and $J \in \mathbb{N}$. This equation describes a standard Global VAR (GVAR). I can therefore use the logic of Pesaran et al. (2004), and reported in Georgiadis (2015) and Feldkircher and Huber (2016), to solve the global model. Specifically, by omitting the explicit differentiation between cross-sectional types for simplicity, I can re-write the model as follows:

$$x_{it} = a_i + \Phi_{i1} x_{it-1} + \Gamma_{i0} x_{it}^* + \epsilon_{it} \tag{2.4}$$

with $i = 0, ..., N$ and $\epsilon_{it} \sim \mathcal{N}(0_{k_i}, \Sigma_{\epsilon,i})$. In this GVAR, the Fed will be ordered first, i.e. x_{0t} will be the Fed's endogenous variables, with related set of equations. When allowing for multiple lags, the model can be generalised as:

$$x_{it} = a_i + \sum_{p=1}^{P} \Phi_{ip} x_{it-p} + \sum_{p=0}^{P} \Gamma_{ip} x_{it-p}^* + \epsilon_{it} \tag{2.5}$$

Note that, as in the simplified version, x_{it} is a vector of size k_i by 1, Φ_{ip} is a coefficient matrix of size $(k_i \times k_i)$, and Γ_{ip} is another coefficient matrix of size $(k_i \times k_i^*)$.[9] I now group the

[8] I keep the general notation in order to group terms in what follows.

[9] For the state equations, we effectively have two Γ_{ip} matrices, namely $\Gamma_{ip}^{(S,S)}$ and $\Gamma_{ip}^{(S,F)}$, which are, respectively, $(k_i \times k_i^*)$ and $(k_i \times k_0)$, as the Fed is ordered first.

contemporaneous and lag terms by defining the $(k_i + k_i^*)$-dimensional vector $z_{it} = (x'_{it}, x'^*_{it})'$, and the $(k_i \times k_i + k_i^*)$-dimensional matrices $A_i = (I_{k_i}, -\Gamma_{i0})$ and $B_{ip} = (\Phi_{ip}, \Gamma_{ip})$. The unit model can therefore be written as:

$$A_i z_{it} = a_i + \sum_{p=1}^{P} B_{ip} z_{it-1} + \epsilon_{it} \tag{2.6}$$

I now express this model in terms of a global (k)-dimensional vector $x_t = (x'_{0t}, x'_{1t}, ..., x'_{Nt})'$, where $k = \sum_{i=0}^{N} k_i$. To do so, I use a feasible link matrix W_i such that

$$A_i W_i x_t = a_i + \sum_{p=1}^{P} B_{ip} W_i x_{t-1} + \epsilon_{it} \tag{2.7}$$

Note that the link matrix W_i is of size $(k_i + k_i^* \times k)$. I can now stack the unit models to express the global model in its integrity. To do so, I define the (k)-dimensional vectors $\epsilon_t = (\epsilon'_{0t}, \epsilon'_{1t}, ..., \epsilon'_{Nt})'$ and $a = (a'_0, a'_1, ..., a'_N)'$, and the stacked $(k \times k)$-dimensional matrices $G = [(A_0 W_0)', ..., (A_N W_N)']$ and $H_p = [(B_{0p} W_0)', ..., (B_{Np} W_N)']$. The model in its global form can therefore be written as follows:

$$G x_t = a + \sum_{p=1}^{P} H_p x_{t-1} + \epsilon_t \tag{2.8}$$

with $\epsilon_t \sim \mathcal{N}(0_k, \Sigma_\epsilon)$, where Σ_ϵ is a block-diagonal matrix with Σ_{ϵ_i} in the main diagonal. After pre-multiplying by G^{-1}, and defining $F_p = G^{-1} H_p$ and $e_t = G^{-1} \epsilon_t$, I can express the global VAR in the reduced form of a standard VAR:

$$x_t = b + \sum_{p=1}^{P} F_p x_{t-1} + e_t \tag{2.9}$$

with $e_t \sim \mathcal{N}(0_k, \Sigma_e)$, where $\Sigma_e = G^{-1} \Sigma_\epsilon G^{-1\,\prime}$. This simple form is useful to describe the identification strategy used in this paper.

2.3.2 Identification

For the baseline model, I use a simple identification based on a Cholesky decomposition in the Fed's equations. As I order the Fed first in the Global VAR, the Fed's equations can be written as follows:

$$x_{0t} = a_0 + \sum_{p=1}^{P} \Phi_{0p} x_{0t-p} + \sum_{p=0}^{P} \Gamma_{0p} x^*_{0t-p} + \epsilon_{0t} \tag{2.10}$$

with $\epsilon_{0t} \sim \mathcal{N}(0_{k_0}, \Sigma_{\epsilon_0})$. By defining P_{k_0} as the lower Cholesky factor of Σ_{ϵ_0}, namely $\Sigma_{\epsilon_0} = P_{k_0}^{-1} P_{k_0}^{-1\,\prime}$, I can write the Fed's VAR in terms of its structural shocks:

$$x_{0t} = a_0 + \sum_{p=1}^{P} \Phi_{0p} x_{0t-p} + \sum_{p=0}^{P} \Gamma_{0p} x^*_{0t-p} + P_{k_0} \eta_{0t} \tag{2.11}$$

with $\eta_{0t} = P_{k_0}^{-1} \epsilon_{0t} \sim \mathcal{N}(0_{k_0}, I_{k_0})$. In order to write the full model, I can define the $(k \times k)$-dimensional matrix P as

$$P = \begin{pmatrix} P_{k_0} & 0 & \dots & 0 \\ 0 & I_{k_1} & \dots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \dots & I_{k_N} \end{pmatrix} \tag{2.12}$$

The full Global VAR expressed in terms of its structural shocks can thus be written as

$$x_t = b + \sum_{p=1}^{P} F_p x_{t-1} + P \eta_t \tag{2.13}$$

where $\eta_t \sim \mathcal{N}(0_k, \Sigma_\eta)$ and

$$\Sigma_\eta = \begin{pmatrix} I_{k_0} & 0 & \dots & 0 \\ 0 & \Sigma_{k_1} & \dots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \dots & \Sigma_{k_N} \end{pmatrix} \tag{2.14}$$

These structural forms highlight that the identification happens only at the level of the Fed. Following Christiano et al. (1999) and Ramey (2016), the ordering implies a first block with macro variables, namely CPI and commodity prices, a second block with the Shadow Federal Funds Rate, and a third block with monetary variables, namely total reserves and the monetary base. The structure of the model implies that the monetary-policy shocks identified with such ordering and Cholesky decomposition propagate to the rest of the economy.[10]

2.3.3 Bayesian Estimation

In this paper, I estimate the above-describe model on pre and post crisis periods, namely 1990-2007 and 2010-2019. Among other things, Bayesian methods allow to reduce issues related to small-sample estimation. These methods require an ex-ante structure on the models' parameters, which is implemented via a setting of the prior moments. For this Global

[10]In Appendix 2.A, I will augment the Fed's VAR model with other variables used in the literature, namely 10-year yields, stock-market index and an index for financial conditions.

VAR, I will be using the well-known Minnesota prior, which was first developed in Doan et al. (1984) and Litterman (1986). As the Global VAR requires a different shrinkage parameter for the (weakly) exogenous variables, I will be using the non-conjugate (independent) case of the Minnesota prior, which requires Markov chain Monte Carlo (MCMC) posterior simulation methods, such as the Gibbs sampler.

The Minnesota prior assumes that variables' time series mainly depend on the first own lag, while the other own lags and the lags of the other endogenous and exogenous variables play only a minor role. To implement this assumption, the coefficients' prior means are set to 1 for own lags and 0 otherwise. In addition, an informative Minnesota prior would set the coefficients' prior variance to decrease as the lag length increases. As a result, coefficients of distant lags are shrunk around 0, and matter little in explaining the time-series variation of the variables.

To express the Minnesota prior, it is convenient to work with a stacked version of the model, in which coefficients in Equation 2.5 are expressed in a vectorised form. The stacked version of endogenous variables is built by stacking all T observations on the first dependent variable, then all T observations on the second dependent variable, and so on.[11] Equation 2.5 can be expressed as

$$y_i = (I_{k_i} \otimes X_i)\alpha_i + \epsilon_i \tag{2.15}$$

y_i is a $(k_i T)$-dimensional vector of endogenous variables. X_i is a matrix containing the endogenous and exogenous right-hand-side variables and their lags of dimensions $T \times K_i$, where $K_i = 1 + Pk_i + Qk_i^*$. P is the number of lags of endogenous variables while Q is the number of lags of exogenous variables.[12] α_i is the vectorised version of coefficients, defined as $\alpha_i = (a_i', vec(\Phi_{i1})', ..., vec(\Phi_{iP})', vec(\Gamma_{i1})', ..., vec(\Gamma_{iP})')'$. This vector is of dimensions $k_i + Pk_i + Qk_i k_i^* = k_i K_i$. Finally, $\epsilon_i \sim \mathcal{N}(0_{k_i T}, \Sigma_i)$ is the $(k_i T)$-dimensional vector of white noises.

First, the approximation in the Minnesota prior assumes that Σ_i is diagonal. Following Feldkircher and Huber (2016), I use an Inverted Wishart prior on Σ_i:

$$\Sigma_i \sim \mathcal{IW}(\underline{S}_i, \underline{v}_i) \tag{2.16}$$

where $\underline{S}_i$ is a $k_i T \times k_i T$ prior scaling matrix, and $\underline{v}_i$ are the prior degrees of freedom. I set the related shape and rate hyperparameters equal to 0.01. Furthermore, in this paper I will allow stochastic volatility, as heteroscedastic error variances can be useful when the time period under study is volatile, as the post-crisis period (Clark, 2011). To set the related prior parameters, I follow the default values suggested by Bock et al. (2020). The

[11] For detailed information about the size of matrices, see Koop and Korobilis (2010).

[12] Endogenous variables enter at lag = 1, so $p = 1, ..., P$, while exogenous variables enter at lag = 0, so $q = 0, ..., Q$.

prior hyperparameter for the mean and variance of the log-volatilities are both 0, the two hyperparameters for the Beta prior on the persistence parameter of the log-volatilities are 25 and 1.5, and the hyperparameter for the Gamma prior on the variance of the log-volatilities is 1.[13]

Second, the Minnesota prior assumes random-walk processes for variables in levels. The coefficient of the first own lag will therefore be shrunk around 1 and the coefficients on the other own lags and other variables around 0. We can express the prior moments of the Minnesota prior on α as

$$\alpha_i \sim \mathcal{N}(\underline{\alpha_i}_{Min}, \underline{V_i}_{Min}) \tag{2.17}$$

The prior means $\underline{\alpha_i}_{Min}$ is set to 0, aside from the coefficient on the first own lag, which is set to 1. The prior variance-covariance matrices $\underline{V_i}_{Min}$ are assumed to be diagonal. By letting $\underline{V_i}_l$ being the block of $\underline{V_i}_{Min}$ associated with the K_i coefficients in equation l of the unit i and the $\underline{V_i}_{l,jj}$ being its diagonal elements, we can express the Minnesota prior on the variance covariance matrix as follows:

$$\underline{V_i}_{l,jj} = \begin{cases} \frac{\underline{a}_1}{p^2}, & \text{for coefficients on own lags} \\ \frac{\underline{a}_2 \sigma_{ll}}{p^2 \sigma_{jj}}, & \text{for coefficients on lags of variables } j \neq l \\ \underline{a}_3, & \text{for coefficients on exogenous variables} \end{cases}$$

As a result, the choice of the prior parameters for the coefficients' prior variance-covariance matrix boils down to choosing three parameters $\underline{a}_1$, $\underline{a}_2$ and $\underline{a}_3$. The hyperparameters on the lags are all scaled by the square of the lag length, so that the variance decreases as lag length increases and parameters are shrunk to zero, i.e. they count less than closer lags. In addition, the coefficients of lags on other variables are scaled by the ratio of respective variances to control for scaling differences. I set the initial parameter values as $a_1 = 0.1$, $a_2 = 0.2$ and $a_3 = 0.1$. Since I use the non-conjugate Minnesota prior, there are no closed-form solution for unconditional posterior moments. As such, I use the Gibbs Sampler, an MCMC algorithm, to repetitively draw from conditional distributions, for which the expressions of posterior moments are known. These draws of conditional distributions approximate well the unconditional distributions, and can be used for inference. For computational reasons, I use 500 draws, with no burn in and thinning factor of 1 (each draw is saved). Overall, the advantage of the Minnesota prior is that the number of hyperparameters to set is very low. The disadvantage is that the same shrinkage (a_3) is applied to contemporaneous values of x^*_{it}.

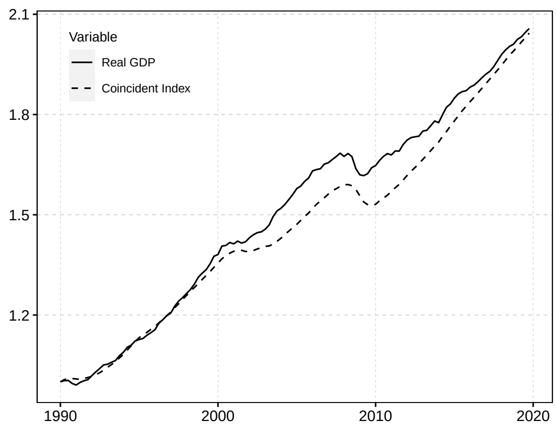
[13]For more information on the priors for stochastic volatility, see Fischer et al. (2021).

2.4 Data

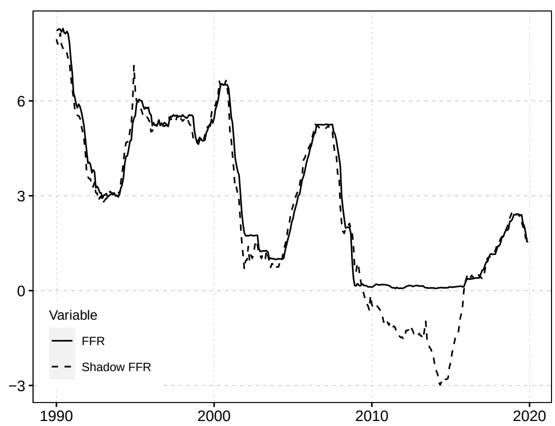
For this analysis, I build a dataset using different sources for both nation-level and state-level variables. The baseline samples include variables for real economic activity, unemployment, prices and monetary-policy variables over 1990-2019. To measure real economic activity at the state level, I refer to the Coincident Index provided by the Fed of Philadelphia. The Coincident Index is a composite index for real economic activity, computed for each state via a dynamic single-factor model including nonfarm payroll employment, average hours worked in manufacturing by production workers, the unemployment rate, and wage and salary disbursements deflated by the consumer price index at the U.S. city average (Crone and Clayton-Matthews, 2005).

Panel (a) of Fifure 2.1 plots the quarterly, indexed time series of the Coincident Index and the Real GDP chained at 2012 provided by the FRED. The Coincident Index follows closely the evolution of the Real GDP over the period of interest and can be considered a good proxy for real economic activity. As the Coincident Index is available for each state at the monthly frequency, I follow Beckworth (2010) and I use it as the main measure for real economic activity. The other state-level variable present in the model is the state-level unemployment rate provided by the U.S. Bureau of Labor Statistics.

Figure 2.1: Real Economic Activity and Policy Rate



(a) 1990



(b) 2000

Notes: Figure 2.1 plots the time series of Real Economic Activity (REA) and policy rate over 1990-2019. Panel (a) reports the time series of the real GDP, in chained dollar values (base year 2012), and of the Coincident Index for the United States, indexed to 1990. Panel (b) shows the time series for the Federal Fund Rate (FFR) and the Shadow FFR computed by Wu and Xia (2016).

Moving to aggregate variables, the variables in the Fed's VAR can be divided in three main blocks. Following Ramey (2016), the first block includes two measures of inflation. The Consumer Price Index is a measure of the average monthly change in the price for goods and services paid by urban consumers. Commodity Price Index is proxied with the Producer

Price Index (PPI) for all commodities, which measures the average monthly change in the selling prices on all commodities received by domestic producers for their output. [14] Both are sourced from the Federal Reserve's Economic Data.

The second block includes the policy variable. After the financial crisis, the main monetary-policy interest rate, the Federal Funds Rate, is characterised by the Zero Lower Bound (ZLB). I will therefore rely on the Shadow Federal Funds Rate (FFR) computed by Wu and Xia (2016) as the main measure of monetary policy. The Shadow FFR is computed with a shadow rate term structure model, which is more tractable than the standard Gaussian affine term structure model previously used in the literature. The model of Wu and Xia (2016) provides a Shadow FFR that closely follows the FFR in normal times, and simulates its path at the Zero Lower Bound. Panel (b) of 2.1 plots the time series of the FFR and the Shadow FFR over 1990-2019. After 2009, the FFR hits the ZLB, while the Shadow FFR keeps decreasing. The Shadow FFR hits the minimum as the Fed's Quantitative Easing programme turns to an end in 2014, to then re-join the FFR above zero in December 2015. Overall, the Shadow FFR appears to be a good proxy to measure monetary policy at the ZLB and I will be using it as a the policy rate in the model estimated over 2010-2019.

The third block of the Fed's VAR includes monetary variables, namely total reserves and the monetary base. Total reserves is the sum of total reserve balances maintained by the Fed plus vault cash used to satisfy required reserves. The monetary base is currency in circulation, demand deposits at commercial banks and other checkable deposits. In Appendix 2.A, I estimate a nation model augmented with other variables proposed by the literature. Among others, Miranda-Agrippino and Ricco (2021) augment their VAR model for the US with a stock-market index and treasury rates. I include the year-on-year change of stock-market Index S&P 500 and the 10-year Treasury constant maturity rate. In addition, as a measure of general financial conditions in the money, debt and equity market, I include the Financial Conditions Index (NFCI) by the Chicago Fed. Positive values of the NFCI indicate financial conditions that are tighter than average, while negative values indicate financial conditions that are looser than average. The summary statistics for all these monthly variables over 1990-2019 are summarised in Table 2.1.

Finally, I will estimate the GVAR model over two samples, namely 1990-2007 (pre crisis) and 2010-2019 (post crisis). The first windows begins in 1990 as the Shadow FFR provided by Wu and Xia (2016) starts in 1990, and ends in December of 2007, in order to exclude the financial crisis of 2008. The second windows starts in 2010, as measures of the length of the crisis for the USA report that the economic turbulence lasted until the second half of 2009.[15]

[14] The PPI for all commodities is composed of 15 major commodity groupings, which include farm products, processed foods and feeds, and industrial commodities (textile, fuel, chemicals, etc.). Detailed information can be found on the News Releases of the Bureau of Labour Statistics, here.

[15] The indicator I consider is the NBER based Recession Indicators for the United States, computed by the Federal Reserve of St. Luis.

Table 2.1: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	25th	75th	Max
USA							
Shadow FFR (%)	360	2.51	2.75	−2.99	0.48	5.14	8.14
Consumer Price Index (log)	360	5.25	0.20	4.85	5.08	5.43	5.55
Commodity Price Index (log)	360	5.04	0.21	4.74	4.83	5.26	5.34
Reserves (log)	360	5.22	1.76	3.63	3.81	7.37	7.95
Money (log)	360	7.35	0.46	6.67	7.01	7.73	8.30
10-Year Rate (%)	360	4.51	1.93	1.50	2.72	5.97	8.89
Equity S&P (yoy)	360	7.34	15.62	−55.35	2.67	17.07	41.98
Financial Conditions (yoy)	360	−140	1,635	−21,196	−32	35	4,560
STATES							
Real Economic Activity (log)	18,000	4.51	0.21	3.86	4.37	4.64	5.07
Unemployment (%)	18,000	5.45	1.87	2.10	4.10	6.50	14.60

Notes: Table 2.1 reports the summary statistics for the main variables used in the analysis. Shadow FFR is the Shadow Federal Funds Rate computed by Wu and Xia (2016). Consumer Price Index is a measure of the average monthly change in the price for goods and services paid by urban consumers. Commodity Price Index is proxied with the Producer Price Index for all commodities, which measures the average monthly change in the selling prices on all commodities received by domestic producers for their output. Reserves is the sum of total reserve balances maintained by the Fed plus vault cash used to satisfy required reserves. Money is currency in circulation, demand deposits at commercial banks and other checkable deposits. 10-Year Rate is the 10-year Treasury constant maturity rate. Equity S&P is the year-on-year change of stock-market Index S&P 500. Financial Conditions is the National Financial Conditions Index (NFCI) by the Chicago Fed. Real Economic Activity is the Coincident index for state-level real economic activity by the Fed of Philadelphia. Unemployment is the state-level unemployment rate reported by the Bureau of Labor Statistics.

The second window ends in 2019 to exclude the pandemic period. More than half of this sub-sample is characterised by the zero lower bound and unconventional monetary policies by the Fed.

2.5 Baseline Before the Crisis

The previous literature on the state-level impact the Fed's monetary policy focuses on contractionary measures. Conceptually, authors estimate state-by-state VARs with a Fed block and restrictions on coefficient matrices. They thus obtain impulse responses of state-level variables to a positive shock (increase) in the FFR. These papers therefore identify the regions or states in which this contractionary effect was larger. Here I consider the findings of five main papers of the literature, namely Carlino and DeFina (1998), Kouparitsas (2001), Crone (2005), Owyang and Wall (2009) and Beckworth (2010). Such findings can be grouped by main geographical areas in the United States. Regional references in this paper refer to the eight aggregate regions defined by the Bureau of Economic Analysis, based on states' economic characteristics. They are New England, Mideast, Great Lakes, Plains, Southeast, Southwest, Rocky Mountain and Far West.[16] Among the authors, there is a consensus that northern states, especially in the Great Lakes (e.g. Wisconsin and Michigan), Plains (e.g. Minnesota and Iowa) and Mideast (e.g. Maryland and Pennsylvania), register large reactions to monetary-policy shock. Crone (2005) and Beckworth (2010), and in part the other authors, also agree that the states that react less are grouped in the Southwest and Rocky Mountain regions, from Texas to Montana. This area is also referred to as the Energy Belt by Crone (2005). In principle, we would expect larger effects of monetary policy in regions where monetary-policy channels are more likely to work, given the characteristics of the local industrial, banking and house sector. The findings of the literature are generally in line with the logic of the interest rate channels, as the northern regions are the ones with larger shares of manufacturing jobs (Helper et al., 2012).

In order to benchmark the model with the rest of the empirical literature, I start by reporting the results for the pre-crisis sample, namely 1990-2007. Specifically, I obtain posterior estimates of the parameters in Equations 2.1 and 2.2 for the Fed and the states and I consider posterior medians to build the impulse-response functions of state-level variables to a cut in the policy rate. In my model, as in the other models of the literature, monetary policy shocks are assumed to be symmetric and, as such, states with larger negative reactions to an increase in the Shadow FFR are also the states with larger positive reactions to a decrease in the Shadow FFR. This is to say that, as far as the distribution of monetary-policy effects across states is concerned, it does not make a difference in considering either a contraction or an expansion.

Figure 2.2 maps the cumulative impulse responses of REA (panel a) and unemployment (panel b) 48 months ahead of a one-standard deviation cut in the Shadow FFR. The responses are grouped by their quartiles, i.e. four groups, with one colour per group. Panel (a) shows that Wisconsin, Michigan and Minnesota are all in the top quartiles of the distribution. This is in line with most of the papers mentioned above, which report large effects in the Great Lakes. In addition, the model estimates that also West Virginia and South Carolina register a large expansionary effect of monetary policy on REA, which is in line with the findings reported by Beckworth (2010).[17]

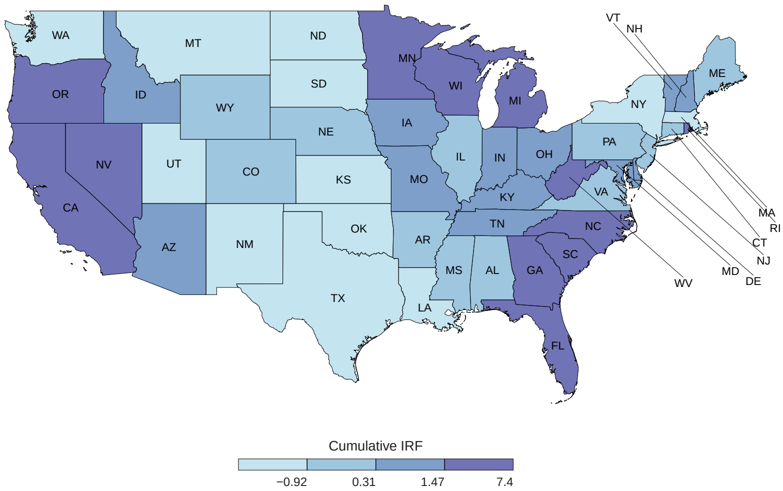
The model also estimates large expansionary effects for other states in the Southeast region

[16]The New England includes Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont. The Mideast includes Delaware, District of Columbia, Maryland, New Jersey, New York, and Pennsylvania. The Great Lakes includes Illinois, Indiana, Michigan, Ohio, and Wisconsin. The Plains includes Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, and South Dakota. Southeast includes Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia. The Southwest includes Arizona, New Mexico, Oklahoma, and Texas. The Rocky Mountain includes Colorado, Idaho, Montana, Utah, and Wyoming. The Far West includes Alaska, California, Hawaii, Nevada, Oregon, and Washington.

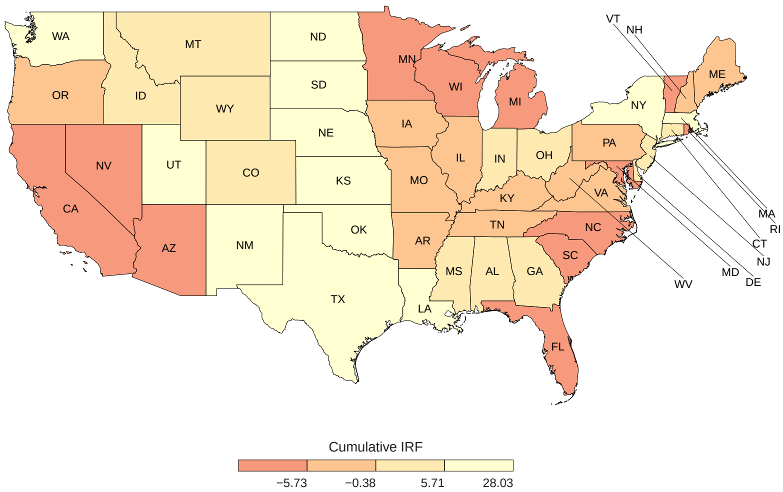
[17]Beckworth (2010) analyses a contractionary monetary policy, so he reports that West Virginia and South Carolina are among the states which did worse than the United States. As his model, like mine, assumes symmetric effects, it is implied that West Virginia and South Carolina would do significantly better than the United States in case of an expansion.

Figure 2.2: States' Responses Before the Crisis

(a) REA



(b) Unemployment



Notes: Figure 2.2 maps the geographical distribution of the states' cumulative responses (posterior medians) in real economic activity (panel a, in blue) and unemployment (panel b, in orange) for 48 periods ahead of a one standard-deviation decrease in the policy rate. Responses are grouped by their quartiles (four groups, with one colour per group).

(North Carolina, Georgia and Florida) and in the Far West region (California, Nevada and Oregon), which differs from Beckworth (2010).[18] However, in line with Beckworth (2010) and the rest of the literature, the model reports that the lowest effects (first quartile) are concentrated in the Energy belt, going from Montana and North Dakota to Oklahoma and Texas. Panel (b) reports the same type of impulse responses for unemployment. These results broadly follow the ones for REA, i.e. the largest decreases in unemployment are registered in the Far West, Great Lakes and Southeast regions.

[18] Carlino et al. (1999) estimate large effects for Oregon and Arizona, which is in line with my findings.

Overall, the states' reactions estimated by the GVAR for the pre-crisis period are generally in line with the previous literature. Differently from the literature, the model estimates large expansionary effects in other states with a large share of manufacturing production, like Georgia (with Atlanta), California and Oregon (Helper et al., 2012). This aspect is in line with dynamics in REA and unemployment related to the interest-rate channel of monetary policy. The model's results in the pre crisis therefore appear to be a good benchmark to which compare the reactions in the post-crisis period. I will be focusing on this comparison in the following sections.

2.6 The Fed

I start by reporting results for for Equation 2.1, namely the Fed. As mentioned, the endogenous variables in the Fed's equations can be divided into three main blocks. The first block includes two measures for the price levels (log transformed). The Consumer Price Index is a measure for core inflation in the United States, while the Commodity Price Index is an indicator for inflation on commodity prices. The second block includes the policy rate, which is the Shadow FFR computed by Wu and Xia (2016). The third block contains the monetary variables, which are total reserves and the monetary base (log transformed). With this ordering, I use the Cholesky decomposition illustrated in Equation 2.11 to obtain posterior orthogonal impulse responses of the endogenous variables following a one standard-deviation decrease in the Shadow FFR.

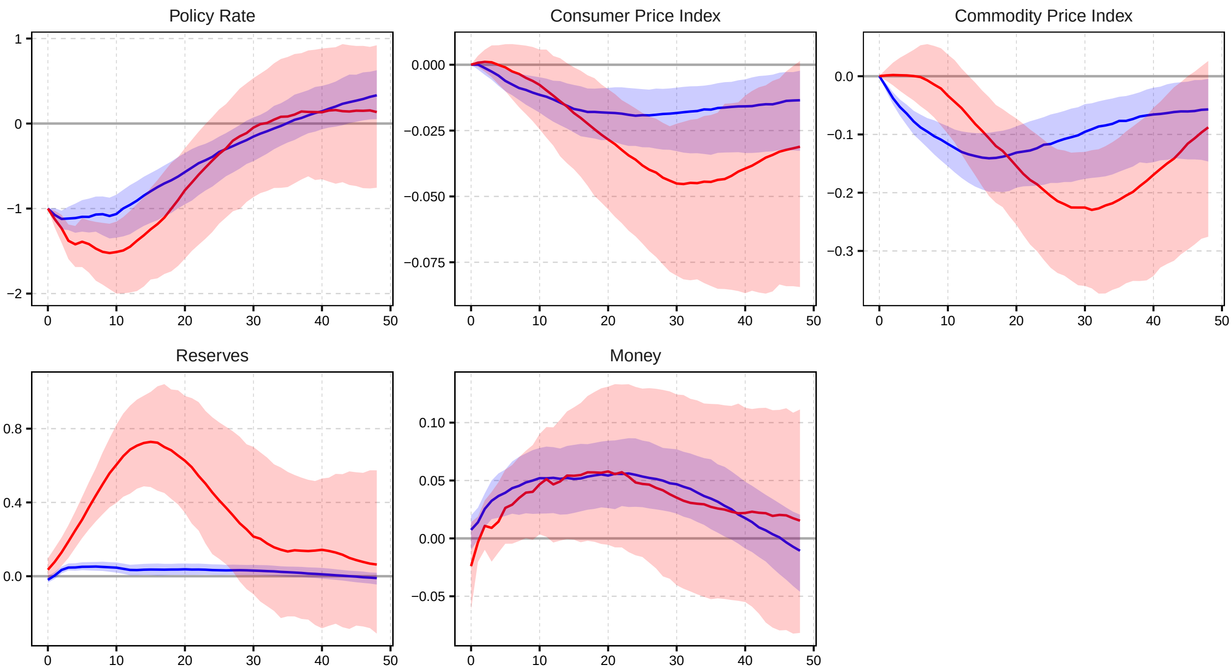
Figure 2.3 reports the results for 48 periods ahead of the shock. The blue and red lines are the IRFs obtained with, respectively, the pre-crisis and post-crisis sample. Following Fischer et al. (2021) and the majority of the Bayesian literature, I report the 68-% confidence intervals, estimated conditional draws of the Gibbs sampler (shaded areas).[19] The first panel illustrates the dynamics of the policy rate, which, as expected, goes back to zero around 30 periods ahead of its own shock, with no relevant differences between pre- and post-crisis samples. The second and the third panel show the movements in the CPI and the Commodity Price Index. In line with the majority of the literature, the model estimates a clear price puzzle, as prices decrease rather than increase. In addition, the decrease in prices seem to be more accentuated in the post-crisis sample. The first panel of the second row reports the response of total reserves. As expected, both in the pre- and post- crisis sample such reserves increase, to then go back to zero after around 30 periods ahead of the shock.[20] In addition, the total reserves increase much more in the model estimated on the post-crisis sample. This is also to be expected, as the Fed's reserves increased massively during the

[19] The recent literature on large-model Bayesian VAR considers the 68-% confidence intervals as the baseline (rather than 95 %). This is mostly due to the size of these models, which limits the significance of the results at the 68-% level.

[20] The confidence intervals meet the zero line around 30 periods ahead.

round of quantitative easing, as banks opened reserves accounts as soon as bond holders deposited liquidity.

Figure 2.3: National Responses



Notes: Figure 2.3 plots the posterior medians of the variables' impulse responses for 48 periods ahead following a one standard-deviation decrease in the policy rate. Blue and red lines refer to the model estimated over, respectively, 1990-2007 and 2010-2019. The policy rate is the shadow FFR. Shaded areas are the 68-% confidence intervals, drawn from the posterior distributions.

The last panel in the second row reports the reactions of the monetary base. As expected, the monetary base increases temporarily, to then go back to zero around 40 periods ahead for the pre-crisis sample, while the increase is not statistically significant in the post crisis. As the last two panels show, both reserves and monetary base react right away to a negative shock in the policy rate. This happens because they are part of the third block of the Fed's endogenous variables, which is ordered after the second block of the policy rate. On the other hand, both the indexes for consumer and commodity prices react with a lag, as they are in the first block of the model.

In Appendix 2.A, I report the results for the model estimated with different combinations of the Fed's endogenous variables. For computational reasons, I estimate the national model only, without the state-level equations. I thus substitute the exogenous national aggregates with endogenous national variables, as in a standard Bayesian VAR. Figure 2.A.1 reports the impulse responses for the endogenous variables of this model 48 periods ahead of a one standard-deviation cut in the policy rate. The responses for the policy rate, prices and monetary variables are generally similar to the ones presented in Figure 2.3. In addition, we now have the impulse responses for national REA and unemployment. They present general expansionary effects, i.e. REA increases and unemployment decreases, though these effects are more evident in the post-crisis sample. We will find that this larger effect is present also in the state-level variables. Among others, Feldkircher and Huber (2018) suggest one explanation behind the larger real effects of monetary policy in the aftermath the last financial crisis. In a nutshell, after the Fed has committed to a zero rate for a prolonged period of time, an unexpected deviation from such commitment can boost real output significantly. I will address these aspects later on in the paper.

The strong increase in the impulse responses of reserves in the aftermath of the financial crisis may be a reason of concern for the stability of the VAR. In Figure 2.A.2, I estimate the same model by excluding monetary variables. While the result on the policy rate remains approximately the same, the reactions both REA and unemployment in the pre-crisis sample (blue lines) present a more accentuated puzzle for the first periods ahead of the shock, to then overshoot around period 20. Also prices now differ more between pre- and post- crisis samples. These differences motivate the inclusion of monetary variables in the baseline model, as they contribute in pinning down the reactions of the rest of the variables.

Moreover, in order to increase precision and possibly reduce the price puzzle, the literature proposes the inclusion of other endogenous variables. For example, among others, Miranda-Agrippino and Ricco (2021) run robustness checks by including an index for the stock market and treasury rates. I include the year-on-year change of stock-market Index S&P 500 and the 10-year Treasury constant maturity rate. In addition, as a measure of general financial conditions in the money, debt and equity market, I include the Financial Conditions Index (NFCI) by the Chicago Fed. Figure 2.A.3 reports the results. The IRFs of the main variables remain approximately unchanged. However, the initial-period puzzles for both REA and unemployment are accentuated when these additional variables are included. Overall, the baseline specification, with prices, policy rate and monetary variables, appears to provide a good basis for the Fed's equations for both before and after the crisis. Indeed, results for the Fed remain generally similar in both samples and there are little or no initial puzzles on REA and unemployment. Also considering that the GVAR is already quite large in terms of the number of parameters estimated, and that such number would increase when adding endogenous variables in the Fed block, I rely on the baseline specification for the estimation of the global model. In the next section, I will present and comment the state-level results.

2.7 The States

2.7.1 Impulse Response Functions

I report here the results for the state economies. The posterior orthogonal IRFs for this section are derived from estimates of Equation 2.2. The endogenous variables are state-level

REA and unemployment for each state. The model is augmented with a first set of (weakly) exogenous variables, which includes aggregates of REA and unemployment in the bordering states and capture cross-state spillovers. In addition, the Fed's variables enter these equations through a third weakly exogenous vector. It is through this vector that monetary-policy shocks impact the economy of each single state.

Figure 2.4 plots the states' responses in REA (panel a) and unemployment (panel b) 48 periods after a one standard-deviation cut in the Shadow FFR. These responses are reported with box plots, which graph the distribution of all 48 state-level responses in each period (Alaska and Hawaii are excluded). The boxes represent the 25th, 50th and 75th percentiles of the states' response in each period. The lower and upper whiskers include states that are no further away than one and a half inter-quartile range from the, respectively, 25th and 75th percentiles. Outliers are defined as all states' responses that are outside of the whiskers' ranges. Blue and red boxes are for estimates obtained with, respectively, the pre- and post-crisis samples.

I start by considering the results in the pre-crisis sample, i.e. first sub-panel on the left, which mirror the cumulative results we saw in the map of Section 2.6. Some aspects stem out. First, the estimated median response to an expansionary monetary-policy shock across states is contained. It is even in the negative territory in the first periods after the shocks, and it becomes positive only around 25 periods ahead. Second, as shown in Section 2.6, there is a certain level of heterogeneity in the way states react to the expansionary monetary-policy shock. This heterogeneity increases as the periods ahead of the shock increase, with the first and third quartiles moving further away from the median. Also the tails of the distribution become wider, with some states that become outliers. Interestingly, we can see that the states of Michigan, Nevada, California and Florida are the ones that perform significantly better than the other states in this pre-crisis sample.

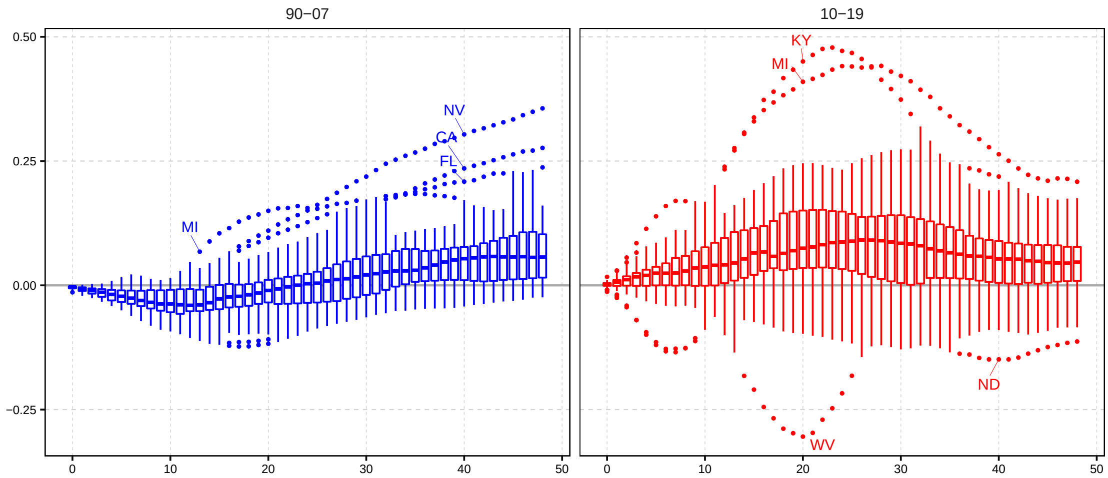
Let us now consider the same reactions estimated in the period after the crisis. Three main differences become evident. First, now the median (expansionary) effect on REA is larger, both on impact and throughout the considered periods. In other words, states appear to react generally better to the expansionary shock. Second, the heterogeneity of states' responses become much larger. This is true for the responses both within the first and third quartiles and on the tails. In addition, this heterogeneity is the largest between 20 and 35 periods ahead, and starts converging back around 40 periods ahead. Third, the outliers present some differences. While Michigan still registers quite large effects, California, Florida and Nevada no longer stem out as positive outliers. The model also estimates some negative effects for West Virginia - though they last only for about 10 periods - and Indiana towards the end of the time window.

Panel (b) shows a similar pattern for unemployment. Before the crisis, the cut in the policy rate implies a median reduction in unemployment, though only after 25 periods ahead.

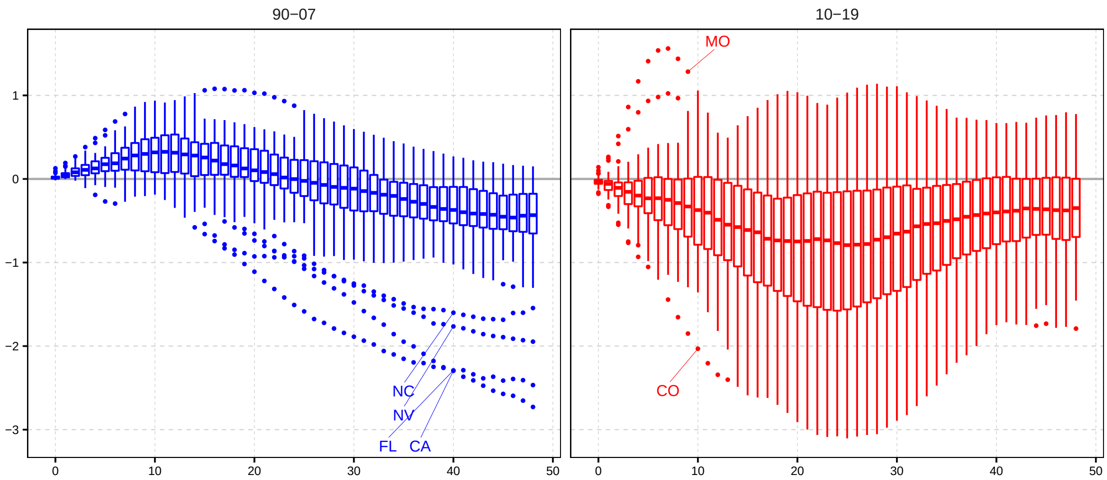
There is also a contained cross-state heterogeneity, which, somewhat differently from REA, materializes already in the first 10 periods, and remain approximately unchanged throughout the periods. As it was the case for REA, the expansionary impact of monetary policy on unemployment is the largest in California, Florida and Nevada.

Figure 2.4: States' Responses - Dispersion

(a) REA



(b) Unemployment



Notes: Figure 2.4 reports the distribution of states' posterior responses for each of the 48 periods ahead of a one standard-deviation decrease in the policy rate. Blue and red lines refer to the model estimated over, respectively, 1990-2007 and 2010-2019. The boxes represent the 25th, 50th and 75th percentiles of the states' response in each period. The lower and upper whiskers include states that are no further away than one and a half inter-quartile ranges from the, respectively, 25th and 75th percentiles. Outliers are defined as all states' responses that are outside of the whiskers' ranges. The policy rate is the Shadow FFR.

The last panel for the results in the post-crisis sample highlights how the trends that emerged for REA are even more remarked for unemployment. The median impulse response is negative in all analysed periods and the cross-state heterogeneity is considerably large.

State's reactions are more sparse both in the inter-quartile ranges and in the tails. However, there are generally less outliers, i.e. responses that are more than 1.5 inter-quartile ranges away from the first and third quartiles.
Overall, the models estimates that, in the post-crisis sample, an expansionary monetary measure by the Fed has larger median effects across states, and that states reactions are generally more heterogeneous. There are multiple reasons that could explain both these trends. One rationale is that monetary-policy shocks during quantitative easing were simply larger - and therefore played a larger role - than pre-crisis conventional measures. Larger shocks would lead to amplified reactions in all states, and therefore to a larger impact on average.

2.7.2 Analysis of the Forecast Error Variance

One way to analyse the importance of monetary-policy shocks relative to other shocks is the Factor Error Variance Decomposition (FEVD). The FEVD measures the shares of the forecast-error variance of, say, REA, explained by shocks in REA itself, REA of other states, national prices and the policy rate. In the setup of a Global VAR, the presence of cross-state correlation implies non-orthogonal shocks and the shares of the forecast-error variance typically exceeds unity. I therefore follow Feldkircher and Huber (2016) and I compute the Structural Generalised FEVD (SGFEVD). By construction, the SGFEVD implies shares that are between 0 and 1. Furthermore, while the shocks in the US model can still be interpreted as structural shocks identified by a Cholesky decomposition, the state-level shocks account for the contemporaneous relationships with the other shocks in the global system.[21]
Figure 2.5 reports the SGFEVD for both REA (panel a) and unemployment (panel b) for 25 periods ahead of the shocks. The shares reported here are median values of state-level shares. The shocks are grouped into five categories. "Own" are shocks to states' own variables, i.e own REA and unemployment. "Border States" are shocks coming from REA and unemployment of bordering states. These two groups represent the shocks stemming from local economies, while the remaining three embody nation-level shocks. "National Prices" are shocks to national prices, namely CPI and Commodity Price Index. Finally, "Money" and "Policy Rate" are shocks coming from the Fed, and respectively from either reserves or monetary base and the FFR.
The first sub-panel shows the SGFEVD for REA in the pre-crisis sample. It is evident how, in the pre-crisis, own shocks accounted for most of the forecast error variance of the global model. In line with the rest of the literature on global VARs and the findings of Beckworth (2010), own shocks explain all variation on impact, while its share decreases with the forecast horizon, as other shocks gain importance. In the case of US states, an important source

[21]For more information on the SGFEVD, refer to Dees et al. (2007) and Feldkircher and Huber (2016).

of forecast-error variance of REA in the pre-crisis period comes from shocks from either REA or unemployment in bordering states. Intuitively, this result suggests that real output of, say, Nevada, is positively affected by a shock in real output of California. Differently, shocks in national variables seem to play a minor role in explaining REA at the state level. Importantly, shocks in the policy rate account for a low share of FEVD throughout the reported periods. Together with the low impact on REA reported in Figure 2.4, the model generally estimates a limited effectiveness of the Fed in driving REA's in the United States before the crisis.

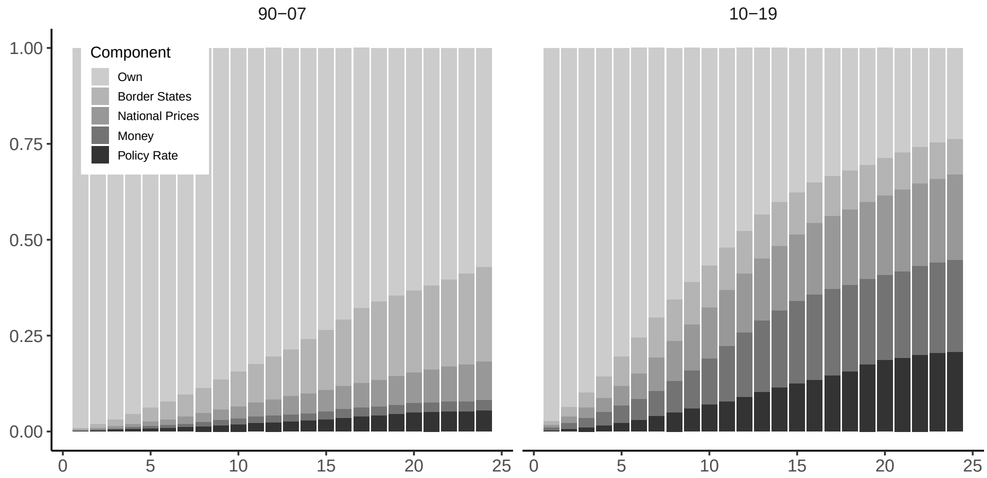
The second sub-panel of Panel (a) shows how the contribution of each shock changes significantly in the post-crisis sample. Three main differences arise. First, after 24 months, own shocks explain less than 25% of the forecast error variance (FEV) - against more than 50% before the crisis. Second, shocks on real variables from bordering states play a much smaller role. Third, and most importantly, shocks from national and Fed variables play now a major role. Specifically, after 24 months, shocks in national prices, monetary variables and the policy rate explain approximately one fourth each of the forecast error variance. Very similar patterns are estimated for unemployment, as it is reported in Panel b.

Overall, this analysis of the error variance highlights that the role of the Fed in driving states' real economy in recent years was larger than it was before the financial crisis. This difference in the effectiveness of the Fed could be due to what we can call *aggregate component*. Such aggregate component is determined by the nature of monetary-policy shocks or the states' position in the business cycle. On the one hand, in the pre-crisis sample, monetary policy was conducted systematically and agents could anticipate changes in the FFR. As a result, there were no true monetary-policy shocks, and what the model grasps is only a fraction of the true effect of monetary policy (Ramey, 2016). In the post-crisis period, the Fed committed to zero rates for a prolonged period of time. A deviation from that commitment could have come as unexpected for the market, thus leading the model to estimate larger real effects (Feldkircher and Huber, 2018). On the other hand, larger stimulus effects could be due to a post-crisis convergence of states' economies in the initial phase of their business cycle. While before the crisis their position in the cycle was heterogeneous, the crisis might have erased those differences. As states went back to the beginning of the cycle, they reacted better to a monetary-policy stimulus. In Appendix 2.B and Section 2.8, I report anecdotal and regression-based evidence in support of this hypothesis.

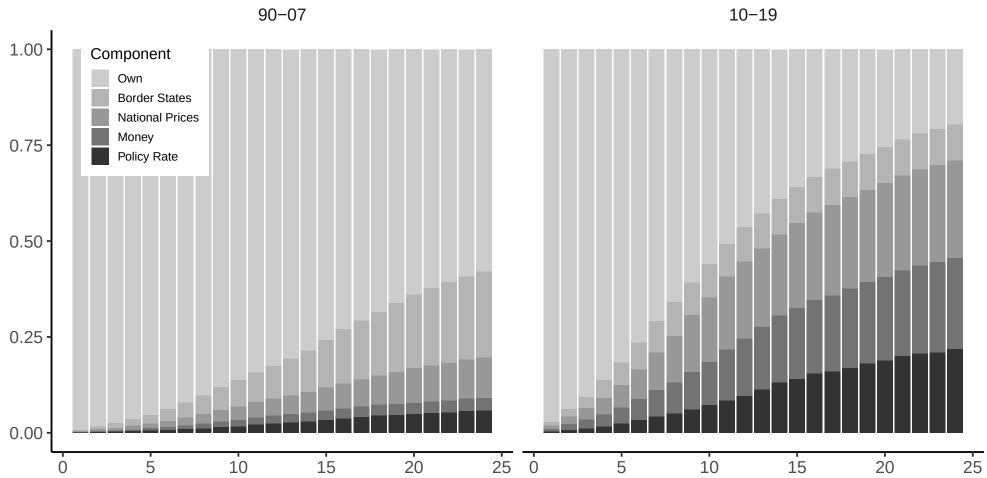
In either way, the final outcome of a change in the aggregate component as described would be a larger average effect. However, a change in the aggregate component alone does not explain the larger heterogeneity. Indeed, all states would simply react more, and the distribution around the median would remain unchanged. Something else must have changed in the characteristics of states' economies, thus changing the local effectiveness of monetary policy

Figure 2.5: FEVD

(a) REA



(b) Unemployment



Notes: Figure 2.5 reports the Structural Generalised Forecast Error Variance Decomposition (SGFEVD) for Real Economic Activity (REA) and Unemployment in the Global VAR. The shocks are grouped by own variable (REA or unemployment), variables in bordering states (REA or unemployment), national prices (Consumer Price Index and Commodity Prices), money (reserves and monetary base) and the policy rate (Shadow Federal Funds rate). Panel (a) reports the results for REA, while Panel (b) reports the results for unemployment.

and increasing heterogeneity.[22] In Section 2.8 I will be focusing on changes to this component,

[22]Intuitively, we can consider an example with two states, T (Texas) and C (California). C has generally higher house prices than T. Before the crisis, both states react positively to a monetary expansion, though C reacts better, has it has higher prices. C's IRF is slightly above T's for some time, and then they converge back to equilibrium. There is some distance between the two IRFs, but not much (low heterogeneity). After the crisis, house prices collapse in C, while remain the same in T. T's IRF remains approximately unchanged. However, C's IRF converges to equilibrium much faster now. As C's IRF is significantly below T's for the majority of the considered periods, the distance between them increased significantly (more heterogeneity). In addition, the more quantitative easing has a positive impact on T, and the more the house-price collapse keeps C down, the more this distance increases.

which is specific for each state and that we can refer to as the *idiosyncratic component*.

2.8 State Responses and the House-Price Bubble

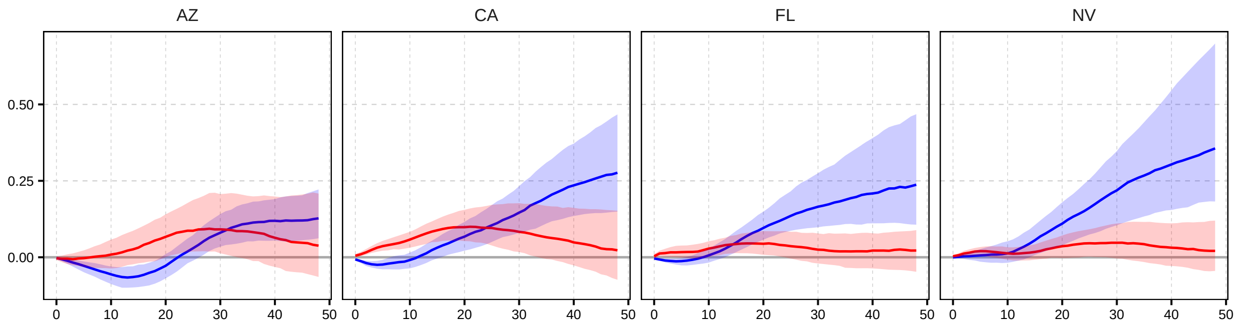
A monetary-policy expansion produces real stimulus by dragging down the cost of capital, pushing investment spending and increasing aggregate demand. This is the main channel of monetary policy, which can be referred to as the interest-rate channel (e.g. Mishkin, 1996). The literature has identified several other channels, that are induced by monetary policy itself and serve as amplification channels. Most of them are related to a policy-induced change in the value of assets agents hold, similar to the financial-accelerator channel for business cycles (Bernanke et al., 1999). For example, the balance-sheet and bank-lending channels of monetary policy - also known as broad and narrow credit channels - theorise that a decrease in the policy rate increases the size of borrowers' (firms) and lenders' (banks) balance sheets and therefore their ability to, respectively, access and grant credit (e.g. Bernanke and Blinder, 1988; Kashyap et al., 1994; Bernanke and Gertler, 1995). The so called house-price channel of monetary policy predicts a similar logic for houses. Intuitively, a cut in the Fed rate would drive house prices up. As individuals can use their house as collateral to access credit, they would extract home equity to finance current consumption (Elbourne, 2008). A similar mechanism can take place with constant prices through mortgage refinancing. As the Fed policy rate decreases, mortgage rates follow, and households can refinance their mortgage at lower rates and use the money saved to finance consumption (Beraja et al., 2019).
In this Section, I will focus on the cross-sectional dimension of these channels. For example, with regards to the amplification channels, I will consider the cross-state distribution of assets as given at a point in time and analyse how it affects the functioning of such channels. This is slightly different than considering a policy-induced change in asset value. Specifically, I will focus on how cross-state differences in house prices can affect the real effects of monetary policy across states.

2.8.1 Arizona, California, Florida and Nevada

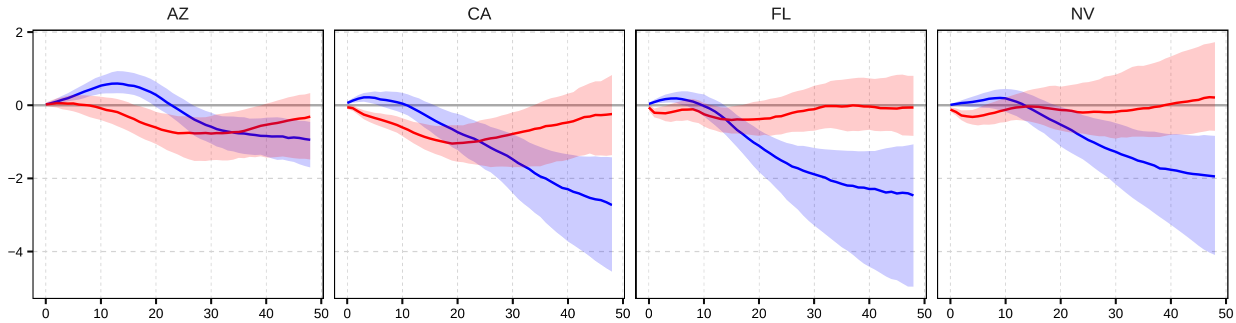
The financial crisis of 2008 was a large negative shock for house prices throughout the United States. However, some states were more severely affected than others. For example, Wang (2019) shows that the largest boom-bust shocks in house prices over 2005-2013 were registered in Arizona, California, Florida and Nevada. It is therefore reasonable to believe that the house-price channel may have worked less in these states in the aftermath of the financial crisis. Indeed, with low house prices and therefore low collateral value, households in these states would not be able to refinance their mortgage as rates decrease. I start to explore trends in support of these hypothesis by looking at the impulse responses for Arizona, California, Florida and Nevada. They are reported in Figure 2.6. Panel (a) and (b) show results for, respectively, REA and unemployment in these four states. As usual, they are the posterior median responses 48 periods after a one-standard deviation cut in the Shadow FFR, computed with parameters of the states' equations in the GVAR. Blue and red colors refer to IRFs for, respectively, pre- and post-crisis samples.

Figure 2.6: Arizona, California, Florida and Nevada

(a) REA



(b) Unemployment



Notes: Figure 2.6 plots the posterior median impulse responses of real economic activity in the considered states for 48 periods ahead of a one standard-deviation decrease in the policy rate. Blue and red lines refer to the model estimated over, respectively, 1990-2007 and 2010-2019. The policy rate is the shadow FFR. Shaded areas are the 68-% confidence intervals, drawn from the posterior distributions.

A clear pattern emerges, both for REA and unemployment. I first consider the responses for REA, starting with the responses in the pre crisis (blue lines). In all states, after an initial period of mild reactions, a cut in the policy implied a long-term stimulus on real economic activity.[23] These effects were especially large in Nevada, followed by Florida and California. This scenario changes for responses in the post-crisis sample (red lines). Initially - and in line with what discussed in Section 2.7 - a monetary expansion after the crisis triggers positive reactions in all states. However, states' REA converges back to equilibrium much faster than in the pre-crisis sample. This difference is especially evident for Nevada, where

[23]The positive effect took some time to kick in. In Arizona, it took around 20 months, while in the other states it took about 10 months.

the response of REA in the post-crisis sample is almost never statistically significant. The response becomes insignificant after 20 months in Florida, and after 30 months in California and Arizona.

Panel (b) shows a similar pattern for unemployment. In the pre-crisis sample, California registers the largest long-lasting reduction in unemployment, followed by Florida, Nevada and Arizona. As it was the case for REA, the estimated reduction in unemployment is much milder after the financial crisis in these four states. The IRFs are almost never statistically significant for Florida and Nevada, while converge back to zero after 30 months in Arizona and Florida.

Overall, Figure 2.6 reports that the expansionary effects of monetary policy on both REA and unemployment last significantly less in the post-crisis sample, as economies converge back to equilibrium faster. As these four states were the ones more severely affected by the house-price bubble of 2008, it is possible that households in those states were not able to refinance their mortgage and thus the house-price channel could not work as before.

2.8.2 Regression Analysis for All States

It is interesting to assess whether these trends hold only in these outliers, or whether we can find a more consistent pattern across all states. In principle, I would expect that states that registered the largest boom-bust shocks of house prices were also the ones with the largest reduction in the effectiveness of monetary policy.

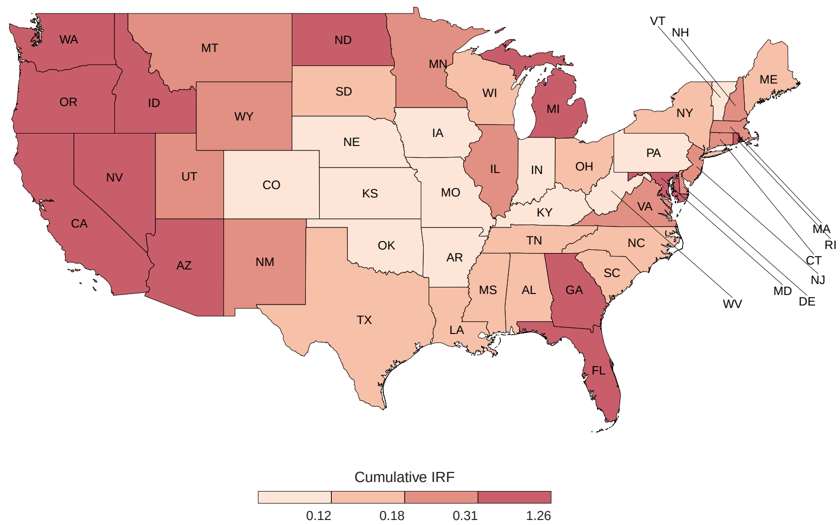
Figure 2.7 maps these two dimensions across all states. Panel (a) reports the post-crisis changes in house prices for each state. I follow Wang (2019) and I compute these differences as the relative change between maximum and minimum value of house prices over 2005-2013. Large values indicate large drops in house prices, i.e. large boom-bust shocks. Panel (b) maps the post-crisis difference in state-level REA's cumulative IRFs for 48 periods after a one-standard deviation cut in the Shadow FFR. Specifically, I compute the relative change between the cumulative response after the crisis and the cumulative response before the crisis. Negative values refer to a decrease in the state's cumulative response. Values for both variables are divided in quartile groups, with one colour for each quartile group.

Panel (a) shows that, as mentioned, Arizona, Nevada, California and Florida are all in the top-quartile group of boom-bust shocks to house prices (dark red). These shocks were focused in the West, as also Washington, Oregon and Idaho are in the top quartile group. On the other hand, the Central regions did not register sharp decreases in house prices, as states like Colorado, Nebraska, Kansas, Oklahoma, Iowa, Missouri and Arkansas are all in the bottom-quartile group.

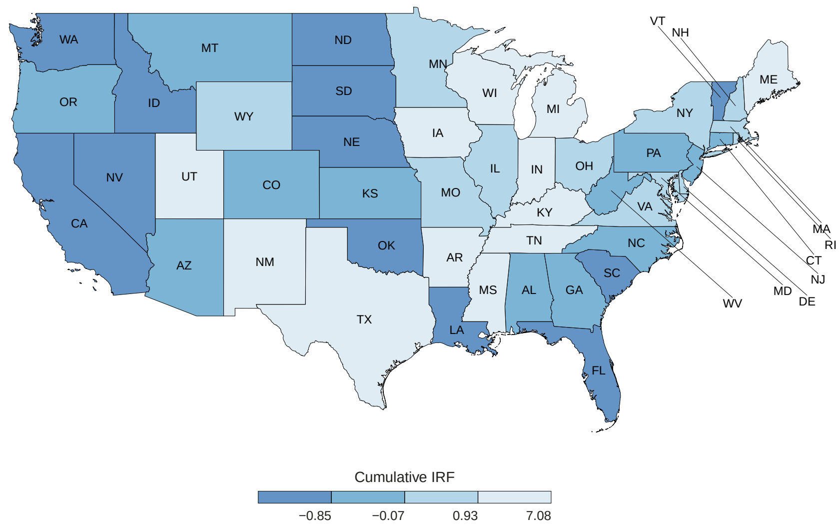
Panel (b) suggests that there is a certain degree of correlation with post-crisis differences in the impact of monetary policy. California, Nevada and Florida are all in the bottom-quartile

Figure 2.7: House Prices and Responses to Monetary Policy

(a) HPI Shock



(b) Pre-Post Differences



Notes: Figure 2.7 maps the geographical distribution of the pre-post crisis differences in states' cumulative responses in real economic activity 48 months ahead of a one standard-deviation cut in the policy rate (panel a, in blue) and in house prices (panel b, in blue). These differences are grouped by their quartiles (four groups, with one colour per group). Differences in house prices are defined as the percentage change between the state-level maximum and minimum value of house prices over the period 2005-2013.

group of such differences, while Arizona is between the median and the third quartile. Other Western states that registered large house-price shocks, such as Washington, Oregon and Idaho, are all below median. This is also the case for Georgia in the East. While this pattern is clear, there are states that were not subject to house-price shocks but that, nonetheless, reacted relatively worse than before to a monetary stimulus. For example, South Dakota, Nebraska and Oklahoma are all in the bottom-quartile group. These discrepancies suggest that there can be state-specific factors other than boom-bust shocks in house prices that can explain post-crisis differences in the way states react to a monetary expansion.

I can control for these factors in a cross-sectional regression setup. My dependent variable is states' responses to a one standard-deviation cut in the policy rate estimated through the Bayesian GVAR. I start by considering two samples of responses, namely before and after the crisis. I regress such responses on independent variables of interest. For all dependent variables, I consider the state-level averages for the pre and post-crisis periods.

The main variable of interest is the House Price Index, which is the simplest measure to grasp cross-state differences in house prices. Specifically, I use the All-Transactions House Price Index (HPI) provided by the Federal Housing Finance Agency (FHFA), deflated it with the Consumer Price Index at the national level.[24] I expect a generally positive coefficient, as individuals in states with largest house prices should be able to refinance their mortgage, which boost the effect of monetary policy. In addition, this mechanism should become stronger in the post-crisis sample, as cross-state variation in house prices should be larger. I thus expect the positive coefficient for the post-crisis sample to be larger than for the pre-crisis sample.

I then control for other cross-state characteristics that could explain different reactions across different states. The first two variables measure how channels of monetary policy can work differently in different states. First, the interest-rate channel - i.e. the main channel of monetary policy - indicates that firms react to changes in interest rates by adapting their capital investment. In principle, capital-intense firms, such as manufacturing firms, are more sensitive to interest-rate changes than firms in other sectors (Furceri et al., 2019). The states' industry mix therefore determines the functioning of the interest-rate channel. States with high concentration of manufacturing firms would see larger effects of monetary policy than firms with high concentration of, say, service firms. I measure the industry mix with the manufacturing share of the Gross State Product (GSP). I obtain this variable from the annual GSP in current dollars constructed by the Bureau of Economic Analysis (BEA) at the industry level.[25]

Second, the broad credit channel suggests that cuts in interest rates boost firms' asset values, thus reducing the external finance premium and allowing access to credit. If we assume firms' size to be fixed - and not endogenously determined by the Fed rate -, we would expect monetary policy to work less in states with higher concentration of small firms.[26] I control

[24]The same methodology is used by Furceri et al. (2019).

[25]In 1997, BEA has updated the definitions of the industrial sectors for this variable, which since then follow the NAICS classification. To avoid issues related to a change in the classification, I consider only post-1997 data to compute the state-level data points for the pre-crisis levels (rather than including also pre-1997 data). Note that these are shares of manufacturing production within each state, which is different from shares of manufacturing production with respect to the United States as a nation.

[26]Small firms have less access to credit than large firms.

for this logic with employment accounted for by a state's small firms. I obtain the data from the U.S. Small Business Administration, Office of Advocacy, based on data provided by the U.S. Census Bureau, and I define small firms as firms with less than 100 employees.[27]
Furthermore, I consider a second set of controls for the OCA criteria, based on the variables proposed by Beckworth (2010). I thus compute the correlation between the state's and USA's monthly growth rate of the coincident indicator before and after the 2008 crisis as a measure for similarity in business cycles.[28] To measure wage flexibility, I consider the average percent deviation of a state's hourly manufacturing wage from the US hourly manufacturing wage, obtained from the Bureau of Labor Statistics. Finally, I compute the state-level implicit price deflator by dividing the nominal GSP in current dollars by the real GSP in chained-dollar value and I use it to compute the yearly inflation rate by state. I then consider the difference between the state's and USA's inflation rate as a measure for price flexibility. The data on GSP is sourced from BEA.

Table 2.2 presents the regression results. Panel A and B report the results for responses of, respectively, REA and unemployment. Odd- and even- numbered columns show the results for responses obtained with, respectively, the pre- and post- crisis samples. In order, the type of responses reported are cumulative responses for 36 and 48 periods ahead of the shock, and minimum, maximum and peak values of responses over 48 periods ahead of the shock.[29]
Columns 1 and 2 show that the HPI and other controls generally do not explain the cross-state differences of cumulative responses 36 months ahead of the cut in interest rates. Only the coefficient of similar business cycles is statistically significant at conventional levels in the pre-crisis sample. The positive value of .566 suggests that states that were more similar to the US registered the largest responses.[30] The coefficient becomes statistically insignificant after the crisis, which suggest that differences in business cycles were no longer a relevant explanatory factor. This evidence is in line with the logic on business cycles discussed in Section 2.7. As the crisis pushed all states back to the initial phase of the business cycle, cross-state differences in output are low and do not longer explain differences in how states react to monetary policy.

Columns 3 and 4 highlight some post-crisis differences for house prices and industry mix. The coefficient of HPI is positive and significant at the 10-% level in the post-crisis sample (column 4), while it is insignificant in the pre-crisis sample (column 3). Specifically, the coefficient estimate of 1.212 indicate that a one-standard deviation increase in the HPI leads

[27] This methodology is similar to the one used by Furceri et al. (2019), just they define small firms as the ones with less than 250 employees. I defined it with less than 100 employees as this was the classification reported by the Small Business Administration office.

[28] The considered year-on-year monthly growth rates are the ones reported in Figure 2.B.1.

[29] Results for cumulative responses 12 and 24 months after the shock are not reported. In general, explanatory variables are never significant for 12 and 24 months ahead.

[30] This is somewhat in line with Beckworth (2010). However, Beckworth (2010) considers absolute deviations from the US as the left-hand side, and he can thus interpret the results as the states being more or less similar to the US. As I have raw responses as dependent variable, I cannot interpret the sign of this coefficient.

Table 2.2: Monetary-Policy Heterogeneity - Before and After the Crisis

	Cum. 36		Cum. 48		Min		Max		Peak	
	(1) Pre	(2) Post	(3) Pre	(4) Post	(5) Pre	(6) Post	(7) Pre	(8) Post	(9) Pre	(10) Post
Panel A: REA										
HPI	−0.352	0.780	−0.352	1.212*	−0.004	0.026**	−0.0002	0.015	0.002	0.056**
	(0.260)	(0.537)	(0.390)	(0.670)	(0.006)	(0.011)	(0.013)	(0.020)	(0.018)	(0.027)
Manuf. Share	−0.297	0.799	−0.638	1.146*	0.005	0.018*	−0.033**	0.018	−0.023	0.040
	(0.292)	(0.486)	(0.437)	(0.606)	(0.006)	(0.010)	(0.015)	(0.018)	(0.021)	(0.025)
Small Firms	−0.183	−0.242	−0.388	−0.427	0.005	0.00003	−0.018	−0.010	−0.021	−0.017
	(0.251)	(0.422)	(0.376)	(0.527)	(0.005)	(0.008)	(0.013)	(0.015)	(0.018)	(0.021)
Similar BC	0.566*	−0.141	0.871*	−0.032	0.012*	0.024***	0.026*	−0.028*	0.030	−0.017
	(0.291)	(0.411)	(0.435)	(0.513)	(0.006)	(0.008)	(0.015)	(0.015)	(0.021)	(0.021)
Flex. Wages	0.101	0.251	0.120	0.162	−0.004	−0.003	0.004	0.012	0.002	0.0002
	(0.212)	(0.428)	(0.317)	(0.535)	(0.005)	(0.008)	(0.011)	(0.016)	(0.015)	(0.022)
Flex. Prices	−0.258	0.588	−0.402	0.698	0.003	0.009	−0.013	0.020	−0.019	0.028
	(0.304)	(0.406)	(0.455)	(0.507)	(0.007)	(0.008)	(0.015)	(0.015)	(0.021)	(0.021)
Constant	−0.191	2.189***	0.571*	2.804***	−0.046***	−0.032***	0.080***	0.126***	0.055***	0.094***
	(0.198)	(0.387)	(0.297)	(0.483)	(0.004)	(0.008)	(0.010)	(0.014)	(0.014)	(0.020)
Adjusted R^2	0.087	0.065	0.124	0.101	0.019	0.253	0.198	0.046	0.122	0.099
Panel B: Unemployment										
HPI	1.278	−7.280	0.518	−10.391*	−0.062	−0.170	0.011	−0.193**	−0.087	−0.468**
	(2.141)	(4.440)	(3.157)	(5.497)	(0.102)	(0.152)	(0.050)	(0.078)	(0.164)	(0.212)
Manuf. Share	1.290	−8.008*	3.163	−11.048**	0.169	−0.281**	−0.049	−0.087	0.084	−0.364*
	(2.402)	(4.018)	(3.542)	(4.974)	(0.115)	(0.138)	(0.057)	(0.071)	(0.184)	(0.192)
Small Firms	−0.453	3.027	0.785	4.249	0.114	0.103	−0.102**	0.041	0.010	0.253
	(2.065)	(3.492)	(3.045)	(4.324)	(0.099)	(0.120)	(0.049)	(0.062)	(0.158)	(0.167)
Similar BC	−4.531*	−4.018	−7.371**	−4.998	−0.237**	−0.052	−0.048	−0.097	−0.217	−0.097
	(2.391)	(3.396)	(3.527)	(4.205)	(0.114)	(0.116)	(0.056)	(0.060)	(0.183)	(0.162)
Flex. Wages	0.199	−1.310	1.143	−0.541	0.053	−0.086	0.017	0.053	0.112	0.024
	(1.740)	(3.542)	(2.565)	(4.386)	(0.083)	(0.121)	(0.041)	(0.062)	(0.133)	(0.169)
Flex. Prices	1.604	0.215	1.786	0.125	0.037	−0.008	0.006	0.009	0.137	0.022
	(2.498)	(3.361)	(3.684)	(4.161)	(0.120)	(0.115)	(0.059)	(0.059)	(0.192)	(0.161)
Constant	1.784	−20.144***	−4.026	−24.816***	−0.602***	−1.072***	0.394***	0.218***	−0.247*	−0.832***
	(1.632)	(3.198)	(2.406)	(3.960)	(0.078)	(0.110)	(0.038)	(0.056)	(0.125)	(0.153)
Adjusted R^2	0.009	0.083	0.068	0.112	0.195	0.048	−0.012	0.095	0.017	0.110
Observations	48	48	48	48	48	48	48	48	48	48

Notes: Table 2.2 reports coefficient estimates for regressions of the responses in states' real economic activity (REA) and unemployment to a one standard-deviation cut in the policy rate on state-level explanatory variables. Cum. 36 and Cum. 48 are cumulative responses for 36 and 48 months ahead of the shock. Min, Max and Peak are minimum, maximum and peak responses over a 48-month window. HPI is the state's House Price Index. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

to an increase in the states' response for REA of 1.212 standard deviations. This finding suggests that, after the crisis, states with larger house prices register larger responses, while this is not the case before the crisis. Columns 5 and 6, and 9 and 10 show a similar logic for minimum and peak responses, where results are significant at the 5-% level. Columns 7 and 8 report similar signs for the coefficients, though the positive coefficient in the post-crisis sample is not significant at conventional standards.

The other coefficients that are at times significant are the ones for manufacturing share and similar business cycles. However, sign and significance change depending on the type of response considered. For example, the coefficient on manufacturing share is positive and significant in the post-crisis sample when cumulative responses for 48 periods ahead of the shock and minimum responses are used (columns 4 and 6), though these results are not significant at the 5-% level. In addition, the coefficient for the pre-crisis sample becomes negative and significant when using maximum responses, which is puzzling. Also the coefficient for similarity in business cycles present confounding results, as it moves from positive to negative (and significant) in the post-crisis for minimum and maximum responses (columns 6 and 8). Overall, I do not find stable evidence for a difference in how the manufacturing share and similarity in business cycles affect states' responses in REA before and after the crisis. On the other hand, results on house prices are more stable across regressions, as the related coefficient becomes a significant explanatory variable in the post-crisis sample.

Panel B reports results for unemployment, which provide a somehow clearer cut for the results throughout the specifications than we had for REA. As responses of unemployment is the dependent variable, the expectations on all signs are now inverted. Results for the HPI mirror the ones for REA. The negative and significant coefficients in columns 4, 8 and 10 suggest that states with higher house prices registered a larger decrease in unemployment in the aftermath of the financial crisis. This channel was not there before the crisis, as coefficients in columns 3, 7 and 9 are not significant.[31] Differently than for REA, coefficient estimates for manufacturing shares clearly suggest that states with larger shares of manufacturing production recorded larger decreases in unemployment in the post-crisis sample.

The 2008 financial crisis per se should not have induced major changes in the geographical distribution of manufacturing firms in the US. However, in the post-crisis sample manufacturing shares are about 18-% lower on average than in the pre-crisis sample. These changes in states' industry mix may reflect a transition to an economy more centered towards services. While this is true on average, in some states this contraction is larger than in other states. These idiosyncratic (post-crisis) differences may drive the significance in the results for the industry mix in the post-crisis sample.[32] Also the results on the similarity of business cycles are somehow neater for unemployment. Related coefficients in columns 1 to 6 show that, while similarity between states' and United States' business cycles explained unemployment's reactions before the crisis, it was not a relevant factor after the crisis.

Overall, regressions in Table 2.2 suggest that differences in house prices can explain differences in states' reaction to an expansionary monetary policy in the post-crisis sample, while they were not a relevant factor before the crisis. In Table 2.3 I test this finding with a selection of robustness checks. For brevity, I report only the results obtained with cumulative responses 48 periods ahead of the shock as dependent variable.

[31]The difference with REA is that now the results with minimum responses are not significant at conventional standards, while the results with maximum responses are significant. This makes sense when considering that IRFs of unemployment are inverted.

[32]Simply put, the difference with the pre-crisis sample might be driven by differences in the right-hand side - as it is for house prices and similarity of business cycles - rather than in the left-hand side.

Table 2.3: Robustness Checks

	Dependent variable:											
	REA						Undemployment					
	HPI Shock		Drop Outl.		Drop Outl. HPI		HPI Shock		Drop Outl.		Drop Outl. HPI	
	(1) Pre	(2) Post	(3) Pre	(4) Post	(5) Pre	(6) Post	(7) Pre	(8) Post	(9) Pre	(10) Post	(11) Pre	(12) Post
HPI Shock	1.550***	0.673					−12.873***	−1.411				
	(0.301)	(0.601)					(2.358)	(5.019)				
HPI			−0.128	1.303**	0.318	1.212*			1.990	−11.684**	−1.706	−11.132*
			(0.391)	(0.532)	(0.276)	(0.618)			(3.177)	(5.398)	(2.077)	(6.322)
Manuf. Share	0.827**	1.107	−0.700	0.565	0.565	0.362	−7.550**	−8.100	6.260	−13.678**	−4.391	−12.458*
	(0.378)	(0.673)	(0.427)	(0.487)	(0.345)	(0.619)	(2.961)	(5.617)	(3.740)	(5.068)	(2.769)	(6.517)
Small Firms	−0.023	−0.256	−0.365	−0.255	0.195	−0.383	−1.925	3.887	1.052	3.474	−2.941	4.281
	(0.299)	(0.561)	(0.359)	(0.404)	(0.265)	(0.477)	(2.344)	(4.681)	(2.966)	(4.185)	(2.016)	(5.015)
Similar BC	0.884**	−0.120	0.834*	0.555	0.447	0.639	−7.590***	−4.944	−6.510*	−4.397	−3.184	−4.844
	(0.342)	(0.533)	(0.426)	(0.469)	(0.308)	(0.504)	(2.682)	(4.454)	(3.462)	(4.156)	(2.248)	(4.494)
Flex. Wages	−0.068	0.460	−0.421	−0.168	−0.274	−0.139	2.060	−3.814	−0.590	4.653	−0.928	4.380
	(0.236)	(0.506)	(0.354)	(0.416)	(0.237)	(0.441)	(1.848)	(4.222)	(2.637)	(4.831)	(1.599)	(5.214)
Flex. Prices	0.427	0.906*	−0.150	0.677*	−0.111	0.708	−3.678	−2.064	3.343	−1.292	1.390	−1.437
	(0.331)	(0.499)	(0.461)	(0.393)	(0.319)	(0.424)	(2.597)	(4.168)	(3.663)	(4.042)	(2.310)	(4.388)
Constant	0.467*	2.714***	0.481	2.415***	−0.088	2.525***	−3.159*	−24.330***	−3.856	−23.643***	1.287	−24.401***
	(0.235)	(0.495)	(0.292)	(0.394)	(0.212)	(0.443)	(1.838)	(4.132)	(2.409)	(3.925)	(1.612)	(4.555)
Observations	48	48	44	44	40	40	48	48	45	45	41	41
Adjusted R^2	0.458	0.058	0.153	0.157	0.170	0.151	0.460	0.037	0.074	0.145	0.145	0.119

Notes: Table 2.3 reports coefficient estimates for regressions of the cumulative responses in states' real economic activity (REA) and unemployment 48 periods ahead of a one standard-deviation cut in the policy rate on state-level explanatory variables. The specification "Drop Outl." drops potential outliers, namely Michigan, Kentucky, West Virginia and North Dakota for REA, and Colorado, North Carolina and Missouri for unemployment. "Drop Outl. HPI" drops, on the top of "Drop Outl.", potential outliers for shocks in house prices, namely Florida, Nevada, California and Arizona. HPI is the state's House Price Index. HPI Shock is the difference between maximum and minimum values of the House Price Index over 2005-2013. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

In columns 1 and 2, I measure differences in house prices with the variable on boom-bust shocks used in Panel (b) of Figure 2.7. States with large values are those states in which prices decreased more over 2005-2013, i.e. states where the house-price bubble was larger. We expect that, in those states, the impact of a monetary expansion on REA and unemployment was lower than before the crisis. Column 1 estimates the coefficient in the pre-crisis sample. The positive and statistically significant coefficient of 1.550 suggests that, before the crisis, states that then experienced large decreases in house prices were the ones that reacted better to a monetary expansion. Note that this estimate reflects what we found in Panel (a) of Figure 2.4, where we showed that the states that reacted significantly well to a monetary stimulus before the crisis were California, Nevada and Florida, which were also the ones with the largest drops in house prices after 2008. Column 2 estimates the same coefficient in the post-crisis sample and shows that it is no longer statistically significant. Intuitively, as states like California, Nevada and Florida register lower effects of monetary policy in the post-crisis sample, the positive correlation between the boom-bust variable and states' reactions disappears.

In columns 3 and 4 I estimate the same regressions of columns 3 and 4 of Table 2.2, Panel A, by dropping states that can be considered outliers in the post-crisis samples. As shown in Panel (a) of Figure 2.4, these states are Michigan, Kentucky, West Virginia and North Dakota.[33] In columns 5 and 6 I estimate the same regressions by dropping also the four states with the largest values of boom-bust shocks in house prices, namely California, Nevada, Arizona and Florida. Columns 3 and 4 confirm the baseline results, with a somewhat larger statistical significance for the post-crisis sample. Columns 5 and 6 also confirm the baseline results, though the statistical significance for the post-crisis sample is not as strong as for column 4.[34]

Columns 7 to 12 report the same selection of robustness checks for reactions in unemployment and confirm the same findings. In Appendix 2.C, I report the robustness checks for cumulated responses 36 periods ahead of the shock, and minimum, maximum and peak responses. Results remain approximately unchanged throughout the tables.

2.8.3 Regressions with Stacked Samples

Overall, Table 2.2 and 2.3 show that differences in house prices explain differences in states' reaction in the aftermath of the financial crisis, while this is not the case in the pre-crisis sample. While this true in absolute values, it is interesting to test whether this difference is also statistically significant. One way to test this aspect is estimating regressions with interaction terms on a stacked dataset. Specifically, I stack states' reactions estimated on the pre- and post- crisis samples and related controls in a single dataset, which now accounts for 96 observations. I then estimate the baseline regressions by interacting all the independent variables with a dummy which equals 0 for the pre-crisis sample and 1 for the post-crisis sample. Table 2.4 reports a summary of the results for both REA and unemployment, which excludes single-term coefficients. Tables 2.C.1 and 2.C.2 in Appendix 2.C report the full results.

Columns 1 to 5 show the results when I use the simple HPI as the measure for house prices. A positive and statistically significant coefficient for the interaction term would suggest that the house-price channel of monetary policy is significantly stronger in the post-crisis sample, compared to the pre-crisis sample. I start by considering coefficients in Panel A, which estimate the correlations for responses in REA. The positive and statistically significant coefficients in columns 1 to 3 suggest that this is the case when we consider cumulated responses for 36 and 48 periods ahead of the monetary-policy shock, and for minimum responses over 48 periods ahead of the shock. The positive but statistically insignificant

[33] These states are defined as states with at least one response beyond 1.5 interquartile ranges from the 25th and the 75th percentiles in all 48 periods ahead of the shock.

[34] This is to be expected, as those 4 states are the ones with the largest boom-bust values. However, it is comforting to see that results are still significant after dropping them.

Table 2.4: Stacked Samples

	Cum. 36	Cum. 48	Min	Max	Peak	Cum. 24	Cum. 48	Min	Max	Peak
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: REA										
Crisis × HPI	1.132*	1.563**	0.030**	0.016	0.054					
	(0.587)	(0.766)	(0.012)	(0.023)	(0.033)					
Crisis × HPI Shock						−0.348	−0.877	0.010	−0.049**	−0.032
						(0.534)	(0.685)	(0.011)	(0.020)	(0.029)
Crisis × Manuf. Share	1.096*	1.784**	0.014	0.051**	0.063*	0.203	0.280	0.006	0.004	0.010
	(0.595)	(0.776)	(0.012)	(0.024)	(0.033)	(0.638)	(0.818)	(0.013)	(0.024)	(0.034)
Crisis × Small Firms	−0.059	−0.039	−0.005	0.009	0.004	−0.151	−0.233	−0.002	−0.001	−0.001
	(0.514)	(0.670)	(0.010)	(0.021)	(0.028)	(0.516)	(0.662)	(0.011)	(0.019)	(0.028)
Crisis × Similar BC	−0.706	−0.903	0.013	−0.054**	−0.047	−0.782	−1.004	0.010	−0.056***	−0.054*
	(0.557)	(0.726)	(0.011)	(0.022)	(0.031)	(0.549)	(0.704)	(0.011)	(0.020)	(0.030)
Crisis × Flex. Wage	0.151	0.042	0.001	0.007	−0.001	0.472	0.528	0.008	0.014	0.015
	(0.473)	(0.616)	(0.010)	(0.019)	(0.026)	(0.433)	(0.556)	(0.009)	(0.016)	(0.023)
Crisis × Flex. Prices	0.846	1.100	0.007	0.033	0.047	0.402	0.479	0.005	0.012	0.029
	(0.572)	(0.745)	(0.012)	(0.023)	(0.032)	(0.525)	(0.674)	(0.011)	(0.020)	(0.028)
Constant	−0.191	0.571	−0.046***	0.080***	0.055***	−0.250	0.467	−0.046***	0.077***	0.050***
	(0.308)	(0.402)	(0.006)	(0.012)	(0.017)	(0.303)	(0.389)	(0.006)	(0.011)	(0.016)
Adjusted R^2	0.274	0.214	0.215	0.149	0.117	0.302	0.269	0.192	0.285	0.201
Panel B: Unemployment										
Crisis × HPI	−8.558*	−10.910*	−0.108	−0.204**	−0.381					
	(4.853)	(6.262)	(0.182)	(0.092)	(0.267)					
Crisis × HPI Shock						6.237	11.462**	0.513***	−0.060	0.527**
						(4.483)	(5.660)	(0.153)	(0.088)	(0.236)
Crisis × Manuf. Share	−9.299*	−14.211**	−0.449**	−0.039	−0.449	−0.600	−0.550	−0.014	0.011	0.121
	(4.919)	(6.347)	(0.184)	(0.093)	(0.270)	(5.351)	(6.756)	(0.183)	(0.105)	(0.282)
Crisis × Small Firms	3.480	3.464	−0.011	0.143*	0.242	4.877	5.811	0.087	0.127	0.332
	(4.248)	(5.482)	(0.159)	(0.081)	(0.234)	(4.330)	(5.466)	(0.148)	(0.085)	(0.228)
Crisis × Similar BC	0.513	2.373	0.186	−0.049	0.120	0.643	2.646	0.192	−0.035	0.149
	(4.602)	(5.938)	(0.172)	(0.087)	(0.253)	(4.606)	(5.816)	(0.158)	(0.090)	(0.242)
Crisis × Flex. Wage	−1.509	−1.684	−0.139	0.035	−0.088	−4.572	−5.874	−0.213*	−0.019	−0.241
	(3.907)	(5.041)	(0.146)	(0.074)	(0.215)	(3.637)	(4.592)	(0.124)	(0.071)	(0.191)
Crisis × Flex. Prices	−1.389	−1.661	−0.045	0.003	−0.114	0.777	1.614	0.076	−0.008	0.002
	(4.722)	(6.093)	(0.177)	(0.090)	(0.260)	(4.408)	(5.565)	(0.151)	(0.087)	(0.232)
Constant	1.784	−4.026	−0.602***	0.394***	−0.247*	2.275	−3.159	−0.568***	0.397***	−0.203
	(2.546)	(3.285)	(0.095)	(0.048)	(0.140)	(2.544)	(3.212)	(0.087)	(0.050)	(0.134)
Adjusted R^2	0.318	0.243	0.197	0.107	0.138	0.324	0.282	0.336	0.053	0.217
Observations	96	96	96	96	96	96	96	96	96	96
Singe Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 2.4 reports coefficient estimates for stacked regressions of the responses in states' real economic activity (REA) and unemployment to a one standard-deviation cut in the policy rate on state-level explanatory variables. Cum. 36 and Cum. 48 are cumulative responses for 36 and 48 months ahead of the shock. Min, Max and Peak are minimum, maximum and peak responses over a 48-month window. Crisis is a dummy which equals 0 and 1 for, respectively, pre- and post-crisis samples. HPI is the state's House Price Index. HPI Shock is the difference between maximum and minimum values of the House Price Index over 2005-2013. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

coefficients in columns 4 and 5 show that significance goes away when considering maximum and peak responses.

Columns 6 to 10 show the results when I use boom-bust shocks in HPI as the measure for house prices. In this case, negative interaction terms would suggest that regions with large shocks register lower responses after the crisis. While the sign is negative in all regressions but for the minimum responses (column 8), it is statistically significant only for the maximum responses (column 9). The coefficient estimate of column 9 would therefore suggest that states with larger shocks in house prices register maximum responses that are significantly lower after the crisis (compared to before the crisis). Coefficients for the manufacturing share and similarity in business cycles confirm the baseline results of Table 2.2, with some specifications that estimate, respectively, a positive and negative post-crisis difference that is statistically significant.

Panel B confirms these results for responses in unemployment. The negative and statistically significant interactions in columns 1, 2, 4, 7, 8 and 10 suggest that real effects of the house-price channel of monetary policy on unemployment are significantly stronger in the post-crisis sample. In summary, Tables 2.2, 2.3 and 2.4 show that differences in house prices explained differences in states' reactions to a monetary-policy shock especially after the financial crisis. These results bear all the obvious caveats of small-sample, cross-sectional regressions, as the coefficients may be biased due to issues of latent response variables, endogeneity, and potential model misspecification (Fischer et al., 2021). Nonetheless, they provide a general understanding of what could be the cross-sectional drivers of the state-level results estimated via the GVAR model.

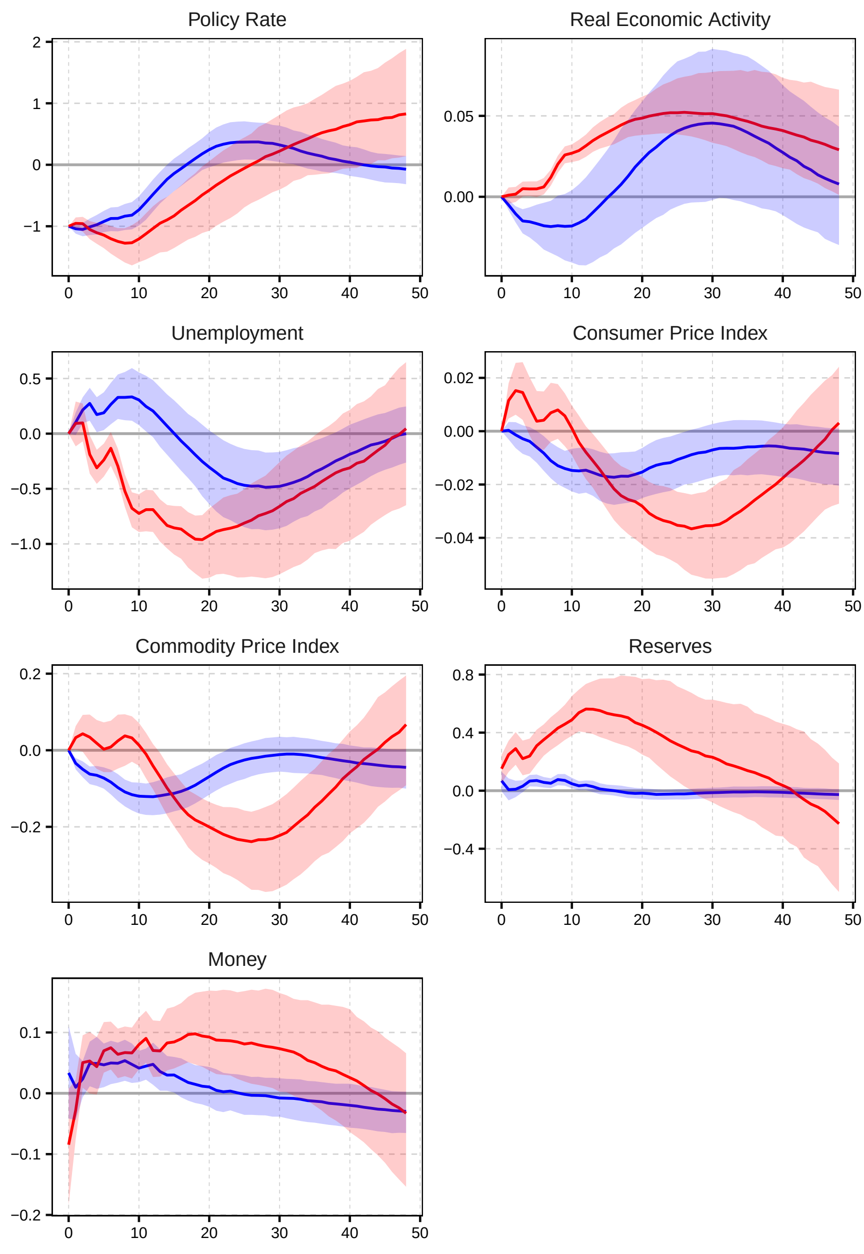
2.9 Conclusion

In the U.S., the effects of monetary policy are heterogeneous across states. This is the case because, at a given moment in time, economic conditions are different across states. For example, the housing market in some states might be more sound than in others. Large exogenous shocks, such as financial crises, can reshuffle the cross-state distribution of these economic conditions. As a result, states' reaction to a monetary expansion can change.

This paper addresses how states responded differently to the policies of the Fed in the aftermath of the last financial crisis. It does so with a VAR analysis on pre and post-crisis samples, i.e. 1990-2007 and 2010-2019. Specifically, I estimate a Bayesian Global VAR, that allows for different equations for the Fed and the states. As a measure of policy rate, I use the Shadow Federal Funds Rate computed by Wu and Xia (2016), which accounts for the zero lower bound in the post-crisis sample. States' real variables are a monthly state-level index for real economic activity estimated by the Fed of Philadelphia and monthly unemployment. The model estimates two main results. First, the median effect of a monetary expansion across all states is larger in the post-crisis sample. Second, the cross-state heterogeneity in the post-crisis sample is also larger. In the second part of the paper, I explore the possible drivers of the larger heterogeneity. With a cross-sectional regression analysis, I find that differences in house prices explain the cross-state differences for post-crisis results. These results are in line with a recent literature that uses micro-level loan data and shows that quantitative easing worked less in areas more affected by the house-price bubble (Beraja et al., 2019). This paper highlights the importance of tracking the evolution of economic fundamentals across different regions in the U.S., and using it for taking informed policy decisions. Above all, national fiscal policies that address regional inequalities should take house-price dynamics into account, and possibly move resources from least to most affected areas to support the positive effects of a monetary expansion.

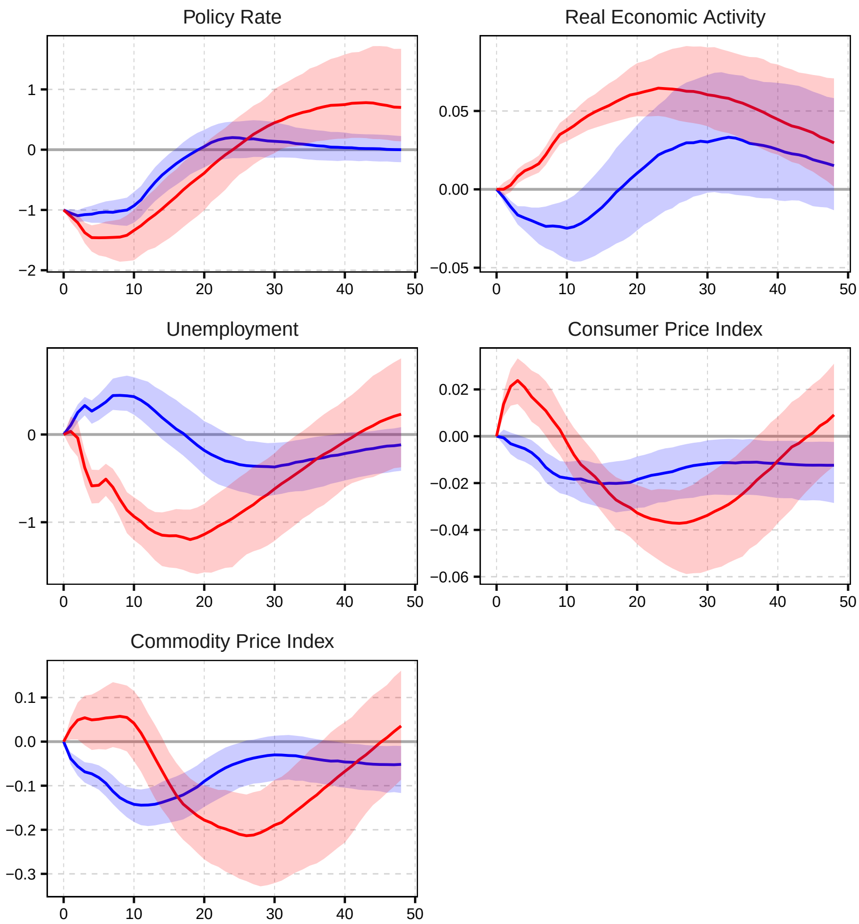
Appendix 2.A National Model - Robustness Checks

Figure 2.A.1: National Model - Baseline



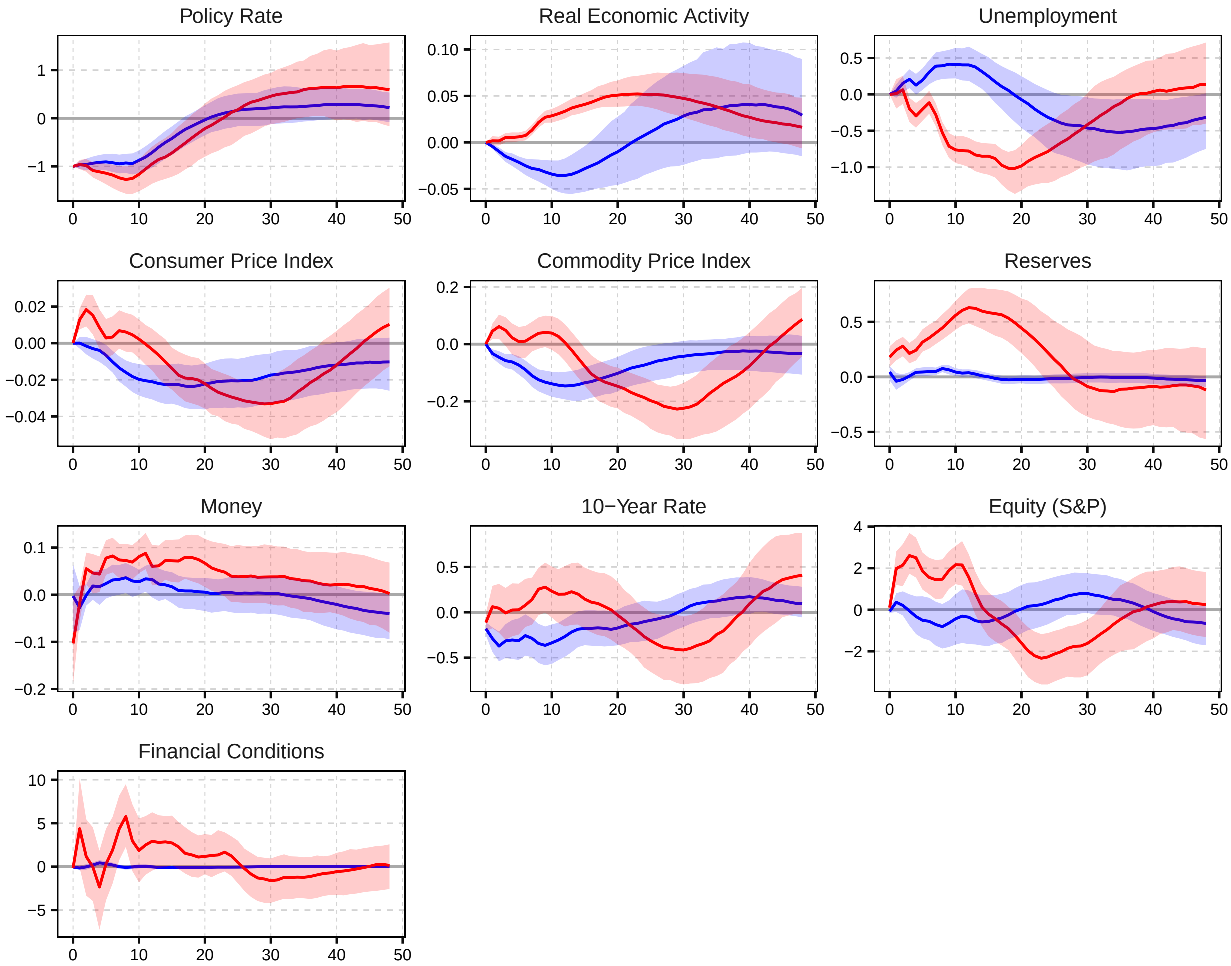
Notes: Figure 2.A.1 plots the posterior medians of the variables' impulse responses for 48 periods ahead of a one standard-deviation decrease in the policy rate. Blue and red lines refer to the model estimated over, respectively, 1990-2007 and 2010-2019. The policy rate is the shadow FFR. Shaded areas are the 68-% confidence intervals, drawn from the posterior distributions.

Figure 2.A.2: National Model - No Money



Notes: Figure 2.A.2 plots the posterior medians of the variables' impulse responses for 48 periods ahead of a one standard-deviation decrease in the policy rate. Blue and red lines refer to the model estimated over, respectively, 1990-2007 and 2010-2019. The policy rate is the shadow FFR. Shaded areas are the 68-% confidence intervals, drawn from the posterior distributions.

Figure 2.A.3: National Model - Extra Variables



Notes: Figure 2.A.3 plots the posterior medians of the variables' impulse responses for 48 periods ahead of a one standard-deviation decrease in the policy rate. Blue and red lines refer to the model estimated over, respectively, 1990-2007 and 2010-2019. The policy rate is the shadow FFR. 10-Year Rate is the 10-Year Treasury Constant Maturity Rate. Equity (S&P) is the Standard and Poor 500 Index. Financial Conditions is the Chicago Fed National Financial Conditions Index. Shaded areas are the 68-% confidence intervals, drawn from the posterior distributions.

Appendix 2.B Similarity in Business Cycles

The literature points out that, during 1990-07, differences in states' responses were also driven by differences in states' position in the business cycle. For example, Beckworth (2010) shows that, before the crisis, states with business cycles that differ from the US were the ones registering the larger asymmetric effects of monetary policy.[35] Section 2.7 reports that, after the crisis, all states register larger reactions to an expansionary monetary policy by the Fed on impact. To check whether the similarity between states' and USA's business cycles has changed in the two samples, I consider the time series of the year-on-year monthly growth of the Coincident Index for Real Economic Activity for all states and the aggregate economy over 1990-2019. These time series are reported in Panel (a) of Figure 2.B.1.

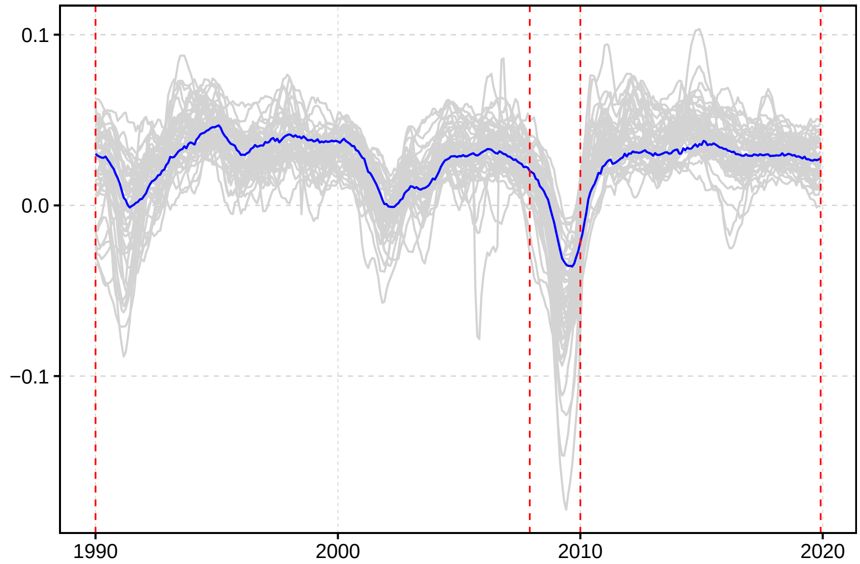
Overall, there is a clear heterogeneity in the pattern of states' real economic activity (REA) with respect to the USA throughout the considered sample. This is especially true in the first half of the 1990-07 sample, when states' deviations from the US are quite large. Consider now the argument that a large economic crisis like the one in 2008 would push the states in at the beginning of the business cycle right in the aftermath of the crisis. It appears from the graph in Panel (a) that the band of states' REA (grey band) around the US was somehow wider than it was in 2010. To better emphasize this aspect, I plot the distributions of states' changes in REA in the years 1990 and 2010 in Panel (b) of Figure 2.B.1. The distribution for 1990 (blue line) reports a relatively high variance of states' REA, with a standard deviation of .024. On the other hand, the distribution for 2010 (red line) has a lower variance, with a standard deviation of .014, about half of the distribution for 1990.

Overall, Figure 2.B.1 shows that the differences between states' and USA's business cycles registered in the 1990-07 sample are generally lower for the 2010-19 sample.

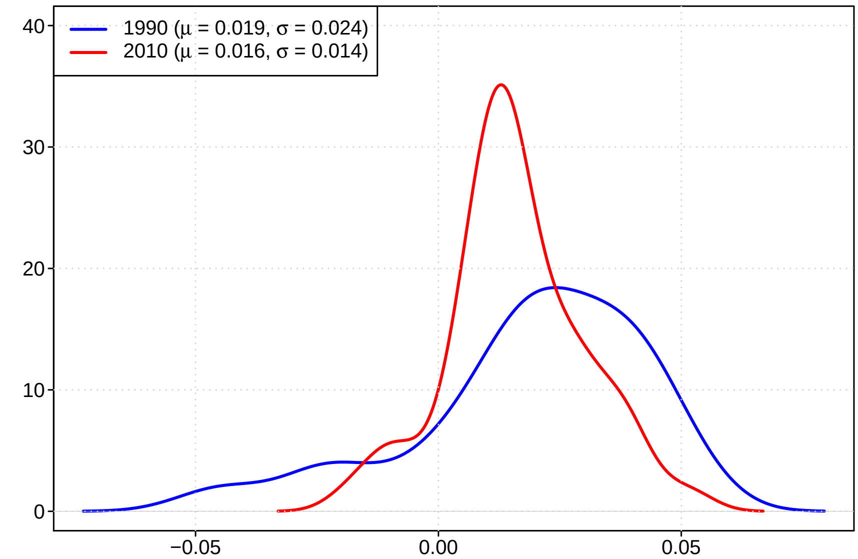
This is especially true when considering the initial years of the sample, namely 1983 and 2010. This trend supports the hypothesis that the 2008 financial crisis has re-set states' business cycles, pushing them towards an initial phase. More symmetric business cycles may in turn play a role in explaining the generally larger, on-impact effect of the Fed's monetary policy on state's economies in the post-crisis period. I will now turn to test the second of the mentioned hypotheses, namely the house-price crisis as one of the drivers behind the increase in the variance across state's responses in the post-crisis period.

[35] Beckworth argues that these asymmetries were also due to the lack of shock absorbers across states, that could have smoothed the impact, such as wages and price flexibility.

Figure 2.B.1: Business Cycles



(a) REA Growth



(b) Distributions in 1990 and 2010

Notes: the plots refer to the time series of the year-on-year monthly change in states' real economic activity (REA) over 1983-2019. Panel (a) reports the times series for all states (grey lines) and the United States (blue line). The the red dashed lines delimit the samples considered in the analysis, namely 1983-07 and 2010-19. Panel (b) draws the distributions of year-on-year monthly change in REA across states in the initial year of the samples, namely 1983 (blue line) and 2010 (red line). For both panels, I drop Alaska and Hawaii, as they are not included in the baseline specification of the model, and West Virginia, as it is a clear outlier over 1983-07.

Appendix 2.C Regressions - Full Tables and Robustness Checks

Table 2.C.1: REA - Before and After

	Cum. 36	Cum. 48	Min	Max	Peak	Cum. 24	Cum. 48	Min	Max	Peak
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Crisis	2.380***	2.233***	0.014	0.045**	0.039	2.375***	2.247***	0.012	0.048***	0.039*
	(0.435)	(0.567)	(0.009)	(0.017)	(0.024)	(0.428)	(0.549)	(0.009)	(0.016)	(0.023)
HPI	−0.352	−0.352	−0.004	−0.0002	0.002					
	(0.404)	(0.527)	(0.008)	(0.016)	(0.022)					
HPI Shock						0.897**	1.550***	0.008	0.058***	0.070***
						(0.389)	(0.499)	(0.008)	(0.014)	(0.021)
Manuf. Share	−0.297	−0.638	0.005	−0.033*	−0.023	0.641	0.827	0.013	0.014	0.032
	(0.454)	(0.591)	(0.009)	(0.018)	(0.025)	(0.488)	(0.627)	(0.010)	(0.018)	(0.026)
Small Firms	−0.183	−0.388	0.005	−0.018	−0.021	0.049	−0.023	0.007	−0.006	−0.007
	(0.390)	(0.508)	(0.008)	(0.016)	(0.022)	(0.387)	(0.496)	(0.008)	(0.014)	(0.021)
Similar BC	0.566	0.871	0.012	0.026	0.030	0.566	0.884	0.012	0.027	0.031
	(0.452)	(0.589)	(0.009)	(0.018)	(0.025)	(0.442)	(0.567)	(0.009)	(0.016)	(0.024)
Flex. Wage	0.101	0.120	−0.004	0.004	0.002	−0.048	−0.068	−0.006	0.001	−0.002
	(0.328)	(0.428)	(0.007)	(0.013)	(0.018)	(0.305)	(0.391)	(0.006)	(0.011)	(0.016)
Flex. Prices	−0.258	−0.402	0.003	−0.013	−0.019	0.309	0.427	0.008	0.010	0.008
	(0.472)	(0.615)	(0.010)	(0.019)	(0.026)	(0.428)	(0.549)	(0.009)	(0.016)	(0.023)
Crisis × HPI	1.132*	1.563**	0.030**	0.016	0.054					
	(0.587)	(0.766)	(0.012)	(0.023)	(0.033)					
Crisis × HPI Shock						−0.348	−0.877	0.010	−0.049**	−0.032
						(0.534)	(0.685)	(0.011)	(0.020)	(0.029)
Crisis × Manuf. Share	1.096*	1.784**	0.014	0.051**	0.063*	0.203	0.280	0.006	0.004	0.010
	(0.595)	(0.776)	(0.012)	(0.024)	(0.033)	(0.638)	(0.818)	(0.013)	(0.024)	(0.034)
Crisis × Small Firms	−0.059	−0.039	−0.005	0.009	0.004	−0.151	−0.233	−0.002	−0.001	−0.001
	(0.514)	(0.670)	(0.010)	(0.021)	(0.028)	(0.516)	(0.662)	(0.011)	(0.019)	(0.028)
Crisis × Similar BC	−0.706	−0.903	0.013	−0.054**	−0.047	−0.782	−1.004	0.010	−0.056***	−0.054*
	(0.557)	(0.726)	(0.011)	(0.022)	(0.031)	(0.549)	(0.704)	(0.011)	(0.020)	(0.030)
Crisis × Flex. Wage	0.151	0.042	0.001	0.007	−0.001	0.472	0.528	0.008	0.014	0.015
	(0.473)	(0.616)	(0.010)	(0.019)	(0.026)	(0.433)	(0.556)	(0.009)	(0.016)	(0.023)
Crisis × Flex. Prices	0.846	1.100	0.007	0.033	0.047	0.402	0.479	0.005	0.012	0.029
	(0.572)	(0.745)	(0.012)	(0.023)	(0.032)	(0.525)	(0.674)	(0.011)	(0.020)	(0.028)
Constant	−0.191	0.571	−0.046***	0.080***	0.055***	−0.250	0.467	−0.046***	0.077***	0.050***
	(0.308)	(0.402)	(0.006)	(0.012)	(0.017)	(0.303)	(0.389)	(0.006)	(0.011)	(0.016)
Observations	96	96	96	96	96	96	96	96	96	96
Adjusted R^2	0.274	0.214	0.215	0.149	0.117	0.302	0.269	0.192	0.285	0.201

Notes: Table 2.C.1 reports coefficient estimates for stacked regressions of the responses in states' real economic activity (REA) and unemployment to a one standard-deviation cut in the policy rate on state-level explanatory variables. Cum. 36 and Cum. 48 are cumulative responses for 36 and 48 months ahead of the shock. Min, Max and Peak are minimum, maximum and peak responses over a 48-month window. Crisis is a dummy which equals 0 and 1 for, respectively, pre- and post-crisis samples. HPI is the state's House Price Index. HPI Shock is the difference between maximum and minimum values of the House Price Index over 2005-2013. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

Table 2.C.2: Unemployment - Before and After

	Cum. 36	Cum. 48	Min	Max	Peak	Cum. 24	Cum. 48	Min	Max	Peak
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Crisis	-21.929^{***}	-20.790^{***}	-0.471^{***}	-0.176^{**}	-0.584^{***}	-22.073^{***}	-21.171^{***}	-0.499^{***}	-0.165^{**}	-0.603^{***}
	(3.594)	(4.637)	(0.135)	(0.068)	(0.198)	(3.588)	(4.531)	(0.123)	(0.070)	(0.189)
HPI	1.278	0.518	−0.062	0.011	−0.087					
	(3.340)	(4.309)	(0.125)	(0.063)	(0.184)					
HPI Shock						-7.326^{**}	-12.873^{***}	-0.493^{***}	−0.041	-0.650^{***}
						(3.263)	(4.119)	(0.112)	(0.064)	(0.172)
Manuf. Share	1.290	3.163	0.169	−0.049	0.084	−5.402	−7.550	−0.192	−0.089	-0.388^{*}
	(3.747)	(4.835)	(0.140)	(0.071)	(0.206)	(4.098)	(5.175)	(0.140)	(0.080)	(0.216)
Small Firms	−0.453	0.785	0.114	−0.102	0.010	−2.129	−1.925	0.021	-0.112^{*}	−0.111
	(3.221)	(4.156)	(0.121)	(0.061)	(0.177)	(3.244)	(4.096)	(0.111)	(0.064)	(0.171)
Similar BC	−4.531	−7.371	-0.237^{*}	−0.048	−0.217	−4.607	−7.590	-0.250^{*}	−0.048	−0.234
	(3.731)	(4.814)	(0.140)	(0.071)	(0.205)	(3.712)	(4.686)	(0.127)	(0.073)	(0.195)
Flex. Wage	0.199	1.143	0.053	0.017	0.112	0.985	2.060	0.066	0.023	0.128
	(2.714)	(3.502)	(0.102)	(0.052)	(0.149)	(2.557)	(3.229)	(0.088)	(0.050)	(0.135)
Flex. Prices	1.604	1.786	0.037	0.006	0.137	−2.086	−3.678	−0.124	−0.017	−0.072
	(3.898)	(5.029)	(0.146)	(0.074)	(0.214)	(3.593)	(4.537)	(0.123)	(0.071)	(0.189)
Crisis × HPI	-8.558^{*}	-10.910^{*}	−0.108	-0.204^{**}	−0.381					
	(4.853)	(6.262)	(0.182)	(0.092)	(0.267)					
Crisis × HPI Shock						6.237	11.462^{**}	0.513^{***}	−0.060	0.527^{**}
						(4.483)	(5.660)	(0.153)	(0.088)	(0.236)
Crisis × Manuf. Share	-9.299^{*}	-14.211^{**}	-0.449^{**}	−0.039	−0.449	−0.600	−0.550	−0.014	0.011	0.121
	(4.919)	(6.347)	(0.184)	(0.093)	(0.270)	(5.351)	(6.756)	(0.183)	(0.105)	(0.282)
Crisis × Small Firms	3.480	3.464	−0.011	0.143^{*}	0.242	4.877	5.811	0.087	0.127	0.332
	(4.248)	(5.482)	(0.159)	(0.081)	(0.234)	(4.330)	(5.466)	(0.148)	(0.085)	(0.228)
Crisis × Similar BC	0.513	2.373	0.186	−0.049	0.120	0.643	2.646	0.192	−0.035	0.149
	(4.602)	(5.938)	(0.172)	(0.087)	(0.253)	(4.606)	(5.816)	(0.158)	(0.090)	(0.242)
Crisis × Flex. Wage	−1.509	−1.684	−0.139	0.035	−0.088	−4.572	−5.874	-0.213^{*}	−0.019	−0.241
	(3.907)	(5.041)	(0.146)	(0.074)	(0.215)	(3.637)	(4.592)	(0.124)	(0.071)	(0.191)
Crisis × Flex. Prices	−1.389	−1.661	−0.045	0.003	−0.114	0.777	1.614	0.076	−0.008	0.002
	(4.722)	(6.093)	(0.177)	(0.090)	(0.260)	(4.408)	(5.565)	(0.151)	(0.087)	(0.232)
Constant	1.784	−4.026	-0.602^{***}	0.394^{***}	-0.247^{*}	2.275	−3.159	-0.568^{***}	0.397^{***}	−0.203
	(2.546)	(3.285)	(0.095)	(0.048)	(0.140)	(2.544)	(3.212)	(0.087)	(0.050)	(0.134)
Observations	96	96	96	96	96	96	96	96	96	96
Adjusted R^2	0.318	0.243	0.197	0.107	0.138	0.324	0.282	0.336	0.053	0.217

Notes: Table 2.C.2 reports coefficient estimates for stacked regressions of the responses in states' real economic activity (REA) and unemployment to a one standard-deviation cut in the policy rate on state-level explanatory variables. Cum. 36 and Cum. 48 are cumulative responses for 36 and 48 months ahead of the shock. Min, Max and Peak are minimum, maximum and peak responses over a 48-month window. Crisis is a dummy which equals 0 and 1 for, respectively, pre- and post-crisis samples. HPI is the state's House Price Index. HPI Shock is the difference between maximum and minimum values of the House Price Index over 2005-2013. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

Table 2.C.3: Robustness Checks - 36 Periods Ahead

	Dependent variable:											
	REA						Undemployment					
	HPI Shock		Drop Outl.		Drop Outl. HPI		HPI Shock		Drop Outl.		Drop Outl. HPI	
	(1) Pre	(2) Post	(3) Pre	(4) Post	(5) Pre	(6) Post	(7) Pre	(8) Post	(9) Pre	(10) Post	(11) Pre	(12) Post
HPI Shock	0.897***	0.549					−7.326***	−1.089				
	(0.220)	(0.475)					(1.772)	(4.014)				
HPI			−0.219	0.773*	0.039	0.658			2.287	−8.080*	−0.040	−7.507
			(0.252)	(0.409)	(0.204)	(0.474)			(2.158)	(4.332)	(1.693)	(5.077)
Manuf. Share	0.641**	0.844	−0.360	0.317	0.376	0.119	−5.402**	−6.003	3.422	−9.793**	−3.156	−8.721
	(0.277)	(0.531)	(0.276)	(0.374)	(0.255)	(0.475)	(2.226)	(4.492)	(2.540)	(4.068)	(2.257)	(5.233)
Small Firms	0.049	−0.102	−0.152	−0.104	0.168	−0.225	−2.129	2.748	−0.268	2.503	−2.736	3.211
	(0.219)	(0.443)	(0.232)	(0.311)	(0.195)	(0.366)	(1.762)	(3.743)	(2.015)	(3.359)	(1.643)	(4.027)
Similar BC	0.566**	−0.216	0.584**	0.411	0.347	0.471	−4.607**	−3.963	−3.963	−3.325	−1.844	−3.636
	(0.251)	(0.421)	(0.275)	(0.360)	(0.227)	(0.387)	(2.016)	(3.561)	(2.352)	(3.335)	(1.833)	(3.609)
Flex. Wages	−0.048	0.424	−0.290	0.019	−0.203	0.045	0.985	−3.587	−0.958	2.658	−1.133	2.428
	(0.173)	(0.399)	(0.228)	(0.320)	(0.175)	(0.339)	(1.389)	(3.376)	(1.791)	(3.877)	(1.304)	(4.187)
Flex. Prices	0.309	0.711*	−0.032	0.577*	−0.018	0.617*	−2.086	−1.309	2.685	−0.934	1.614	−1.094
	(0.243)	(0.394)	(0.297)	(0.302)	(0.236)	(0.325)	(1.952)	(3.333)	(2.489)	(3.244)	(1.884)	(3.523)
Constant	−0.250	2.125***	−0.232	1.879***	−0.557***	1.970***	2.275	−19.798***	1.839	−19.171***	4.908***	−19.776***
	(0.172)	(0.391)	(0.189)	(0.303)	(0.157)	(0.340)	(1.382)	(3.304)	(1.637)	(3.150)	(1.314)	(3.657)
Observations	48	48	44	44	40	40	48	48	45	45	41	41
Adjusted R^2	0.320	0.048	0.118	0.116	0.105	0.109	0.294	0.025	0.011	0.098	0.095	0.070

Notes: Table 2.3 reports coefficient estimates for regressions of the cumulative responses in states' real economic activity (REA) and unemployment 36 periods ahead of a one standard-deviation cut in the policy rate on state-level explanatory variables. The specification "Drop Outl." drops potential outliers, namely Michigan, Kentucky, West Virginia and North Dakota for REA, and Colorado, North Carolina and Missouri for unemployment. "Drop Outl. HPI" drops, on the top of "Drop Outl.", potential outliers for shocks in house prices, namely Florida, Nevada, California and Arizona. HPI is the state's House Price Index. HPI Shock is the difference between maximum and minimum values of the House Price Index over 2005-2013. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

Table 2.C.4: Robustness Checks - Minimum

	Dependent variable:											
	REA						Undemployment					
	HPI Shock		Drop Outl.		Drop Outl. HPI		HPI Shock		Drop Outl.		Drop Outl. HPI	
	(1) Pre	(2) Post	(3) Pre	(4) Post	(5) Pre	(6) Post	(7) Pre	(8) Post	(9) Pre	(10) Post	(11) Pre	(12) Post
HPI Shock	0.008	0.018^{*}					-0.493^{***}	0.020				
	(0.005)	(0.010)					(0.065)	(0.135)				
HPI			-0.004	0.015^{*}	-0.001	0.019^{**}			-0.022	-0.203	-0.132^{**}	-0.153
			(0.006)	(0.008)	(0.006)	(0.009)			(0.105)	(0.152)	(0.056)	(0.176)
Manuf. Share	0.013^{*}	0.020^{*}	0.003	0.009	0.012	0.014	-0.192^{**}	-0.206	0.251^{**}	-0.341^{**}	-0.093	-0.254
	(0.007)	(0.011)	(0.006)	(0.007)	(0.008)	(0.009)	(0.082)	(0.152)	(0.123)	(0.142)	(0.075)	(0.182)
Small Firms	0.007	0.005	0.006	0.0003	0.010^{*}	0.004	0.021	0.108	0.121	0.084	-0.002	0.140
	(0.005)	(0.009)	(0.005)	(0.006)	(0.006)	(0.007)	(0.065)	(0.126)	(0.098)	(0.118)	(0.054)	(0.140)
Similar BC	0.012^{*}	0.022^{**}	0.012^{*}	0.026^{***}	0.008	0.025^{***}	-0.250^{***}	-0.058	-0.214^{*}	-0.030	-0.102	-0.053
	(0.006)	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)	(0.074)	(0.120)	(0.114)	(0.117)	(0.061)	(0.125)
Flex. Wages	-0.006	0.002	-0.009	-0.002	-0.008	-0.002	0.066	-0.147	0.003	0.042	-0.013	0.021
	(0.004)	(0.008)	(0.005)	(0.006)	(0.005)	(0.006)	(0.051)	(0.114)	(0.087)	(0.136)	(0.043)	(0.145)
Flex. Prices	0.008	0.013^{*}	0.005	0.007	0.004	0.006	-0.124^{*}	-0.047	0.078	-0.041	0.006	-0.055
	(0.006)	(0.008)	(0.007)	(0.006)	(0.007)	(0.006)	(0.072)	(0.112)	(0.121)	(0.114)	(0.062)	(0.122)
Constant	-0.046^{***}	-0.034^{***}	-0.045^{***}	-0.029^{***}	-0.049^{***}	-0.031^{***}	-0.568^{***}	-1.067^{***}	-0.594^{***}	-1.037^{***}	-0.416^{***}	-1.084^{***}
	(0.004)	(0.008)	(0.004)	(0.006)	(0.005)	(0.006)	(0.051)	(0.111)	(0.080)	(0.110)	(0.043)	(0.127)
Observations	48	48	44	44	40	40	48	48	45	45	41	41
Adjusted R^2	0.054	0.215	0.068	0.262	0.099	0.253	0.660	0.020	0.206	0.060	0.328	0.023

Notes: Table 2.3 reports coefficient estimates for regressions of the minimum responses in states' real economic activity (REA) and unemployment of a one standard-deviation cut in the policy rate on state-level explanatory variables. The specification "Drop Outl." drops potential outliers, namely Michigan, Kentucky, West Virginia and North Dakota for REA, and Colorado, North Carolina and Missouri for unemployment. "Drop Outl. HPI" drops, on the top of "Drop Outl.", potential outliers for shocks in house prices, namely Florida, Nevada, California and Arizona. HPI is the state's House Price Index. HPI Shock is the difference between maximum and minimum values of the House Price Index over 2005-2013. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

Table 2.C.5: Robustness Checks - Maximum

	Dependent variable:											
	REA						Undemployment					
	HPI Shock		Drop Outl.		Drop Outl. HPI		HPI Shock		Drop Outl.		Drop Outl. HPI	
	(1) Pre	(2) Post	(3) Pre	(4) Post	(5) Pre	(6) Post	(7) Pre	(8) Post	(9) Pre	(10) Post	(11) Pre	(12) Post
HPI Shock	0.058***	0.009					−0.041	−0.101				
	(0.009)	(0.017)					(0.049)	(0.072)				
HPI			0.008	0.024*	0.024***	0.017			0.027	−0.167**	−0.003	−0.182**
			(0.014)	(0.014)	(0.008)	(0.016)			(0.053)	(0.070)	(0.056)	(0.081)
Manuf. Share	0.014	0.018	−0.033**	0.005	0.014	−0.008	−0.089	−0.078	−0.015	−0.087	−0.097	−0.112
	(0.012)	(0.019)	(0.015)	(0.013)	(0.010)	(0.016)	(0.062)	(0.080)	(0.062)	(0.065)	(0.075)	(0.083)
Small Firms	−0.006	−0.007	−0.019	−0.003	0.002	−0.011	−0.112**	0.016	−0.099*	0.049	−0.132**	0.032
	(0.009)	(0.016)	(0.013)	(0.011)	(0.008)	(0.013)	(0.049)	(0.067)	(0.049)	(0.054)	(0.055)	(0.064)
Similar BC	0.027**	−0.029*	0.021	−0.003	0.007	0.0004	−0.048	−0.084	−0.039	−0.097*	−0.007	−0.092
	(0.010)	(0.015)	(0.015)	(0.013)	(0.009)	(0.013)	(0.056)	(0.064)	(0.057)	(0.054)	(0.061)	(0.057)
Flex. Wages	0.001	0.015	−0.010	0.00002	−0.005	0.002	0.023	0.004	−0.0004	0.065	0.001	0.075
	(0.007)	(0.015)	(0.012)	(0.011)	(0.007)	(0.012)	(0.038)	(0.060)	(0.044)	(0.062)	(0.043)	(0.067)
Flex. Prices	0.010	0.023	−0.010	0.022**	−0.009	0.025**	−0.017	−0.025	0.023	−0.011	0.022	−0.009
	(0.010)	(0.014)	(0.016)	(0.011)	(0.010)	(0.011)	(0.054)	(0.060)	(0.061)	(0.052)	(0.063)	(0.056)
Constant	0.077***	0.125***	0.076***	0.108***	0.055***	0.114***	0.397***	0.232***	0.393***	0.200***	0.426***	0.214***
	(0.007)	(0.014)	(0.010)	(0.011)	(0.006)	(0.012)	(0.038)	(0.059)	(0.040)	(0.051)	(0.044)	(0.058)
Observations	48	48	44	44	40	40	48	48	45	45	41	41
Adjusted R^2	0.595	0.039	0.230	0.093	0.343	0.105	0.004	0.009	−0.026	0.134	0.047	0.115

Notes: Table 2.3 reports coefficient estimates for regressions of the maximum responses in states' real economic activity (REA) and unemployment of a one standard-deviation cut in the policy rate on state-level explanatory variables. The specification "Drop Outl." drops potential outliers, namely Michigan, Kentucky, West Virginia and North Dakota for REA, and Colorado, North Carolina and Missouri for unemployment. "Drop Outl. HPI" drops, on the top of "Drop Outl.", potential outliers for shocks in house prices, namely Florida, Nevada, California and Arizona. HPI is the state's House Price Index. HPI Shock is the difference between maximum and minimum values of the House Price Index over 2005-2013. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

Table 2.C.6: Robustness Checks - Peak

	Dependent variable:											
	REA						Undemployment					
	HPI Shock		Drop Outl.		Drop Outl. HPI		HPI Shock		Drop Outl.		Drop Outl. HPI	
	(1) Pre	(2) Post	(3) Pre	(4) Post	(5) Pre	(6) Post	(7) Pre	(8) Post	(9) Pre	(10) Post	(11) Pre	(12) Post
HPI Shock	0.070***	0.038					−0.650***	−0.123				
	(0.014)	(0.024)					(0.126)	(0.196)				
HPI			0.010	0.052**	0.030**	0.049**			−0.028	−0.463**	−0.216	−0.444*
			(0.019)	(0.020)	(0.015)	(0.024)			(0.169)	(0.200)	(0.129)	(0.234)
Manuf. Share	0.032*	0.042	−0.025	0.021	0.033*	0.015	−0.388**	−0.267	0.220	−0.426**	−0.306*	−0.382
	(0.018)	(0.027)	(0.021)	(0.019)	(0.019)	(0.024)	(0.158)	(0.219)	(0.198)	(0.188)	(0.172)	(0.241)
Small Firms	−0.007	−0.008	−0.020	−0.011	0.006	−0.015	−0.111	0.221	0.021	0.244	−0.194	0.274
	(0.014)	(0.023)	(0.018)	(0.015)	(0.014)	(0.018)	(0.125)	(0.183)	(0.157)	(0.155)	(0.125)	(0.186)
Similar BC	0.031*	−0.022	0.022	0.021	0.006	0.024	−0.234	−0.085	−0.179	−0.082	−0.045	−0.098
	(0.016)	(0.022)	(0.021)	(0.018)	(0.017)	(0.019)	(0.143)	(0.174)	(0.184)	(0.154)	(0.140)	(0.166)
Flex. Wages	−0.002	0.013	−0.015	−0.009	−0.009	−0.008	0.128	−0.113	0.047	0.169	0.035	0.159
	(0.011)	(0.020)	(0.017)	(0.016)	(0.013)	(0.017)	(0.099)	(0.165)	(0.140)	(0.179)	(0.099)	(0.193)
Flex. Prices	0.008	0.037*	−0.019	0.030*	−0.016	0.031*	−0.072	−0.071	0.206	−0.041	0.096	−0.045
	(0.016)	(0.020)	(0.023)	(0.015)	(0.017)	(0.016)	(0.138)	(0.163)	(0.194)	(0.150)	(0.144)	(0.163)
Constant	0.050***	0.089***	0.051***	0.077***	0.025**	0.080***	−0.203**	−0.806***	−0.247*	−0.827***	0.011	−0.855***
	(0.011)	(0.020)	(0.014)	(0.015)	(0.011)	(0.017)	(0.098)	(0.161)	(0.128)	(0.145)	(0.100)	(0.169)
Observations	48	48	44	44	40	40	48	48	45	45	41	41
Adjusted R^2	0.440	0.061	0.131	0.190	0.216	0.185	0.401	0.014	0.013	0.154	0.091	0.136

Notes: Table 2.3 reports coefficient estimates for regressions of the peak responses in states' real economic activity (REA) and unemployment of a one standard-deviation cut in the policy rate on state-level explanatory variables. The specification "Drop Outl." drops potential outliers, namely Michigan, Kentucky, West Virginia and North Dakota for REA, and Colorado, North Carolina and Missouri for unemployment. "Drop Outl. HPI" drops, on the top of "Drop Outl.", potential outliers for shocks in house prices, namely Florida, Nevada, California and Arizona. HPI is the state's House Price Index. HPI Shock is the difference between maximum and minimum values of the House Price Index over 2005-2013. Manuf. Share is the share of Gross State Product produced by the manufacturing sector by state. Small Firms is the employment accounted for by a state's firms with less than 100 employees. Similar BC is the correlation between a state's and the nation's business cycle. Flex. Wages and Flex. Prices are indicators for, respectively, wage and price flexibility of a state's economy with respect to the national economy. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level., ** at the 5% level, and * at the 10% level.

Chapter 3

Assessing Basel III: Automatic Distribution Restrictions, Regulatory Capital and Bank Lending

Assessing Basel III: Automatic Distribution Restrictions, Regulatory Capital and Bank Lending*

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Abstract

In 2016, the regulatory framework known as Basel III has introduced automatic restrictions on dividend payments for banks with low levels of regulatory capital. In this paper, we empirically test whether banks increased their regulatory capital specifically in order to avoid being subject to such restrictions. We measure this concern with data on past dividend policies for 65 listed banks over 24 countries, sourced from Capital IQ. Intuitively, banks with high propensity to maintain stable dividends are presumed to be relatively more concerned about automatic restrictions on dividend payments. With a simple difference-in-differences regression analysis, we find that banks that usually smooth dividends increased their capital ratios more after 2016. We confirm these results with a local-projection approach that exploits changes in capital requirements and thus the threshold at which restrictions apply. The impulse response functions suggest that, after a shock in this threshold, dividend-smoothing banks increase their regulatory capital more, and that this difference generally disappears within 8 quarters after the shock. However, we do not find similar results when we use alternative measures of concern on these restrictions based on the volatility of past dividends or presence of dividend targets. Finally, we assess whether the restrictions on dividends had negative spillovers on credit provision and we find no evidence in this sense. These findings contribute to the current assessment and review of the regulatory framework introduced by Basel III.

Keywords: Banks' Capital, Regulatory Buffers, MDA, Dividends

JEL classification: G01, G21, G28, G35

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3.1 Introduction

The financial crisis of 2008 has made it evident that the international banking sector was not prepared to face large financial shocks. One of the main issues was that banks did not hold enough capital to absorb the losses they encountered during the crisis and prevent the "credit crunch" that followed. In 2010, regulators sought to address these, and other, failings in the regulatory framework with a large, international regulatory package: Basel III. This framework introduced new (higher) requirements, of better quality capital, expressed as a percentage of banks' risk-weighted assets. These augmented pre-existing minimum capital requirements by increasing the proportion to be met with the highest quality capital - Common Equity Tier 1 (CET1) - and introduced a new framework of regulatory buffers.
The objectives of Basel III capital buffers are two-fold: i) to increase banks' capacity to absorb losses while remaining solvent institutions; and ii) to enable banks to maintain the provision of critical financial services (e.g. lending) in stress and consequently act as "shock absorbers" rather than "shock amplifiers". The buffers are comprised of a number of different elements tied to different sources of micro- and macro- prudential risk.[1] Basel-III regulatory buffers were subject to a transition period for implementation and were phased in between 2016 and 2018, becoming fully effective from 2019 onwards. Alongside regulatory buffers, Basel III also introduced complementary measures aimed at containing banks' own depletion of capital resources in times of stress. In the global financial crisis, banks were observed to be reluctant to cut dividends in spite of widely anticipated credit losses (e.g. Acharya et al., 2011). This source of equity depletion likely exposed banks to a higher degree of solvency risk and may have impacted their willingness to lend.
Learning from this experience, Basel III introduced a framework of automatic restrictions on capital distributions that would apply when a bank's capital ratio falls below the level of regulatory capital buffers. Specifically, these restrict banks from paying dividends, bonuses and coupons on AT1 convertible instruments above a proportion of their after-tax profits, with the allowable proportion reducing as capital buffers are depleted. This proportion is referred to as the Maximum Distributable Amount (MDA). MDA restrictions apply uniformly across times of economic downturn and growth, and have clear advantages for banks' resilience, i.e. they incentivise banks to build buffers in a timely manner, they set market expectations on regulatory action and they avoid supervisors' forbearance (e.g. Acharya et al., 2016; Schroth, 2021).
But these restrictions may also entail some costs, which is the focus of our paper. The presence of these restrictions are privately costly for banks which may have consequences for

[1] All banks within scope of Basel III regulation face a requirement to maintain a capital conservation buffer set at 2.5% of RWAs. This regulatory buffer is extended by firm-specific add-ons for banks that are of particular systemic importance (i.e. G-SIB and D-SIB buffers) and to protect the banking sector from periods of excess aggregate credit growth associated with the build-up of system-wide risks (CCyB).

their behaviour. Banks are incentivised to maintain management buffers above thresholds for these restrictions which could increase the cost or reduce the potential supply of financial services (e.g. Van den Heuvel, 2008). Second, and perhaps more importantly, the presence of these restrictions may cause banks to act in a procyclical manner. If banks treat the threshold for automatic distribution restrictions as a hard barrier, then they will be unwilling to use buffers in times of stress to support economic activity. Consequently, buffers will satisfy their first objective (of reducing the probability of bank failure) but will fail their second objective (of acting as shock absorbers rather than amplifiers).

We test whether the presence of automatic distribution restrictions creates a material disincentive for banks to use capital buffers using different regression analyses that focus on banks' differing degrees of concern about dividend restrictions. Specifically, we exploit the fact that long-run dividend policies vary materially across banks and that, in principle, banks with higher propensity to pay stable dividends over time would be more concerned about the imposition of these regulatory restrictions.

We measure banks' propensity to pay stable dividends by looking at historical dividend distributions using quarterly data for a sample of 65 publicly listed large banks across 24 countries over the period 2000-2015 in the commercial database Capital IQ.[2] As a baseline measure, we construct a binary variable differentiating banks that historically smoothed dividends and banks that did not. If restrictions on dividends were a reason of concern, we expect dividend-smoothing banks to accumulate more regulatory capital than other banks after the introduction of the regulation in 2016.

We start testing this hypothesis with a simple difference-in-differences (DID) analysis using quarterly balance sheet data for banks in our sample over the period 2013-2019. Our dependent variable of interest is the ratio of Common Equity Tier 1 (CET1) capital over risk-weighted assets. This is regressed on an interaction term between a time dummy which equals 1 after 2016 and the binary variable for dividend-smoothing banks. We find that the implied difference in core-capital ratios between banks that did and did not smooth dividends increased by 1.42 percentage points after 2016. While this result is robust, we do not find similar effects for other groups of banks that could be concerned about restrictions on dividends, namely banks with a stable dividend policy, or with a public, quantitative dividend target, or which distributed dividends in line with analysts' forward-looking expectations.

One of the challenges of the DID analysis is to clearly disentangle the effect of restrictions on dividends from the effects of other measures included in Basel III. We address this challenge by proposing an alternative identification strategy that exploits bank-specific changes in the threshold at which distribution restrictions apply. This threshold varies across banks and can change over time as capital requirements are adjusted by regulators. Consequently,

[2] The sample includes banks headquartered in 17 European countries plus Australia, Canada, China, Japan, Switzerland, United Kingdom and United States.

when a bank faces higher requirements it also faces a higher threshold at which distribution restrictions can apply. We therefore expect dividend-smoothing banks, which are more concerned about these restrictions, to exhibit greater speed of adjustment following an increase in requirements relative to their peers.

We test this hypothesis with local-projection regressions à la Jordà (2005) on a sample on quarterly data over 2016-2019. We find that, following a shock in core-capital requirements, dividend-smoothing banks adjust their core-capital ratio faster. We also extend this analysis to the full sample and we find that this difference is stronger in the post-2016 period (when MDA restrictions apply) compared to the pre-2016 period. In general, these differences in adjustment disappears within 8 quarters from the shock in requirements.

In the last part of the paper, we test whether these dynamics have a consequence on the supply of credit. As mentioned, what matters for regulatory requirements are the levels of regulatory capital expressed as a percentage of risk-weighted assets. There are two main ways in which banks can increase these risk-weighted capital ratios. First, they can act on the numerator by raising new capital or limiting profit distributions. Second, they can act on the denominator by decreasing the overall amount of risk weighted assets. A reduction in risk-weighted assets can be achieved either by cutting assets - deleveraging - or substituting assets with larger risk-weights for those with lower risk-weights - derisking. The deleveraging and derisking channels are particularly important from a macroprudential perspective.

We test whether distribution restrictions led more affected banks to adjust their capital ratios by cutting lending or reducing risk-taking. We do so by running similar specifications to before which use quarterly lending growth as the dependent variable. We focus both on total and commercial lending, as the latter typically has high risk weights under Basel III Fatouh et al. (2019). We run both DID and local projection analyses and we do not find any significant difference in lending between banks that smooth dividends and banks that do not. While data availability on lending in our sample is limited, this evidence suggests that the introduction of distribution restrictions did not have a strong, unintended contractionary effect on the supply of credit. This is likely because the policy was phased-in gradually, allowing banks to build capital organically rather than by deleveraging.

Our results show some evidence to suggest that the introduction of automatic distribution restrictions incentivised banks to build capital buffers in a timely manner during the Basel III transition period, consistent with regulatory objectives. Importantly, this building of capital ratios did not come at the expense of reduced lending activity suggesting no trade-off between micro- and macro-prudential objectives over this period.

Our work thus far has focused on the evaluation of the automatic distribution restrictions policy during a relatively benign period outside of economic stress. Incentives to deleverage might be higher during periods of stress, such as the Covid-19 crisis, when risk-aversion, uncertainty, and market stigma are likely to be heightened.[3] More work is therefore needed to analyse whether the presence of distribution restrictions generates procyclical responses by banks during episodes of stress.

Overall, our work contributes to the ongoing evaluation of Basel III regulatory standards, and provides early evidence regarding implications of automatic distribution restrictions for the usability of capital buffers during normal and stress periods. The rest of the paper is organized as follows. Section 3.2 reports the related literature and discusses our contribution. Section 3.3 outlines a conceptual framework on how banks can increase capital to react to such restrictions. Section 3.4 describes the data sources and variables. Section 3.5 and 3.6 report the findings on capital ratios of, respectively, the DID and the local-projection analyses. Section 3.7 presents the results for lending. Section 3.8 concludes.

3.2 Related Literature

This paper contributes to the literature that studies the impact of dividend restrictions on the banking sector. As automatic dividend restrictions at the international level applied only from 2016 with Basel III, this literature is fairly new. One of the closest papers to our analysis is Ashraf et al. (2016). The authors use a sample of 8689 banks from 58 countries over 1998-2007 and find that banks pay less dividends in countries with either some regulations on core capital or higher risk-based regulations. They also find that these requirements were not enough to prevent banks to pay out dividends over 2008-2012. Overall, these authors argue in favour of higher risk-based capital requirements in Basel III, as such requirements reduce the likelihood that banks pay excessive dividends. Kanas (2013) focuses on the US and assess how regulations can reduce banks' risk-shifting related to dividends payouts.[4] The author uses a VAR regime-switching model to establish whether the deposit insurance scheme introduced by the Prompt Corrective Act in 1992 contained banks' risk shifting through dividends and finds no evidence in this sense. However, he finds that the Troubled Asset Relief Program (TARP) introduced at the end of 2008, which entailed an increase in the deposit insurance cap, erased the relationship between risk shifting and dividends. He thus argues in favour of stronger sanctions on dividends, also in normal times. In a similar paper, Kanas (2014) shows that in 2008 banks that paid larger dividends also had a higher risk of default. The TARP programme likely contributed in reducing this correlation, as the author finds no relationship between default risk and equity prices over 2009-2011. For the EU, Blanco-Alcántara et al. (2020) show that in the banks smoothed script dividends

[3] See, for example, Basel Committee on Banking Supervision (2021); Saporta (2021); Borsuk et al. (2020a,b) among others

[4] Dividend payouts can be considered a way banks have to shift risk from shareholders, who benefit from the distribution, to debt holders, deposit holders and deposit insurers, who would benefit from retained earnings.

rather than cash dividends. They also show that, interestingly, larger capital requirements are associated with larger script dividends, especially in the years 2014-2018 during Basel III. Overall, these authors study the effect of capital and deposit-insurance requirements on dividend payments. Differently, we empirically assess the impact of dividend restrictions on banks' capital and lending.[5]

A recent paper that addresses the relationship between dividend payouts and lending is Martínez-Miera and Sánchez (2021). The authors consider data on loans by Spanish banks to non-financial corporations during the first three quarters of 2020. Following the ECB recommendation to refrain from making dividend payments over March-October 2020, only the banks that already approved dividend payouts could distribute profits in this period. The authors can thus compare banks that did distribute dividends to banks that did not. They find that banks which were dividend constrained lent significantly more (from 12% to 23% more) to firms. While this evidence may be specific for COVID times, it suggests that restrictions on dividend distributions could have a positive effect on the lending supply. Our results differ from Martínez-Miera and Sánchez (2021) as we focus on whether the introduction of automatic restrictions on dividend distributions, rather than cuts in dividend payments per se, can affect either capital or lending. Specifically, in our framework banks would cut lending to deleverage and increase their risk-weighted capital ratios.

Our paper also relates to the literature on the determinants of dividend payments and dividend smoothing, especially during the financial crisis of 2008. For example, Acharya et al. (2012) and Floyd et al. (2015) show that banks in the US were reluctant to cut dividends, i.e. smoothed dividends, in 2008 and 2009.[6] Koussis and Makrominas (2019) focus on a sample of both European and US banks over 1998-2016 and study the determinants of dividends smoothing. They show that in both countries banks smoothed dividends both before and after the crisis. In addition, they find that, in general, dividend-smoothing banks pay more dividends on average, have lower ownership concentration and lower growth opportunities, and rely more on equity issuance. In addition, in the EU dividend smoothing is negatively correlated with size. We build on this literature by considering dividend smoothing as an indicator of concern for MDA policies.[7]

Moreover, our paper contributes to the large empirical literature assessing the impact of capital requirements on capital ratios and lending. Authors generally find a positive

[5]In doing so, we also complement the theoretical literature assessing the welfare effects of dividend restrictions, which is briefly discussed in Section 3.3, in the description of the conceptual framework of our paper.

[6]Hirtle (2014) shows that share repurchases dropped faster than dividends, though this is not necessarily the case for smaller banks.

[7]In our analysis, we assume that dividend policies change relatively little over time, i.e. dividends are sticky. This logic is in line with dividend smoothing per se, and with the fact that future dividends are mostly based by past dividends (Blanco-Alcántara et al., 2020; Koussis and Makrominas, 2019; Fernau and Hirsch, 2019).

relationship between requirements and regulatory capital banks hold.[8] Interestingly, in a recent paper Gropp et al. (2018) consider the 2011 capital exercise of the European Banking Authority and find that banks increased their risk-weighted ratios by cutting risk-weighted assets (instead of raising new capital). The authors also find negative spillovers on the credit supply, as banks de-risked by cutting risky loans to firms and individuals.[9] We apply the same logic to see whether MDA restrictions implied a cut in lending.

Finally, a recent literature has used local projections first introduced by Jordà (2005) to estimate the impact of capital requirements on regulatory capital and lending. For example, Bahaj et al. (2016) consider a sample of UK banks over 1989-2007 and show that, following a shock in capital requirements, banks with larger legacy assets decrease lending less. We build on this literature to study the dynamic effect of a shock in the MDA trigger point on regulatory capital and lending.

3.3 Conceptual Framework

One of the aim of macroprudential regulation is to reduce the likelihood of systemic crises and, when they happen, contain the contraction in lending supply. To achieve this goal, the regulator uses a mix of ex-ante and ex-post measures. The first are in place at all times and require banks to build enough capital to absorb losses in the occurrence of a systemic crisis. The second can change over time and are tailored to specific phases of credit and business cycles.[10] Among other things, Basel III has introduced a new system of requirements on core capital (CET1). The overall level of CET1 capital banks are required to maintain is given by the sum of minimum and buffer requirements. In this "capital stack", buffer requirements sit on top of minimum requirements.[11]

Before Basel III, buffer requirements implied less severe regulatory consequences than minimum requirements do for banks that breach them.[12] Operating below the buffer requirements was often perceived as acceptable and banks did not have a strong incentive to maintain the CET1 capital strictly above buffer requirements. This lack of incentive is an issue especially in the aftermath of an economic crisis, as banks would keep distributing

[8] For a review of this literature, see for example Galati and Moessner (2013) and Galati and Moessner (2018).

[9] For other papers that study the impact of regulations on lending, see the meta database of the BIS here.

[10] For a summary of ex-ante vis-à-vis ex-post policies, see Lorenzoni (2008); Martinez-Miera and Suarez (2012); Begenau (2019); Clerc et al. (2014); Jeanne and Korinek (2013).

[11] While most of these requirements are an ex-ante measure (in place at all times), the buffer requirements also include ex-post components. These are mostly national and institution-specific requirements which allow national regulators to tailor the requirements to the specific characteristics of an economy or institution at a given moment in time.

[12] For example, before Basel III, UK banks that breached older versions of buffer requirements were "only" asked to rebuild them within a given period, and, in case they did not, the regulatory consequences could be decided by the regulator on a case-by-case basis. In other words, buffer requirements could be considered "soft" and subject to regulatory forbearance.

profits rather than using them to rebuild their capital buffers. The literature reported that this was the case in the US, as in the first months after the financial crash of 2007, financial firms kept paying out dividends rather than rebuilding their capital (e.g. Acharya et al., 2017; Goodhart et al., 2010). This evidence led regulators to introduce restrictions on dividend distributions - so called MDA restrictions - for banks that breach their combined buffer requirements.

These MDA restrictions have all the advantages of an ex-ante measure. First, they set market expectations on what happens when banks breach the combined buffer requirements, thus excluding shocks to banks' value coming from sudden changes in market pricing (e.g. Acharya et al., 2016). Second, MDA restrictions reduce the need of case-by-case interventions by supervisors and related forbearance.[13] These restrictions bear all these clear advantages. But are there any costs?

In the normal course of business, banks generally tend to maintain their capital ratios well above the threshold of minimum capital requirements. They maintain this headroom for two main reasons. First, banks maintain extra capital to avoid dipping below capital requirements when uncertain events lead to a decrease in capital resources. Second, banks hold extra capital to signal financial soundness to the market, which can influence funding costs, in particular if banks approach their (disclosed) minimum capital requirements and their viability comes into question.[14] The hypothetical level of bank capitalisation that is considered safe by investors can be referred to as the market-imposed capital requirement(Berger et al., 1995). The introduction of MDA restrictions on top of the combined buffer requirement may have increased the market-imposed requirements, as investors may now require banks to maintain a safe buffer on top of the MDA trigger point.

There is some anecdotal evidence from the Euro Area (EA) of market concerns about banks' proximity to the trigger point for MDA restrictions. While the new set of rules of Basel III were already in force, in the first months of 2016 there was still some uncertainty about the exact level of the trigger point in the EA. As investors realised that EA banks could be close to the trigger point, they started to liquidate their convertible capital bonds, which led to the February sell-offs in the AT1-bond market.[15] This evidence suggests that investors take into account banks' proximity to the MDA trigger point and that the market-imposed requirement may be consequently higher today than it was in the past.

Logically, banks would build extra buffers of capital to meet this new market-imposed requirement if the cost of losing investors - both debt holders and shareholders - is higher than the cost of increasing their core-capital ratio. Indeed, the literature points out that issuing new capital can be costly for several reasons, among which the opportunity cost of

[13]for the advantages of preventing regulatory forbearance, see for example Freixas and Parigi (2008)
[14]The drivers of voluntary buffers are summarised by, among others, Nier and Baumann (2006).
[15]among others, see Cline (2016) and the case of Deutsche Bank.

capital, transaction costs, such as fees to investment banks and lawyers, and other indirect costs, such as changes in the share prices due to signalling effects (Francis and Osborne, 2010). In addition, Acharya et al. (2011) point out that, especially during the recovery from a financial crisis, owners can be reluctant to issue new equity because of fear for dilution.[16] Differently, banks can increase their core-capital ratio by reducing the denominator, namely the risk-weighted assets. For example, they could do so by either substituting or reducing lending - respectively, de-risking or de-leveraging. On the one hand, banks may de-risk by substituting high-risk lending, like loans to non-financial corporations, with safer lending, such as (secured) mortgages to households (Roulet, 2018; Fatouh et al., 2019). On the other hand, banks may de-leverage by cutting lending altogether (e.g. Gropp et al., 2018). In either case, banks bear the cost of losing market shares following the lending cuts. The rationale behind our findings is that MDA restrictions have increased the market-imposed capital requirement and that banks valued more the cost of losing investors than the cost of raising their risk-based capital ratios.

3.4 Data

In this section, we describe the variables and data we use for the analysis. One of the key elements of our analysis is the methodology to measure banks' concern about MDA restrictions. We argue that this concern can be measured by looking at banks' propensity to pay dividends. As described in Section 3.2, banks tend to maintain dividends stable over time (Floyd et al., 2015). Past dividends are therefore informative of banks' general propensity to pay dividends in the future. In addition, dividend policies tend to vary significantly across banks with different characteristics. For example, Koussis and Makrominas (2019) show that banks' dividends depend on ownership concentration, growth opportunities, reliance on equity issuance and size. We can therefore exploit this variation across dividend policies to differentiate between banks that are concerned about possible restrictions on dividend distributions, and banks that are not. Intuitively, banks that generally pay more, or less volatile, dividends will be more concerned than others about MDA restrictions.

We construct four measures of MDA concerned based on past dividends from the dataset Capital IQ. We focus on quarters before 2016, which is the year of introduction of MDA restrictions. First, we consider the volatility in banks' dividends per share before 2016, and we construct a dummy which equals 1 if a bank is below the cross-sectional median volatility and 0 if it is above. Low volatility of past dividends implies a stable dividend policy over time. Banks with stable dividend policies should be more concerned about MDA restrictions. Second, we consider market expectations about banks' dividend payments, measured as the difference between expectations of Capital IQ's analysts on dividends per share and the

[16] This agency problem is similar to the debt overhang problem described by Myers (1977).

realised dividends per share. We create a dummy which equals 1 if a bank never distributed less than what the analysts expected over the period 2000-2016, and 0 otherwise. Third, we consider differences in banks' propensity to smooth dividends. We construct dividends' payout ratio as the ratio of total dividends paid and net income. We then construct a dummy which equals 1 if a bank paid dividends with negative profits at least once over 2000-2016, and 0 otherwise. Fourth, we hand-collect banks' annual reports over 2012-2016 and we create a dummy which equals 1 if a bank clearly mentions a dividend target in its annual reports, and 0 otherwise.[17]

Table 3.1: Summary Statistics

Variable	**N**	**Mean**	**SD**	**p25**	**p50**	**p75**	**Min**	**Max**
CET1 Ratio (% RWA)	1359	13.01	2.86	11.16	12.30	14.28	1.50	25.10
New Loans (Δlog)	1003	0.01	0.06	-0.02	0.01	0.04	-0.27	0.85
New Comm. Loans (Δlog)	984	0.04	0.52	-0.04	0.01	0.06	-4.07	5.06
CET1 Requirement (% RWA)	1359	7.71	3.26	7.00	8.04	9.75	0.00	18.86
Profits (% TA)	1359	0.13	0.20	0.08	0.15	0.22	-2.52	0.59
Deposits (% TA)	1359	58.12	15.90	43.50	61.79	70	21.58	87.60
Assets (log)	1359	12.87	1.36	12.14	13.11	13.99	7.13	15.06
Market Cap. (£)	1359	6.63	3.46	4.20	6.21	8.51	0.21	22.51
Sub. Debt (% TA)	1359	1.44	0.89	0.79	1.20	1.88	0.00	5.21
Short Debt (% TA)	1359	9.46	8.13	3.56	7.10	13.15	0.01	42.90

Notes: This table reports the summary statistics for the main variables used in the analysis. Their unit of measurement is expressed in parentheses. CET1 Ratio is the ratio of CET1 capital over risk weighted assets (RWA). New Loans and New Comm. Loans are the log change of balance-sheet stocks of, respectively, total and commercial loans. CET1 Requirement is the requirement for CET1 capital, expressed as a percentage of (RWA). Profits and Deposits are, respectively, total profits and deposits (stocks) expressed as percentages of total assets. Assets is the natural logarithm of total assets. Market Cap. is the market capitalization, i.e. the aggregate valuation of the bank (share price times number of shares). Sub. Debt and Short Debt are, respectively, subordinated debt and short-term debt expressed as percentage of total assets. Div. Per Share is dividends paid divided the number of shares. Div. Payouts is dividends paid divided by net income.

These binary variables differentiate banks between two groups, namely concerned and not concerned about MDA restrictions. We are then interested to test whether these two groups manage regulatory capital differently after the introduction of the restrictions. MDA restrictions apply when banks' CET1 capital, expressed as a percentage of risk-weighted assets, falls below the overall CET1 requirements. The CET1 ratio is therefore the main dependent variable of interest for our analysis. We source data on CET1 ratios and all other balance-sheet controls from Capital IQ. The final sample includes quarterly data on 65

[17]For all these 4 variables, we exclude the years 2008 and 2009 to avoid the noise around the financial crisis. In the robustness checks, we propose a version of the dividend-smoothing dummy that excludes the years after 2008, and that therefore cannot be affected by early announcements about MDA restrictions, for example by the BIS.

listed banks in 24 countries over 2013-2019.[18] Table 3.4 reports the summary statistics.[19] The mean for the CET1 ratio is 13.01%, which is in line with the average at December 2017 for the major group of banks considered in the Basel III Monitoring Report of March 2019 (12.9%).[20] New Loans and New Commercial Loans are the variables we use for the lending analysis. They are quarter-to-quarter variations in the natural logarithm of stocks of, respectively, loans and commercial loans reported by Capital IQ. Commercial loans are generally short-term loans to corporations. CET1 Requirements are the overall requirements for CET1 ratios, which we hand collected from banks annual reports.[21] In the analysis, we include balance-sheet controls. Profits and Deposits are, respectively, total profits and deposits (stocks) expressed as percentages of total assets. Assets is the natural logarithm of total assets. Market capitalization is the aggregate valuation of the bank (share price times number of shares). We also consider subordinated and short-term debt, expressed as percentage of total assets.

3.5 Before and After 2016

In this Section we focus on the differences in CET1 Ratio before and after 2016 between banks that are concerned about the MDA policy and banks that are not. We do so with a Difference-in-Differences (DID) regression model, which can be written as follows:

$$\begin{aligned} y_{ict} = \beta(Post2016_t \times MDA\ Concern_{ic}) + (Post2016_t \times \boldsymbol{X}_i')\boldsymbol{\gamma} \\ \boldsymbol{X}_{it-1}'\boldsymbol{\lambda} + \boldsymbol{\delta}_{ct} + \boldsymbol{\delta}_i + \epsilon_{ict} \end{aligned} \tag{3.1}$$

y_{ict} is the CET1 Ratio of banks i in country c at quarter t, which is the dependent variable of interest. $Post2016_t$ is a dummy variable which equals 0 before 2016 and 1 after. $MDA\ Concern_i$ is the measure for concern about MDA restrictions of bank i. We measure this concern with four binary variables, namely low dividend volatility, no negative surprise, presence of dividend smoothing and dividend target.[22] Values 0 and 1 of these dummies

[18] Note that We use dividends data over 2000-2016 to build the cross-sectional variables that we use in the 2013-2019 sample. The 24 countries in the final sample are Australia, Austria, Belgium, Canada, China, Cyprus, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Lithuania, Malta, Netherlands, Poland, Portugal, Spain, Sweden, Switzerland, United Kingdom and United States.

[19] They refer to the sample for which CET1 ratio and all controls are available. The regressions will run on samples with less observations, depending on the type of dividend variable used. Aside from data availability on dividends per se, the number of observations will be generally lower because we consider only banks that have non missing observations on dividends for at least 8 quarters over 2000-2016. In addition, the sample for the lending regressions will also have much less observations, as observations drop when we exclude missing data for both lending and controls.

[20] These are banks that have Tier 1 capital of more than Euros 3 billion and are internationally active. The report can be found here.

[21] They are the sum of minimum and buffer requirements.

[22] For a detailed description of these four variables, refer to Section 3.4.

indicate that banks are, respectively, not concerned and concerned about MDA restrictions. A positive and significant β coefficient would therefore indicate that banks that were concerned about MDA restrictions increased their they regulatory-capital ratios more after 2016.

$\boldsymbol{X}_i$ is a set of cross-sectional controls that could correlate with banks' capital-management decisions after 2016 and also with banks' propensity to pay dividends. First, banks that pay a significant amount of dividends may also be the ones with especially low levels of regulatory capital. Indeed, as banks pay dividends, they retain less profits, and therefore have less resources to build regulatory capital. In addition, Basel III introduced generally higher requirements for regulatory capital from January 1st 2016 onwards.[23] As a result, banks that had low levels of regulatory capital before 2016 may have been the ones increasing CET1 ratios more following Basel III. To control for this confounding factor, we compute bank-level averages of CET1 ratios before 2016 and we create a dummy which equals 0 and 1 for banks that are, respectively, above and below median. Second, Basel III also introduced higher requirements for Global Systemically Important Banks (G-SIB).[24] We thus include a dummy which equals 1 for G-SIBs and 0 otherwise. Both dummies in $\boldsymbol{X}_i'$ are interacted with the $Post2016$ dummy, and we expect positive $\boldsymbol{\gamma}$ coefficients.

Moreover, $\boldsymbol{X}_{it-1}$ is a set of bank-level, time-varying variables that control for balance-sheet factors that could drive changes in regulatory capital. They include CET1 requirements expressed as a percentage of risk weighted assets, which control for different regulatory requirements on CET1 capital that vary over time. We also include the natural logarithm of total assets as a proxy for banks' size (Francis and Osborne, 2012). Furthermore, we include measures for profits and market capitalization, as more profitable and capitalized banks can raise capital at short notice with lower costs (Gropp and Heider, 2010). Similarly, we control for levels of banks' short-term and subordinated debt, as transfers from one to another change the cost of funding and therefore banks' ability to raise capital (Gimber and Rajan, 2019). Finally, we include a measure of banks' deposits to control for the well-documented trade-off between capital and liquidity (Diamond and Rajan, 2000; Gorton and Winton, 2017).[25] $\boldsymbol{\delta}_{ct}$ and $\boldsymbol{\delta}_i$ are, respectively, country-time and bank fixed effects, and ϵ_{ict} is the normally-distributed error term.

[23] Some of these stem from higher buffer requirements, namely the Capital Conservation Buffer and the Countercyclical Capital Buffer.

[24] Specifically, Basel III introduced a higher buffer requirement for systemically important banks, also referred to as the G-SIB Buffer or G-SII Buffer (G-SII stays for Global Systemically Important Institutions).

[25] In the baseline regressions, we do not include measures for risk weighted assets, loans and non-performing loans as other authors do. We do not control for such variables because we aim to grasp adjustment in the capital ratio that could come both through the numerator (deleveraging) and the denominator (derisking). In the robustness checks, we test our results by including such controls.

3.5.1 Baseline

Table 3.2 reports the regression results. In all specifications, we use the first lag of all control variables in order to reduce the simultaneity bias that characterise balance-sheet regressions. Columns 1 to 4 and 5 to 8 report results when we, respectively, exclude and include interactions of time-invariant confounding factors and the policy dummy.[26] We start by considering coefficients for the interaction terms of interest in columns 1 to 4. Among the considered measures for MDA concern, the positive and statistically-significant coefficient for the dummy on dividend smoothing indicate that banks which historically smoothed dividends increase their capital ratios more after 2016. Specifically, the coefficient estimate of 1.42 indicates that the implied negative difference in CET1 ratios between banks that did and did not smooth dividends is 1.42 percentage points smaller after 2016.[27] While this result is quite strong for MDA concern measured with dividend smoothing, the other MDA-concern dummies do not detect any significant difference. In general, we find that the DID coefficients for the dummies on low dividend volatility and absence of negative surprises on dividend distributions change with respect to the type of specification and subsample used.[28] Differently, the DID coefficient for the dummy on the dividend target is statistically insignificant in all the attempted specifications.[29]

The coefficients for the rest of the controls are generally in line with our expectations. The negative and at times significant coefficients on CET1 requirements suggest that banks with larger requirements are also the ones with less capital. Intuitively, regulators apply larger requirements for banks that are poorly capitalised, while requirements are less strict for high-capital banks. The positive and at times significant coefficient on profits suggests that more profitable banks also hold more capital, which is in line with the logic of retained earnings. The positive and statistically significant coefficients on deposits are somewhat in contrast with the trade-off between liquidity and capital, as they imply that more liquid banks are also the ones with more capital. However, Distinguin et al. (2013) find that, when considering core deposits as a measure for liquidity - as we are doing here -, banks increase their regulatory capital when they are more liquid.[30]

Columns 5 to 8 show the regression results for more restrictive specifications in which we control for two possible confounding factors, namely banks with a low starting-level of capital

[26] What is referred to as $\boldsymbol{X}_i'$ in Equation 3.1.

[27] The implied difference is assumed to be negative as banks that pay more dividends tend to have lower regulatory capital. If the difference was positive, the interpretation would be that the implied positive difference increases by 1.42 percentage points after 2016.

[28] For example, in the subsample with non-missing observations for risk-weighted assets, loans and non-performing loans, we find a positive coefficient for low volatility, that is significant at the 5-% level.

[29] These specifications include the inclusion of controls one by one and in different groups, and different sets of fixed effects. They are available upon request.

[30] This is also in line with a time dimension of capital management during Basel III, which included liquidity constraints.

Table 3.2: Baseline DID

VARIABLES	(1) CET1 Ratio	(2) CET1 Ratio	(3) CET1 Ratio	(4) CET1 Ratio	(5) CET1 Ratio	(6) CET1 Ratio	(7) CET1 Ratio	(8) CET1 Ratio
Post2016 × Low Volatility	-0.173				-0.083			
	(0.382)				(0.289)			
Post2016 × No Neg. Surprise		0.442				0.631		
		(0.711)				(0.442)		
Post2016 × Smoothing			1.420***				1.043***	
			(0.358)				(0.310)	
Post2016 × Target				-0.143				-0.078
				(0.299)				(0.297)
Post2016 × Low Cap. Surplus					0.847**	0.942***	0.663**	0.824**
					(0.322)	(0.326)	(0.247)	(0.326)
Post2016 × G-SIB					0.468	0.705	1.078***	0.520
					(0.477)	(0.464)	(0.255)	(0.419)
CET1 Req.	-0.127*	-0.094	-0.122*	-0.122*	-0.069	-0.064	-0.119**	-0.075
	(0.065)	(0.082)	(0.072)	(0.067)	(0.056)	(0.047)	(0.047)	(0.059)
Profits	0.249	0.440**	0.799*	0.312	0.308	0.348*	0.760**	0.307
	(0.332)	(0.200)	(0.405)	(0.197)	(0.334)	(0.191)	(0.369)	(0.194)
Deposits	0.046**	0.029	0.019	0.052**	0.042**	0.059***	0.047***	0.052**
	(0.020)	(0.031)	(0.027)	(0.021)	(0.018)	(0.017)	(0.016)	(0.021)
ln(Assets)	-0.018	1.292	-0.228	1.173	0.503	2.035*	0.184	1.470*
	(1.353)	(1.654)	(1.473)	(1.010)	(1.161)	(1.195)	(1.148)	(0.857)
Market Cap.	0.070	0.090	0.077	0.056	0.094	0.104	0.079	0.077
	(0.133)	(0.157)	(0.135)	(0.118)	(0.132)	(0.155)	(0.130)	(0.118)
Subord. Debt	-0.059	-0.061	-0.218	0.058	-0.028	0.207	-0.059	0.057
	(0.101)	(0.257)	(0.192)	(0.112)	(0.107)	(0.163)	(0.120)	(0.112)
Short Debt	-0.008	-0.037	-0.030	-0.032	-0.008	-0.020	-0.018	-0.027
	(0.027)	(0.033)	(0.031)	(0.024)	(0.025)	(0.028)	(0.028)	(0.022)
Observations	1,002	719	849	1,004	1,002	689	819	1,004
Adjusted R-squared	0.900	0.884	0.921	0.891	0.905	0.892	0.927	0.895
FE	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct

Notes: Table 3.2 reports the baseline results for a difference-in-differences analysis comparing periods before and after 2016. The dependent variable is the CET1 ratio. Post2016 is a dummy which equals 0 and 1 for quarters, respectively, before and after 2016. Low Volatility is a dummy which equals 1 for banks with a pre-2016 average of dividends per share below median, and 0 for banks above median. No Neg. Surprise is a dummy which equals 1 for banks that always distributed dividends in line or above analysts expectations over 2000-2016, and 0 otherwise. Smoothing is a dummy which equals 1 for banks that never paid dividends with negative profits over 2000-2016, and 0 otherwise. Target is a dummy which equals 1 if a bank has mentioned a dividend target in annual reports over 2012-2016, and 0 otherwise. Low Cap. Surplus is a dummy which equals 1 for banks with pre-2016 levels of regulatory CET1 capital below median, and 0 for banks above median. G-SIB is a dummy which equals 1 if a bank is defined as Global Systemically Important Bank, and 0 otherwise. CET1 Requirement is the requirement for CET1 capital, expressed as a percentage of (RWA). Profits and Deposits are, respectively, total profits and deposits (stocks) expressed as percentages of total assets. Assets is the natural logarithm of total assets. Market Cap. is the market capitalization, i.e. the aggregate valuation of the bank (share price times number of shares). Sub. Debt and Short Debt are, respectively, subordinated debt and short-term debt expressed as percentage of total assets. FE are the fixed effects included. Standard errors clustered by banks are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

and G-SIBs. The DID coefficient with dividend smoothing decreases in absolute values, which indicates that there could be indeed correlation with the two controls after 2016, as expected. For example, part of the effect grasped in column 3 could be due to differences between non G-SIBs and G-SIBs, as the latter simply faced higher requirements than the first after 2016. This hypothesis is confirmed by the interactions of Low Capital Surplus and G-SIB with the policy dummy, as both are positive and statistically significant. Intuitively, poorly-capitalised banks increased capital more after 2016 because they faced generally higher requirements. In addition, G-SIBs increased capital more after 2016 because they faced GSIB-specific buffer requirements. Even though the coefficient of dividend smoothing is lower than in column 3, it remains statistically significant at conventional standards. The coefficients of the other 3 measures of MDA concern remain statistically insignificant.

There can be multiple reasons why these other three measures do not deliver the expected results. For example, the dummy on low dividend volatility differentiates banks using the median value as threshold. It could therefore be that the cutoff is not neat enough to provide a clean separation between banks that are concerned or not about MDA. The binary variable on dividend targets could have a similar issue, as banks with a dividend target could simply commit to a low payout ratio. Differently, dividend smoothing is computed with the condition that a bank must have paid dividends with negative margins at least once. As dividend-smoothing banks are compared to banks that never paid dividends with negative margins, the related binary variable provides a neater differentiation across banks.

Overall, Table 3.2 suggests that banks that do dividend smoothing have increased their CET1 ratios more after 2016, when MDA restrictions were introduced. Importantly, this result is not driven by the differences in samples for different measures. In Table 3.A.1 in Appendix 3.A, we estimate the same specifications on a sub-sample with available observations for all 4 measures of MDA concern, and results remain approximately unchanged. We focus on the more conservative estimate of column 7 and in the next sub section we test the robustness of this result.

3.5.2 Robustness and Further Checks

All regressions in Table 3.2 include country-quarter and bank fixed effects. This is our choice for the baseline results as our dataset includes multiple countries. In this set up, there can be unobservable country-level factors that could change over time and that could bias the results. These factors are controlled for by country-quarter fixed effects. We start by relaxing this constraint and we estimate our model with a set of less-constraining fixed effects. They include bank and time fixed effects, and country, bank and time fixed effects.

Results are reported in respectively columns 1 and 2 of Table 3.3. In these regressions, we also control for relevant country-level factors, which include the growth of real GDP, the Consumer Price Index (CPI) and the unemployment rate.[31] With these fixed effects, the DID coefficients for dividend smoothing are statistically insignificant at conventional standards. The coefficients of the other controls remain approximately unchanged, and the coefficient on subordinated debt becomes significant. None of the coefficients of the country-level variables is statistically significant. Furthermore, the adjusted R squares, i.e. .794 and .790, dropped significantly with respect to column 7 of Table 3.2 (.927). Overall, it appears that unobserved country-quarter factors are quite relevant, and the model is less precise when we do not control for them. These results motivates further our decision to include country-quarter

[31] All sourced from OECD Statistics.

Table 3.3: Robustness DID

VARIABLES	(1) CET1 Ratio	(2) CET1 Ratio	(3) CET1 Ratio	(4) CET1 Ratio	(5) CET1 Ratio
Post2016 × Smoothing	-0.388	-0.388	0.903***		0.805***
	(0.494)	(0.499)	(0.268)		(0.235)
Post2016 × Smoothing Alt.				0.805*	
				(0.417)	
Post2016 × Low Cap. Surplus	1.327***	1.327***	0.607**	0.826***	0.465**
	(0.387)	(0.390)	(0.240)	(0.203)	(0.206)
Post2016 × GSIB	0.078	0.078	1.130***	1.187***	1.210***
	(0.485)	(0.489)	(0.239)	(0.265)	(0.239)
CET1 Req.	-0.182***	-0.182***	-0.189***	-0.100*	-0.038
	(0.067)	(0.067)	(0.062)	(0.050)	(0.058)
Profits	0.781*	0.781*	0.800***	0.635*	0.819**
	(0.412)	(0.416)	(0.279)	(0.338)	(0.308)
Deposits	0.007	0.007	0.048**	0.045**	0.026
	(0.028)	(0.029)	(0.019)	(0.018)	(0.018)
ln(Assets)	-2.078	-2.078	-0.504	0.847	-2.975***
	(1.481)	(1.495)	(1.263)	(1.034)	(1.021)
Market Cap.	0.018	0.018	0.079	0.148	0.185
	(0.061)	(0.061)	(0.130)	(0.132)	(0.114)
Subord. Debt	-0.403**	-0.403**	-0.046	-0.245	-0.057
	(0.173)	(0.174)	(0.103)	(0.165)	(0.141)
Short-term Debt	0.013	0.013	-0.002	-0.003	-0.006
	(0.023)	(0.024)	(0.028)	(0.028)	(0.027)
RGDP Growth	0.016	0.016			
	(0.062)	(0.062)			
CPI	-0.288	-0.288			
	(0.186)	(0.188)			
Unemployment	0.080	0.080			
	(0.128)	(0.129)			
Loans					-0.030
					(0.040)
NPL Ratio					0.106
					(0.083)
ARW					-0.188***
					(0.038)
Observations	897	897	850	630	602
Adjusted R-squared	0.794	0.790	0.925	0.926	0.950
FE	i-t	i-c-t	i-ct	i-ct	i-ct

Notes: Table 3.2 reports the robustness checks for a difference-in-differences analysis with dividend smoothing comparing periods before and after 2016. The dependent variable is the CET1 ratio. Post2016 is a dummy which equals 0 and 1 for quarters, respectively, before and after 2016. Smoothing is a dummy which equals 1 for banks that never paid dividends with negative profits over 2000-2016, and 0 otherwise. Low Cap. Surplus is a dummy which equals 1 for banks with pre-2016 levels of regulatory CET1 capital below median, and 0 for banks above median. G-SIB is a dummy which equals 1 if a bank is defined as Global Systemically Important Bank, and 0 otherwise. CET1 Requirement is the requirement for CET1 capital, expressed as a percentage of (RWA). Profits and Deposits are, respectively, total profits and deposits (stocks) expressed as percentages of total assets. Assets is the natural logarithm of total assets. Market Cap. is the market capitalization, i.e. the aggregate valuation of the bank (share price times number of shares). Sub. Debt and Short Debt are, respectively, subordinated debt and short-term debt expressed as percentage of total assets. In Column 3, CET1 requirements and all balance-sheet controls enter without lag. RGDP Growth, CPI and Unemployment are, respectively, the growth rate of real GDP, the Consumer Price Index, and the unemployment rate. Loans is the amount of outstanding loans over total assets. NPL Ratio is the share of non-performing loans over total loans. ARW is the average of risk weights. FE are the fixed effects included. Standard errors clustered by banks are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

fixed effects in the baseline specification.

In column 3 we relax the assumption on the simultaneity bias and we include the present values of all controls, rather than their first lag. While inverse causality could bias the results, with this specification we can model the accounting principles of balance sheet variables. For example, if profits increase, retained earnings and regulatory capital should increase without lag. The results show that the coefficient estimates are approximately unchanged.[32]

In column 4 we use a variation of the binary variable on dividend smoothing. In the baseline version of our variable, we considered dividends and margins in the period 2000-2015 - excluding the crisis years (2008 and 2009) - to have the longest period available. In addition, we considered only banks that had at least two years of available observations. While this setup maximises the sample size, the dividend data in the last years of the sample might be subject to issues of reverse causality. The Bank of International Settlements published the guidelines for the implementation of Basel III and MDA restrictions already in 2011. The regulatory bodies of single countries then translated these guidelines into legislation, which became active on January 1st 2016. As a result, market participants already had at least some information on the introduction of MDA restrictions in 2011. Banks might have thus decided to cut dividends between 2011 and 2015, with the goal to build a safe capital buffer by 2016, when restrictions came into force. To control for this aspect, we compute our dummy variable for dividend smoothing considering data only over 2000-2008 and we re-estimate the baseline results. The DID coefficient of interest reduces in size, moving from 1.043 to 0.805, and it is now significant only at the 10-% level. Aside from the data used to construct the dividend-smoothing variable, another major difference with the baseline regression is the sample size. As we consider only those banks with at least two years of observations available, using data on dividends before 2008 implies a drop of 200 observations.

In column 5, we follow the literature and we augment our model with controls of components of risk weighted assets. In the baseline, we did not include these controls as we aimed to allow movements in the denominator of the capital ratio - and not only the numerator. We include the average of banks' risk weights and non-performing loans to total assets, which control for banks' risk profile (Francis and Osborne, 2010). Riskier banks have usually lower regulatory capital. We also controls for total lending volumes by including the ratio of loans over total assets. The estimate of the DID coefficient on dividend smoothing remains positive and statistically significant. However, note that the inclusion of such controls imply another large drop in the number of observations, which now amounts to only 602.

We also assess whether the estimated effect is in line with the timing of the introduction of MDA restrictions. To do so, we estimate the following equation:

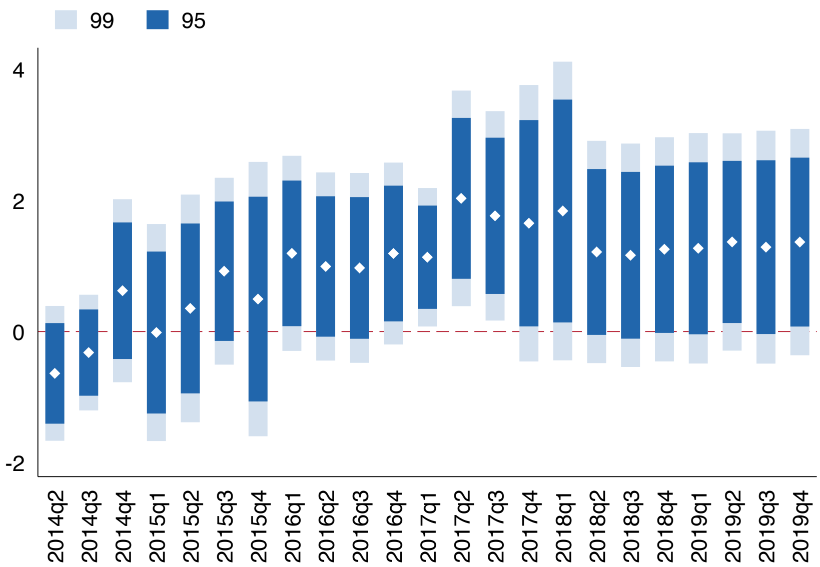
$$\begin{aligned} y_{ict} = &\sum_{l=2014Q2}^{2019Q4} (\beta_l 1\{l=t\} \times MDA\ Concern_{ic}) + \sum_{l=2014Q2}^{2019Q4} (1\{l=t\} \times \boldsymbol{X}_i')\gamma_l) + \\ &\boldsymbol{X}_{it-1}'\boldsymbol{\lambda} + \boldsymbol{\delta}_{ct} + \boldsymbol{\delta}_i + \epsilon_{ict} \end{aligned} \tag{3.2}$$

$1\{l=t\}$ are quarter dummies which equal 1 for a specific quarter and 0 otherwise. Coefficients β_l measure the difference between banks that are concerned and not concerned about MDA

[32]The coefficient on profits increase in magnitude, as expected.

restrictions in a specific quarter compared to the first quarter of 2014. If there are no other factors affecting such difference but the ones we control for, coefficients β_l should become statistically different from 0 only after the first quarter of 2016. We plot the estimates of β_l in Figure 3.1.

Figure 3.1: Impact over Time



Notes: Figure 3.1 reports the coefficient estimates for interactions between the dummy for dividend smoothing and quarterly dummies. The effects are all relative to the first quarter of 2014. Standard errors clustered by banks are used to compute the confidence bands. 95-% confidence bands are reported in dark blue, while 99-% confidence bands are reported in light blue. Results are statistically significant when confidence bands do not cross the zero line (dashed red).

We report both 95-% (dark blue) and 99-% (light blue) confidence intervals. The estimated impact in the corresponding quarter is statistically different from the estimated impact in the first quarter of 2014 (reference quarter) when the confidence bands exclude zero (red dashed line). The graph shows that the estimated impact becomes statistically significant at the 95-% level only in the first quarter of 2016. The impact keeps increasing, with a significance at the 99-% level in three quarters of 2017, to then stabilise in 2018 and 2019. This exercise indicates that, compared to other banks, banks that smoothed dividends increased their CET1 ratios only after 2016, when MDA restrictions applied. In Appendix 3.A, we report similar estimates when we use the other measures for MDA concern. These graphs broadly confirm the results of Table 3.2. Figures 3.A.1 and 3.A.3 show that when we use the dummy for low dividend volatility and target the estimated impact is never statistically different from zero. Figure 3.A.2 show that the estimated impact is positive at times, but the timing does not coincide with the introduction of MDA restrictions.

3.6 Shock in Capital Requirements

The DID regressions shows that dividend-smoothing banks increased their capital ratio more after 2016. The advantage of this type of analysis is that it provides a simple framework to assess the trends in regulatory capital before and after MDA restrictions came into place. However, the challenge of a pre-post comparison is to neatly disentangle the effect of MDA restrictions from the impact of other measures of the Basel-III package. While we addressed these issues by including additional controls for capital level and G-SIB status, there could be other policy measures that might confound our results.

For this reason, we consider an alternative approach based on local projections and shocks in capital requirements, which focuses only on data after 2016. This identification strategy exploits the fact that banks are subject to bank-specific capital requirements. On top of the Basel requirements, which are the same across jurisdictions, national supervisors can apply additional requirements that are tailored to address capital shortages of single banks. For example, in the UK and in the EU these requirements are set by, respectively, the PRA and the ECB, and are known as Pillar 2 requirements.[33] Moreover, these requirements focus on CET1 capital, which is the core capital instrument for loss absorption. Different banks will therefore have different CET1 requirements, which are a combination of international and national (idiosyncratic) requirements.

MDA restrictions apply when the CET1 ratio falls below the level of (cumulated) CET1 requirements.[34] It follows that different CET1 requirements imply different MDA trigger points. We can therefore exploit this bank-quarter variation to assess whether dividend-smoothing banks that are subject to a change in the MDA trigger point increase capital more or quicker than other banks. The adjustment would be stronger for dividend-smoothing banks as they are more concerned of being close to the MDA trigger point. The speed of adjustment may be key here. Indeed, it is reasonable to believe that all banks would adjust to an increase in requirements in the long run. However, dividend-smoothing banks would do it faster than others. This difference in the speed of adjustment can be more precisely estimated in a set up of local projections.

Local projections (LPs) were first introduced by Jordà (2005). In a nutshell, Jordà (2005) proposes to transform the data such that coefficients of a set of regressions can be interpreted as the response of the dependent variable through time to a shock in an independent variable of interest. These responses are conceptually the same of impulse-response functions

[33] Specifically, in the UK they are called Pillar 2A. Also, in addition to bank-specific capital requirements, national supervisors can apply a bank-specific buffer requirement. For example, in the UK this buffer requirement is called the PRA Buffer.

[34] More specifically, these cumulated buffer requirements include capital requirements and buffer requirements. MDA restrictions apply when the level of CET1 ratio dips below the level of buffer requirements, which sit on top of capital requirements.

estimated with a VAR framework. One of the advantages of LPs is that we can use the specification of a simple regression, with related interactions, controls and fixed effects, to estimate dynamic responses. Some authors in the literature have already used LPs to estimate reactions of capital ratios to a shock in capital requirements. In this paper, we proceed similarly to (Bahaj et al., 2016) and we start by defining a sequence of dependent variables over different horizons $h = \{0, 1, 2, ..., H\}$:

$$Y_{t+h,ic} = y_{t+h,ic} - y_{t-1,ic} \tag{3.3}$$

$Y_{t+0,i}$ is the difference between the present value of the CET1 ratio and its first lag for bank i in country c. Then $Y_{t+1,i}$ is the difference between the value of the CET1 ratio in the first future quarter and the same first lag for bank i in in country c. We repeat this process for 8 future quarters (H=8), i.e. we obtain 9 series of Y. Intuitively, we can interpret these series as the cumulative change in the capital ratio for 8 future quarters. We use these series to estimate the following models:

$$Y_{t+h,ic} = (\beta_1^h MDA\ Concern_{ic} + \beta_2^h)\Delta REQ_{t-1,ic} + \boldsymbol{\delta}_{ct} + \boldsymbol{\delta}_i + \epsilon_{t+h,i} \tag{3.4}$$

with $h = 0, ..., H$ and $H = 8$, i.e. 9 models. For simplicity, we omit interactions with cross-sectional controls and other controls. $MDA\ Concern_{ic}$ is the dummy for MDA concern measured on dividend smoothing and $\Delta REQ_{t-1,ic}$ is the first lag of the quarter-to-quarter change in CET1 requirements. $\boldsymbol{\delta}_{ct}$ and $\boldsymbol{\delta}_i$ are, respectively country-time and bank fixed effects, and $\epsilon_{t+h,i}$ is the normally-distributed error term. β_1^h and β_2^h are the coefficients of interest. β_2^h measures the impact over time of a one percentage-point shock in capital requirements on CET1 ratios for banks that do not smooth dividends. Formally:

$$\beta_2^h = \frac{\partial E(Y_{t+h,ic}|MDA\ Concern_{ic} = 0)}{\partial \Delta REQ_{t-1,i}} \tag{3.5}$$

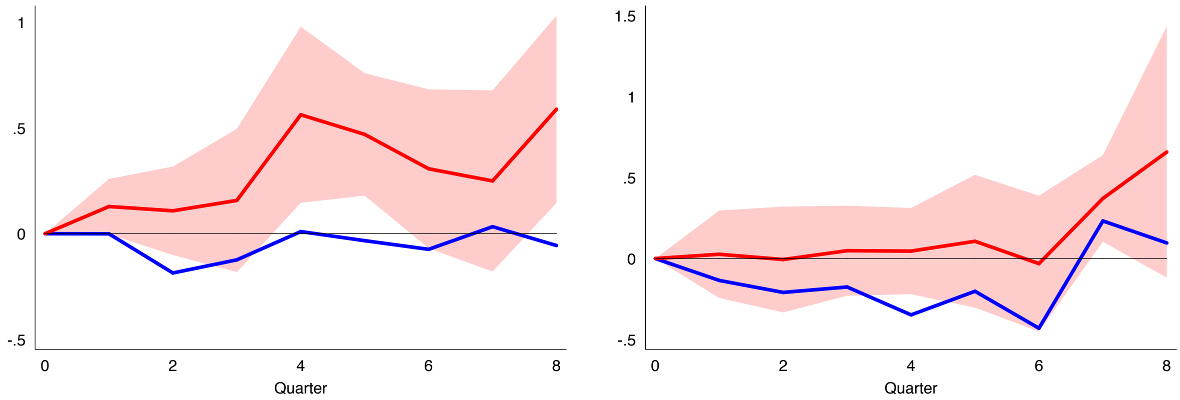
To obtain the same effect for banks that smooth dividends, we can consider the sum of coefficients β_1^h and β_2^h:

$$\beta_1^h + \beta_2^h = \beta_3^h = \frac{\partial E(Y_{t+h,ic}|MDA\ Concern_{ic} = 1)}{\partial \Delta REQ_{t-1,i}} \tag{3.6}$$

The evolution of these coefficients over time would measure how different sets of banks adjust their CET1 ratios in response to a shock in CET1 requirements. If dividend smoothing banks adjust faster than other banks, we would expect β_3^h to be above β_2^h for some quarters after the shock in requirements.
Figure 3.2 plots these impulse-response coefficients for 8 periods ahead of a one percentage-point shock in CET1 requirements. Values for β_2^h and β_3^h are in, respectively, blue and red.

The red shaded areas report the 90-% confidence band for β_3^h.[35] The difference between β_1^h and β_3^h is statistically significant if the confidence band for β_3^h does not cross the estimate for β_2^h. Panels (a) and (b) report results when we estimate Equation 3.4, respectively, without controls and with controls.[36]

Figure 3.2: Shock in Requirements and CET1 Ratio (After 2016)



(a) No Controls

(b) With Controls

Notes: Figure 3.2 reports results obtained with local projections with a sample over 2016-2019. The lines are responses of CET1 ratios 8 quarters after a one percentage-point shock in CET1 requirements. Panel (a) and (b) report results for regressions, respectively, without controls and with controls. Controls include both cross-sectional variables interacted with shocks in CET1 requirements and bi-dimensional balance-sheet variables. Red and blue lines are the responses for, respectively, banks that paid dividends with negative profits at least once over 2000-2016, i.e. dividend-smoothing banks, and banks that did not - namely concerned and not concerned about MDA restrictions. The red shaded area is the 90-% confidence band for the responses of dividend-smoothing banks. The difference in responses between the two groups of banks is statistically significant when the confidence band for dividend-smoothing banks does not cross the response of other banks.

Let us first consider the less constraining results of Panel (a). First of all, CET1 ratios of both groups do not respond on impact to an increase in requirement. This is because shocks in CET1 requirements enter the estimated equation with a lag. Second, the model estimates that, in response to a shock in requirements, the CET1 ratio of banks that smooth dividends increase more than for other banks. In particular, this difference is statistically significant for 5 of the 8 periods after the shock. Panel (b) shows that, when we include controls, this difference is less marked, and it is statistically significant only for one quarter out of the 8 considered. In addition, the CET1 ratio of banks that do not smooth dividends is in the negative territory for most of the considered quarters. This could be considered a puzzle, as

[35] As in our baseline specification, standard errors are clustered by banks. To obtain the standard errors for β_3^h, which is the combination of coefficients $\beta_1^h + \beta_2^h$, we use the following formula: $\hat{SE}^h_{\beta_3^h} = \sqrt{Var(MDAConcern)^h + Var(\Delta REQ)^h + 2Cov(MDAConcern, \Delta REQ)^h}$. The confidence band for β_3^h is computed using $\hat{\beta}_3^h + 1.645 * SE_{\hat{\beta}_3^h}$.

[36] In the version with controls we also include interactions of the dummies for low levels of regulatory capital and G-SIB status with ΔREQ.

we would expect capital to increase following an increase in requirements.

The estimates of coefficients β_1^h and β_2^h are reported in Tables 3.B.1 and 3.B.2. The coefficients β_2^h are the coefficients for the shock in capital requirements, represented as the blue lines in Figure 3.2. Estimates for β_1^h, i.e. the interaction between shock in requirements and dividend smoothing, generally reflect the findings described above. These coefficients can be interpreted as the difference in responses of CET1 ratios between banks that smooth dividends and banks that do not.[37]

The results reported above are obtained with data over 2016-2019. This methodology bears a trade-off. On the one hand, it has the advantage of excluding possible confounding factors related to different regulations that were included in Basel III. On the other hand, it exploits only a limited share of the variation in capital requirements, as quarters before 2016 are excluded a priori. With this trade-off in mind, we generalise our methodology to the full sample of data over 2013-2019. We perform this exercise by estimating the following model:

$$Y_{t+h,ic} = (\beta_4^h Post2016_t \times MDA\ Concern_{ic} + \beta_5^h Post2016_t)\Delta REQ_{t-1,i} + ... + \epsilon_{t+h,ic} \quad (3.7)$$

For simplicity, we omit the added interactions of the model and the fixed effects. $Post2016$ is the dummy we used in Table 3.2, which is 0 before 2016 and 1 after. We can interpret coefficient estimates as we would interpret triple interactions. Specifically, coefficients β_5^h estimate whether banks that do not smooth dividends react differently to a shock in capital requirements before and after 2016. More formally, we can write:

$$\beta_5^h = \frac{\partial E(Y_{t+h,ic}|MDA\ Concern_{ic} = 0)}{\partial \Delta REQ_{t-1,i} \partial Post2016_t} \quad (3.8)$$

As we would expect no major differences in the way react, β_5^h should be close to zero, or not that large. The combination $\beta_4^h + \beta_5^h$ estimates the same difference in differences, just for banks that smooth dividends:

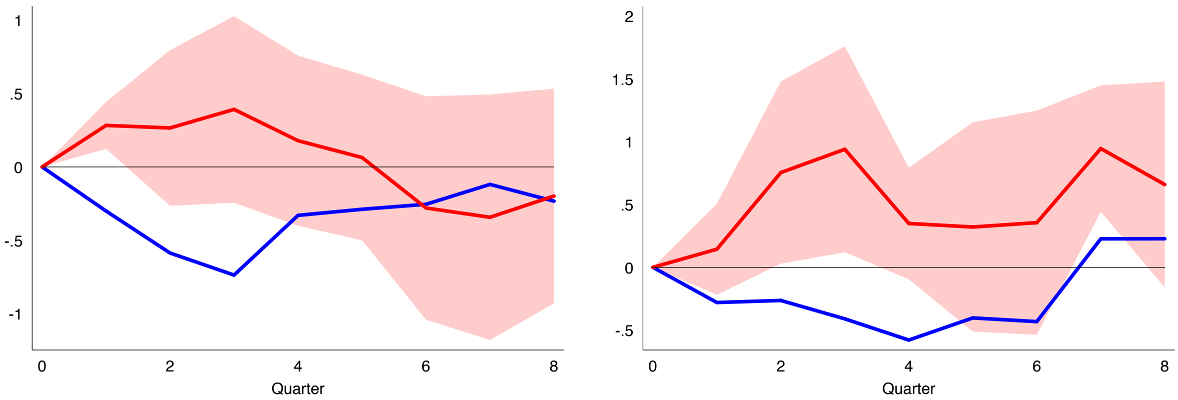
$$\beta_4^h + \beta_5^h = \beta_6^h = \frac{\partial E(Y_{t+h,ic}|MDA\ Concern_{ic} = 1)}{\partial \Delta REQ_{t-1,i} \partial Post2016_t} \quad (3.9)$$

We would expect dividend-smoothing banks to react more after 2016, as MDA restrictions come into place. Estimates for β_6^h should therefore be positive and significant, or at least larger than estimates for β_5^h. We plot coefficients β_5^h (blue) and β_6^h (red) in Figure 3.3. As before, Panel (a) and (b) report results, respectively, without controls and with controls. For dividend-smoothing banks (red), the response of CET1 ratio to a shock in CET1 requirements

[37]The standard errors offer a slightly different output than the comparison of confidence bands for β_3^h and estimates of β_2^h. When we look at the coefficients β_1^h, we have statistical significance for 6 quarters out of 8 - rather than 5 - for the regressions with no controls, and for 3 quarter out of 8 - rather than 1 out of 8 - for the regressions with controls. Coefficients of Table 3.B.1 are plotted in panel (a) of Figure 3.C.1.

is larger after 2016. This is not the case for other banks, which actually appear to react less to a shock in capital requirements after 2016. The confidence band also tells us that this difference in differences between the two groups of banks is statistically significant in the first 4 quarters ahead of the shock.

Figure 3.3: Shock in Requirements and CET1 Ratio (Before and After 2016)



(a) No Controls (b) With Controls

Notes: Figure 3.3 reports results obtained with local projections with a sample over 2013-2019. The lines are post-2016 differences in responses of CET1 ratios 8 quarters ahead of a one percentage-point shock in CET1 requirements. Panel (a) and (b) report results for regressions, respectively, without controls and with controls. Controls include both cross-sectional variables interacted with shocks in CET1 requirements and bi-dimensional balance-sheet variables. Red and blue lines are the responses for, respectively, banks that paid dividends with negative profits at least once over 2000-2016, i.e. dividend-smoothing banks, and banks that did not - namely concerned and not concerned about MDA restrictions. The red shaded area is the 90-% confidence band for the responses of dividend-smoothing banks. The difference in differences for responses between the two groups of banks is statistically significant when the confidence band for dividend-smoothing banks does not cross the response of other banks.

In panel (b) we estimate the same coefficients by including interactions with cross-sectional controls and bi-dimensional balance-sheet controls. Results are slightly more remarked than without controls.[38] We report estimates of coefficients β_4^h and β_5^h for specifications without and with controls in, respectively, Tables 3.B.3 and 3.B.4 in Appendix 3.B.[39] For example, estimates for coefficients β_4^h in Table 3.B.4 are positive and statistically significant in 5 quarters out of 8, which reflect the findings obtained comparing coefficients β_5^h and β_6^h in Panel (b) of Figure 3.3. Overall, estimates with both post-2016 (double interactions) and full sample (triple interactions) suggest that, following a shock in capital requirements - and therefore in the MDA trigger point -, dividend-smoothing banks increase CET1 ratios more. This difference in cumulative responses generally becomes statistically insignificant within 8 quarters after the shock.[40]

[38] The post-2016 difference in reactions of CET1 ratios for banks that smooth dividends is positive and statistically significant in 3 quarters out of 8. In addition

[39] β_5^h coefficients in Table 3.B.3 are plotted in Figure 3.C.1 in Appendix 3.B.

[40] Apart from estimates in the post-2016 sample without controls, which are still statistically significant

In Appendix 3.C, Figures 3.C.2, 3.C.3 and 3.C.4 report responses when we use dummies for, respectively, low volatility of dividends, absence of negative surprises on dividend payments and dividend target. Panels are divided by the set of controls and type of model used - i.e. either the model with simple interactions over 2016-2019 or the model with triple interactions over 2013-2019. Results are mixed. Specifically, triple-interaction models with the dummy on low volatility of dividends produce contradicting results, as banks with larger volatility of dividends seem to increase CET1 ratios more after a capital shock. On the other hand, results with dummies measuring the presence of either negative surprises on dividends or dividend targets are generally not statistically significant.

3.7 Lending

We now move on to considering the lending mechanism of our channel, that is whether, in response to MDA restrictions, banks cut lending in order to increase their risk-weighted capital ratios. There are two main ways banks can do so, namely decreasing lending all together, thus reducing the size of assets (deleveraging), and substituting high-risk lending with low-risk lending (de-risking).

To assess whether MDA restrictions have an unintended negative effect on lending, we start by estimating the DID regression in Equation 3.1 with lending as our dependent variable of interest. Specifically, we consider the quarter-on-quarter log difference of lending stocks in the balance-sheet data of Capital IQ as a measure of new lending (flow). A negative and significant DID coefficient would suggest that, after 2016, dividend-smoothing banks have cut lending more than other banks, thus supporting the hypothesis of deleveraging induced by MDA restrictions.

Furthermore, as a second dependent variable of interest we consider changes in stocks of commercial loans, which are short-term loans to corporations. Basel III assigns generally higher risk weights to corporate loans (Fatouh et al., 2019). By the logic of de-risking, banks that are concerned about MDA restrictions could increase CET1 ratios by cutting corporate loans, which have large risk weights. With flows of corporate loans as the dependent variable, a negative and significant DID coefficient would provide evidence in favour of this hypothesis.

Columns 1-2 and 3-4 of Table 3.4 report the regression results for, respectively, total and commercial lending. In columns 2 and 4, we include interactions between policy dummies and cross-sectional confounding factors, namely a dummy for banks with low levels of capital and a dummy for G-SIB banks. In general, DID coefficients for dividend-smoothing banks are not statistically significant at conventional standards. In addition, differently from the CET1 regressions, none of either the cross-sectional or balance-sheet controls seem to explain changes in lending. Also, the sample size is quite reduced compared to the baseline table for

after 8 quarters.

Table 3.4: Lending DID

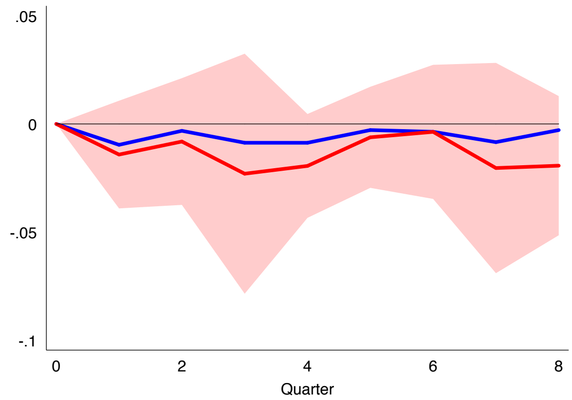
VARIABLES	(1) Δlog(L)	(2) Δlog(L)	(3) Δlog(CL)	(4) Δlog(CL)
Post2016 × Smoothing	0.002	0.000	-0.029	-0.128
	(0.006)	(0.012)	(0.057)	(0.140)
Post2016 × Low Cap. Surplus		-0.010		0.081
		(0.010)		(0.142)
Post2016 × G-SIB		0.024		0.188
		(0.025)		(0.131)
CET1 Req.	0.001	0.000	0.006	0.017
	(0.002)	(0.002)	(0.010)	(0.020)
Profits	-0.005	-0.002	-0.075	-0.064
	(0.037)	(0.036)	(0.115)	(0.112)
Deposits	0.000	0.000	0.006	0.006
	(0.001)	(0.001)	(0.006)	(0.006)
ln(Assets)	-0.062	-0.076	-0.630	-0.775
	(0.058)	(0.055)	(0.458)	(0.525)
Market Cap.	-0.000	-0.001	0.019	0.012
	(0.003)	(0.003)	(0.022)	(0.019)
Subord. Debt	0.001	-0.001	-0.055	-0.096
	(0.007)	(0.007)	(0.087)	(0.128)
Short Debt	0.002	0.002*	0.024	0.026
	(0.001)	(0.001)	(0.017)	(0.018)
Observations	591	561	580	550
Adjusted R-squared	0.439	0.441	0.140	0.151
FE	i-ct	i-ct	i-ct	i-ct

Notes: Table 3.2 reports the regressions for a difference-in-differences analysis on lending. The dependent variable is new loans in columns 1 and 2, and new commercial loans in columns 3 and 4. Post2016 is a dummy which equals 0 and 1 for quarters, respectively, before and after 2016. Smoothing is a dummy which equals 1 for banks that never paid dividends with negative profits over 2000-2016, and 0 otherwise. Low Cap. Surplus is a dummy which equals 1 for banks with pre-2016 levels of regulatory CET1 capital below median, and 0 for banks above median. G-SIB is a dummy which equals 1 if a bank is defined as Global Systemically Important Bank, and 0 otherwise. CET1 Requirement is the requirement for CET1 capital, expressed as a percentage of (RWA). Profits and Deposits are, respectively, total profits and deposits (stocks) expressed as percentages of total assets. Assets is the natural logarithm of total assets. Market Cap. is the market capitalization, i.e. the aggregate valuation of the bank (share price times number of shares). Sub. Debt and Short Debt are, respectively, subordinated debt and short-term debt expressed as percentage of total assets. FE are the fixed effects included. Standard errors clustered by banks are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

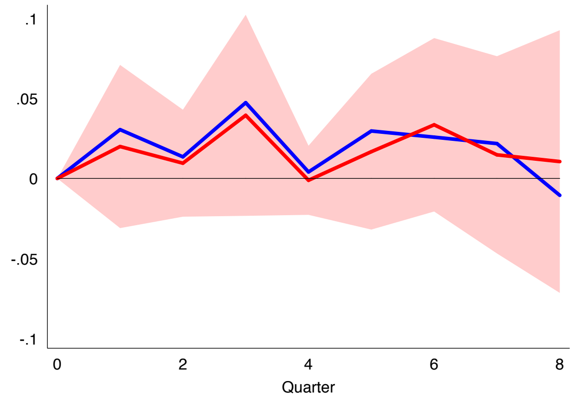
the capital regressions. Finally, the country-quarter and bank fixed effects seem to explain only half of the variation in lending, as adjusted R squared are low for total lending (around .43) and very low for commercial lending (around .15).

We attempt to increase the precision of our estimates by using local-projections models that exploit shocks in capital requirements. We consider the set up introduced in Section 3.6 and we use cumulative changes of new total and commercial lending as the left-hand side. Impulse response functions are reported in Figure 3.4. As before, we differentiate models by specification and type of sample. Panels (a) and (b) refer to the model with simple interactions estimated over 2016-2019, while panels (c) and (d) show the results for models with triple interactions estimated over the full sample 2013-2019. Panels (a) and (c), and (b) and (d), show results when we, respectively, exclude and include controls.

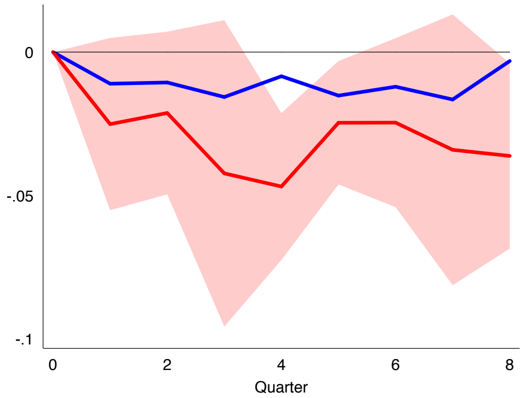
Figure 3.4: Shock in Requirements and Lending



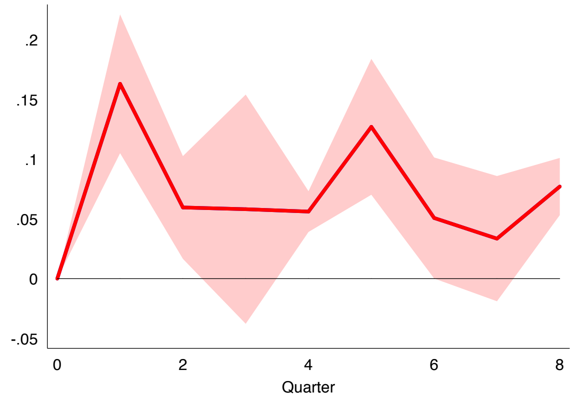
(a) After 2016



(b) After 2016 with Controls



(c) Before and After 2016



(d) Before and After 2016 with Controls

Notes: Figure 3.4 reports responses for total lending obtained with local projections. Panel (a) and (b) show results for the model with simple interactions over 2016-2019, respectively, without controls and with controls. The lines are responses of total new lending 8 quarters after a one percentage-point shock in CET1 requirements. Panel (c) and (d) show results for the model with triple interactions over 2013-2019, respectively, without controls and with controls. The lines are post-2016 differences in responses of total new lending 8 quarters ahead of a one percentage-point shock in CET1 requirements. Controls include both cross-sectional variables interacted with shocks in CET1 requirements and bi-dimensional balance-sheet variables. Red and blue lines are the responses for, respectively, banks that paid dividends with negative profits at least once over 2000-2016 and banks that did not - namely concerned and not concerned about MDA restrictions. The red shaded area is the 90-% confidence band for the responses of dividend-smoothing banks. The responses of the two groups of banks are statistically different from each other when the confidence band for dividend-smoothing banks does not cross the response of other banks.

Overall, responses of lending to a shock in capital requirements do not differ much between banks that smooth dividends and banks that do no. While banks that smooth dividends seem to cut lending slightly more - red line is below the blue line -, this difference is almost never statistically significant at conventional standards. This is the case also when we compare differences before and after 2016, as it is shown in panel (c). In addition, there is not enough variation to estimate triple differences when we include controls. This is why panel (d) reports no difference between groups of banks, i.e. red and blue lines are overlaid.

We estimate the same models for cumulative changes of commercial lending. Results are reported in Figure 3.C.5 in Appendix 3.C. As it is the case for total lending, we find that dividend-smoothing banks do not cut commercial lending more than other banks after a shock in the CET1 requirements (i.e. a shock in the MDA trigger point). Overall, both the DID and LP analyses suggest that MDA restrictions do not induce banks to de-leverage or de-risk by cutting lending.

We reach this conclusion with the caveat that lending data in Capital IQ appear quite constrained, as the sample size is significantly lower than for CET1 ratios. With this caveat in mind, our evidence suggest that MDA restrictions do not raise specific concerns related to credit growth in the current setup. Finally, this evidence, combined with the results presented in the rest of the paper, suggest that banks that are concerned about MDA restrictions adjust their CET1 ratios while keeping lending constant. For example, they could simply increase CET1 capital. However, raising capital is usually more costly than cutting the risk weighted assets. Banks have other ways than lending to cut risk weighted assets. For example, they could sell risky securities, such as corporate bonds with BBB+ rating or lower, which have a risk weight of 100% in the Basel system.

3.8 Conclusions

The regulatory package known as Basel III has introduced a new framework for capital requirements, including for example a set buffer requirements that are intended to address specific risks. Banks that fall below their combined buffer requirements face automatic restrictions on dividend distributions, the so-called MDA restrictions. In this paper, we investigate whether banks increased their risk-based capital ratios in order to avoid being subject to such restrictions. We measure banks' concern about restrictions on dividends with cross-sectional variability in past dividend policies. Intuitively, banks that value a stable and consistent dividend policy would be more concerned about these MDA restrictions than their peers.

With a simple difference-in-differences regression analysis, we find that banks that smooth dividend payments increase their capital ratios by more after 2016. We confirm these results with a local-projection approach that exploits shocks in capital requirements, which imply changes in the threshold at which MDA restrictions apply. The impulse response functions suggest that, after a shock in this threshold, the capital ratio increases more for dividend-smoothing banks, and that this difference generally disappears within 8 quarters after the shock. However, we do not find similar results with alternative measures of banks' concern about dividend restrictions. Our alternative measures consider either the volatility of past dividends, whether banks' dividend distributions were always in line or above analysts' expectations, or whether banks had a public dividend target. Finally, we assess whether these differences can be explained by a cut in lending, as banks can increase their risk-weighted capital ratios by either de-leveraging or de-risking on loans. We find no evidence for this channel.

While the use of MDA restrictions has clear microprudential benefits as it increases banks' resilience and protect debt holders from banks' risk-shifting behaviours in normal times, it may entail unintended private and macroprudential costs. First, the private costs faced by banks may increase in the amount of management buffers they choose to hold, which has implications for funding costs as well as the potential supply of financial services. Second, and relatedly, it may increase procyclicality as banks defend their capital ratios to avoid breaching the MDA threshold. Our results seem to indicate that the introduction of the policy did not have any negative consequences for lending. This is likely because the policy was phased-in gradually, during normal times, allowing banks to build capital organically rather than by deleveraging. However, the incentives to deleverage or derisk are likely be higher during periods of stress, such as the Covid-19 pandemic, when risk-aversion, uncertainty, and market stigma are heightened.

We propose to extend our analysis in several ways. First, we plan to use corporate loan-level data to more clearly identify de-risking or deleveraging by dividend smoothing banks when the MDA policy was introduced. The use of syndicated lending data would allow us to isolate the loan supply response of the bank which is presumably driven by dividend smoothing concerns, while controlling for relationship specific factors using firm-bank fixed effects and firm demand using firm-time fixed effects. Second, we will study how banks with a preference for stable dividend policy have reacted during the Covid-19 crisis. We expect to see stronger deleveraging and/or derisking than in normal times. We plan to use not just loan-level information, but also the fact that some banks out of our sample were subject to distribution bans at the start of the Covid-19 crisis, while others were not. Doing so, we will be able to provide a more complete picture of how the MDA restriction policy works in practice during both normal and stress conditions.

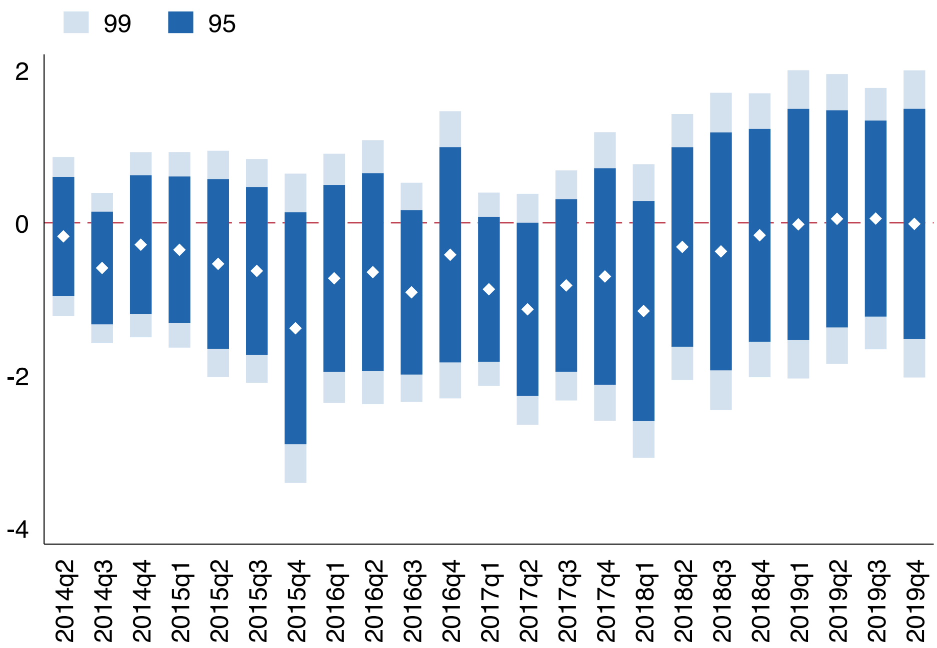
Appendix 3.A DID Analysis - Robustness and Other Measures of MDA concern

Table 3.A.1: Baseline DID - Same N. of Observations Across Columns

VARIABLES	(1) CET1 Ratio	(2) CET1 Ratio	(3) CET1 Ratio	(4) CET1 Ratio	(5) CET1 Ratio	(6) CET1 Ratio	(7) CET1 Ratio	(8) CET1 Ratio
Post2016 × Low Volatility	-0.165				-0.185			
	(0.436)				(0.279)			
Post2016 × No Neg. Surprise		-0.120				0.791		
		(0.850)				(0.572)		
Post2016 × Smoothing			1.476***				0.874**	
			(0.391)				(0.339)	
Post2016 × Target				-0.225				0.262
				(0.455)				(0.471)
Post2016 × Low Cap. Surplus					1.054***	1.097***	0.841**	1.273***
					(0.343)	(0.300)	(0.356)	(0.321)
Post2016 × G-SIB					1.262***	1.386***	1.058***	1.160***
					(0.328)	(0.261)	(0.269)	(0.235)
CET1 Req.	-0.158**	-0.147**	-0.143*	-0.139*	-0.103*	-0.083*	-0.100*	-0.085*
	(0.073)	(0.068)	(0.070)	(0.070)	(0.055)	(0.047)	(0.048)	(0.045)
Profits	0.355	0.377	0.287	0.395	0.511	0.447	0.447	0.494
	(0.372)	(0.357)	(0.371)	(0.378)	(0.362)	(0.380)	(0.352)	(0.354)
Deposits	0.059**	0.059**	0.054***	0.057**	0.055**	0.053**	0.053***	0.055***
	(0.022)	(0.021)	(0.017)	(0.021)	(0.020)	(0.021)	(0.017)	(0.018)
ln(Assets)	0.819	0.632	0.297	0.878	1.508	1.972	0.997	1.428
	(1.731)	(1.993)	(1.525)	(1.815)	(1.266)	(1.406)	(1.406)	(1.398)
Market Cap.	0.117	0.110	0.121	0.120	0.111	0.112	0.111	0.093
	(0.168)	(0.164)	(0.167)	(0.161)	(0.160)	(0.152)	(0.159)	(0.158)
Subord. Debt	-0.070	-0.039	0.101	-0.012	-0.108	0.006	0.019	-0.063
	(0.226)	(0.247)	(0.172)	(0.256)	(0.233)	(0.255)	(0.216)	(0.244)
Short Debt	-0.014	-0.017	-0.031	-0.017	-0.008	-0.011	-0.020	-0.014
	(0.041)	(0.038)	(0.038)	(0.038)	(0.037)	(0.035)	(0.036)	(0.035)
Observations	547	547	547	547	547	547	547	547
Adjusted R-squared	0.910	0.910	0.915	0.910	0.919	0.919	0.920	0.919
FE	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct

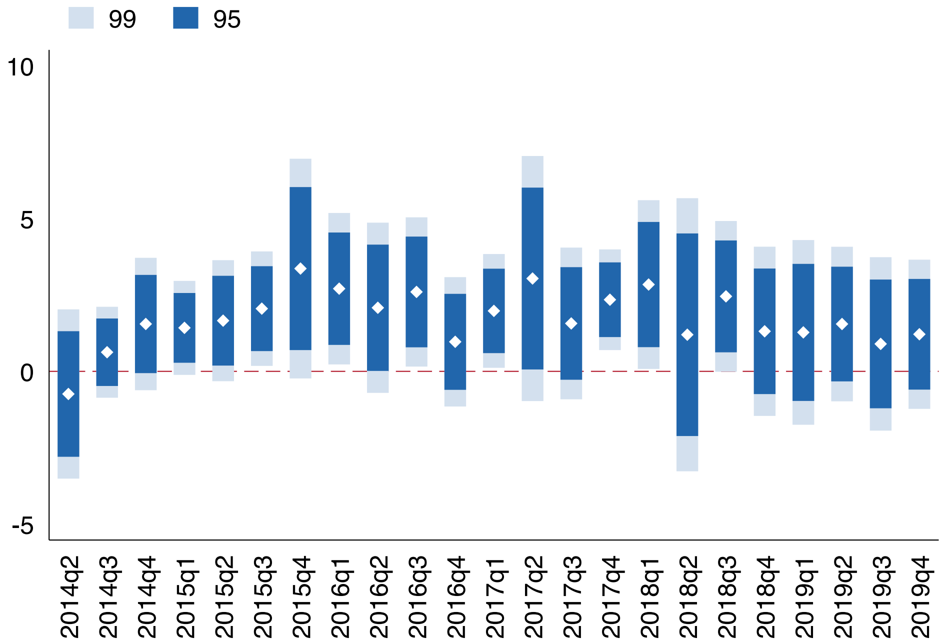
Notes: Table 3.2 reports the baseline results for a difference-in-differences analysis comparing periods before and after 2016. The dependent variable is the CET1 ratio. Post2016 is a dummy which equals 0 and 1 for quarters, respectively, before and after 2016. Low Volatility is a dummy which equals 1 for banks with a pre-2016 average of dividends per share below median, and 0 for banks above median. No Neg. Surprise is a dummy which equals 1 for banks that always distributed dividends in line or above analysts expectations over 2000-2016, and 0 otherwise. Smoothing is a dummy which equals 1 for banks that never paid dividends with negative profits over 2000-2016, and 0 otherwise. Target is a dummy which equals 1 if a bank has mentioned a dividend target in annual reports over 2012-2016, and 0 otherwise. Low Cap. Surplus is a dummy which equals 1 for banks with pre-2016 levels of regulatory CET1 capital below median, and 0 for banks above median. G-SIB is a dummy which equals 1 if a bank is defined as Global Systemically Important Bank, and 0 otherwise. CET1 Requirement is the requirement for CET1 capital, expressed as a percentage of (RWA). Profits and Deposits are, respectively, total profits and deposits (stocks) expressed as percentages of total assets. Assets is the natural logarithm of total assets. Market Cap. is the market capitalization, i.e. the aggregate valuation of the bank (share price times number of shares). Sub. Debt and Short Debt are, respectively, subordinated debt and short-term debt expressed as percentage of total assets. FE are the fixed effects included. Standard errors clustered by banks are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Figure 3.A.1: Impact over Time - Low Volatility



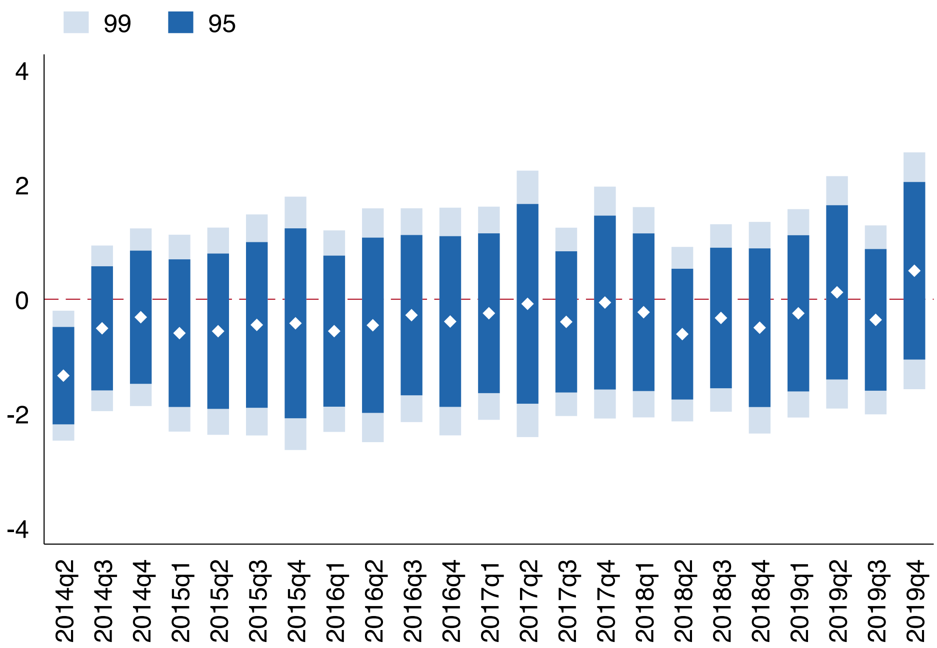
Notes: Figure 3.A.1 reports the coefficient estimates for interactions between the dummy for low dividend volatility and quarterly dummies. The effects are all relative to the first quarter of 2014. Standard errors clustered by banks are used to compute the confidence bands. 95-% confidence bands are reported in dark blue, while 99-% confidence bands are reported in light blue. Results are statistically significant when confidence bands do not cross the zero line (dashed red).

Figure 3.A.2: Impact over Time - No Negative Surprise



Notes: Figure 3.A.2 reports the coefficient estimates for interactions between the dummy for no negative surprise on dividends and quarterly dummies. The effects are all relative to the first quarter of 2014. Standard errors clustered by banks are used to compute the confidence bands. 95-% confidence bands are reported in dark blue, while 99-% confidence bands are reported in light blue. Results are statistically significant when confidence bands do not cross the zero line (dashed red).

Figure 3.A.3: Impact over Time - Target



Notes: Figure 3.A.3 reports the coefficient estimates for interactions between the dummy for dividend target and quarterly dummies. The effects are all relative to the first quarter of 2014. Standard errors clustered by banks are used to compute the confidence bands. 95-% confidence bands are reported in dark blue, while 99-% confidence bands are reported in light blue. Results are statistically significant when confidence bands do not cross the zero line (dashed red).

Appendix 3.B Local Projections - Tables for coefficients on Dividend Smoothing

Table 3.B.1

VARIABLES	(1) h = 1	(2) h = 2	(3) h = 3	(4) h = 4	(5) h = 5	(6) h = 6	(7) h = 7	(8) h = 8
ΔREQ × Smoothing	0.129	0.294**	0.281*	0.552**	0.502***	0.380**	0.216	0.644**
	(0.079)	(0.120)	(0.155)	(0.213)	(0.148)	(0.188)	(0.190)	(0.254)
ΔREQ	-0.001	-0.186**	-0.124	0.010	-0.033	-0.074	0.033	-0.056
	(0.041)	(0.086)	(0.126)	(0.286)	(0.081)	(0.097)	(0.119)	(0.123)
FE	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct
Observations	668	623	579	539	495	450	408	365
Adjusted R-squared	0.416	0.429	0.474	0.520	0.592	0.652	0.709	0.785

Notes: Table 3.B.1 reports coefficient estimates for a local projection model over 2016-2019 without controls. The dependent variable are cumulative changes in CET1 ratios, from 1 to 8 periods ahead. ΔREQ is the shock in CET1 requirements. Smoothing is a dummy which equals 1 for banks that never paid dividends with negative profits over 2000-2016, and 0 otherwise. FE are the fixed effects included. Standard errors clustered by banks are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 3.B.2

VARIABLES	(1) h = 1	(2) h = 2	(3) h = 3	(4) h = 4	(5) h = 5	(6) h = 6	(7) h = 7	(8) h = 8
ΔREQ × Smoothing	0.162	0.203	0.224**	0.394**	0.309	0.399**	0.139	0.561
	(0.145)	(0.154)	(0.103)	(0.159)	(0.205)	(0.169)	(0.199)	(0.359)
ΔREQ × Low Cap. Surplus	0.004	-0.142	-0.392	-0.365*	-0.384*	-0.566**	0.037	-1.168***
	(0.154)	(0.144)	(0.310)	(0.206)	(0.197)	(0.239)	(0.332)	(0.405)
ΔREQ × G-SIB	0.392***	0.284*	0.350	-0.149	0.209	0.028	-0.291	-0.175
	(0.119)	(0.149)	(0.215)	(0.370)	(0.264)	(0.318)	(0.292)	(0.682)
ΔREQ	-0.135	-0.209*	-0.176	-0.348*	-0.202	-0.430*	0.232	0.096
	(0.103)	(0.116)	(0.189)	(0.189)	(0.276)	(0.253)	(0.246)	(0.389)
Profits	-0.336*	-0.659*	-0.200	-0.594*	-0.634**	-0.952*	-1.369***	-1.484***
	(0.179)	(0.346)	(0.462)	(0.296)	(0.281)	(0.506)	(0.379)	(0.486)
Deposits	0.002	0.002	-0.003	-0.014	-0.021	-0.027*	-0.013	-0.008
	(0.006)	(0.008)	(0.015)	(0.015)	(0.014)	(0.016)	(0.017)	(0.017)
ln(Assets)	-0.142	-0.384	-1.262	-1.813	-1.079	-2.997	-3.869	-2.280
	(0.631)	(1.145)	(1.304)	(1.912)	(2.171)	(2.699)	(2.771)	(2.690)
Market Cap.	-0.029	0.001	-0.018	-0.004	0.001	-0.084	-0.120	-0.017
	(0.035)	(0.057)	(0.071)	(0.089)	(0.120)	(0.155)	(0.152)	(0.160)
Subord. Debt	-0.127*	-0.075	-0.047	-0.041	-0.027	-0.134	-0.018	-0.022
	(0.072)	(0.109)	(0.131)	(0.149)	(0.126)	(0.159)	(0.176)	(0.190)
Short Debt	-0.013	-0.010	-0.016	-0.010	-0.051***	-0.009	0.027	0.021
	(0.009)	(0.007)	(0.020)	(0.020)	(0.019)	(0.021)	(0.025)	(0.035)
FE	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct
Observations	486	462	423	395	357	330	297	274
Adjusted R-squared	0.557	0.591	0.609	0.660	0.731	0.749	0.781	0.850

Notes: Table 3.B.2 reports coefficient estimates for a local projection model over 2016-2019 with controls. The dependent variable are cumulative changes in CET1 ratios, from 1 to 8 periods ahead. ΔREQ is the shock in CET1 requirements. Smoothing is a dummy which equals 1 for banks that never paid dividends with negative profits over 2000-2016, and 0 otherwise. Low Cap. Surplus is a dummy which equals 1 for banks with pre-2016 levels of regulatory CET1 capital below median, and 0 for banks above median. G-SIB is a dummy which equals 1 if a bank is defined as Global Systemically Important Bank, and 0 otherwise. CET1 Requirement is the requirement for CET1 capital, expressed as a percentage of (RWA). Profits and Deposits are, respectively, total profits and deposits (stocks) expressed as percentages of total assets. Assets is the natural logarithm of total assets. Market Cap. is the market capitalization, i.e. the aggregate valuation of the bank (share price times number of shares). Sub. Debt and Short Debt are, respectively, subordinated debt and short-term debt expressed as percentage of total assets. FE are the fixed effects included. Standard errors clustered by banks are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 3.B.3

VARIABLES	(1) h = 1	(2) h = 2	(3) h = 3	(4) h = 4	(5) h = 5	(6) h = 6	(7) h = 7	(8) h = 8
Post2016 × ΔREQ × Smoothing	0.583***	0.852**	1.129***	0.509	0.353	-0.025	-0.224	0.035
	(0.151)	(0.330)	(0.362)	(0.318)	(0.351)	(0.470)	(0.533)	(0.451)
Post2016 × Smoothing	0.047	-0.101	-0.080	-0.144	-0.173	-0.250	-0.266	-0.405
	(0.114)	(0.164)	(0.210)	(0.237)	(0.297)	(0.339)	(0.345)	(0.326)
Post2016 × ΔREQ	-0.300***	-0.586***	-0.737***	-0.330	-0.288**	-0.254	-0.119	-0.233
	(0.110)	(0.173)	(0.217)	(0.240)	(0.124)	(0.180)	(0.274)	(0.190)
ΔREQ × Smoothing	-0.475***	-0.595**	-0.912***	-0.053	0.050	0.278	0.296	0.435
	(0.134)	(0.291)	(0.320)	(0.301)	(0.336)	(0.430)	(0.494)	(0.328)
ΔREQ	0.294***	0.398**	0.605***	0.367***	0.244*	0.191	0.124	0.210
	(0.099)	(0.157)	(0.181)	(0.115)	(0.124)	(0.182)	(0.260)	(0.176)
FE	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct
Observations	1,048	1,003	962	926	879	832	791	751
Adjusted R-squared	0.355	0.409	0.444	0.487	0.533	0.567	0.605	0.664

Notes: Table 3.B.3 reports coefficient estimates for a local projection model over 2013-2019 without controls. The dependent variable are cumulative changes in CET1 ratios, from 1 to 8 periods ahead. ΔREQ is the shock in CET1 requirements. Smoothing is a dummy which equals 1 for banks that never paid dividends with negative profits over 2000-2016, and 0 otherwise. Post2016 is a dummy which equals 0 for quarters before 2016 and 1 for quarters after 2016. FE are the fixed effects included. Standard errors clustered by banks are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

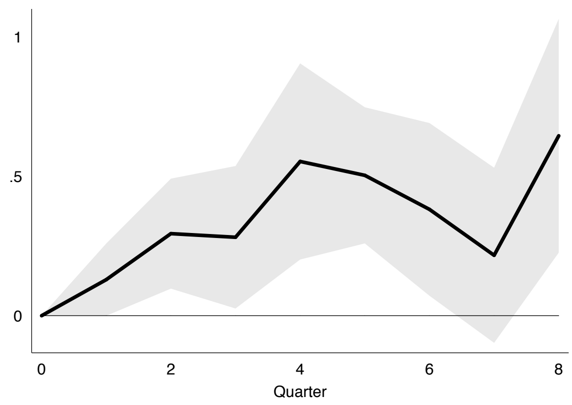
Table 3.B.4

VARIABLES	(1) h = 1	(2) h = 2	(3) h = 3	(4) h = 4	(5) h = 5	(6) h = 6	(7) h = 7	(8) h = 8
Post2016 × Smoothing × ΔREQ	0.424**	1.018**	1.350**	0.928***	0.724	0.788	0.719**	0.431
	(0.206)	(0.445)	(0.500)	(0.230)	(0.461)	(0.476)	(0.270)	(0.337)
Post2016 × G-SIB × ΔREQ	0.403**	1.408***	2.405***	0.013	1.452***	1.787***	0.854	1.217*
	(0.151)	(0.248)	(0.288)	(0.447)	(0.388)	(0.419)	(0.524)	(0.713)
Post2016 × Low Cap. Surplus × ΔREQ	-0.138	-0.408***	-0.641**	-0.327*	-0.127	-0.154	-0.027	-0.515
	(0.159)	(0.132)	(0.287)	(0.178)	(0.262)	(0.343)	(0.482)	(0.530)
Post2016 × Smoothing	0.086	0.146	0.115	0.036	-0.046	-0.194	-0.330	-0.582
	(0.160)	(0.254)	(0.317)	(0.381)	(0.500)	(0.541)	(0.533)	(0.447)
Post2016 × G-SIB	-0.169	-0.165	-0.181	-0.242	-0.360	-0.208	-0.213	-0.263
	(0.135)	(0.268)	(0.369)	(0.382)	(0.466)	(0.474)	(0.580)	(0.593)
Post2016 × Low Cap. Surplus	-0.113	-0.293	-0.243	-0.120	0.029	0.159	0.235	0.385
	(0.154)	(0.273)	(0.299)	(0.321)	(0.342)	(0.419)	(0.435)	(0.495)
Post2016 × ΔREQ	-0.280**	-0.264*	-0.410	-0.579**	-0.403	-0.432	0.227	0.228
	(0.125)	(0.132)	(0.244)	(0.221)	(0.286)	(0.267)	(0.362)	(0.446)
Smoothing × ΔREQ	-0.320*	-0.923**	-1.285***	-0.708***	-0.615	-0.673	-0.836***	-0.255
	(0.178)	(0.373)	(0.445)	(0.226)	(0.472)	(0.433)	(0.270)	(0.225)
G-SIB × ΔREQ	-0.071	-1.254***	-2.274***	-0.404	-1.586***	-2.236***	-1.585***	-2.135***
	(0.111)	(0.185)	(0.192)	(0.258)	(0.352)	(0.350)	(0.399)	(0.399)
Low Cap. Surplus × ΔREQ	0.184***	0.352***	0.379***	0.149**	0.068	0.082	0.184	0.157
	(0.061)	(0.063)	(0.085)	(0.056)	(0.095)	(0.148)	(0.197)	(0.127)
ΔREQ	0.129*	0.090	0.275**	0.248***	0.184	0.106	-0.048	0.066
	(0.074)	(0.086)	(0.114)	(0.068)	(0.126)	(0.156)	(0.169)	(0.117)
Profits	-0.273	-0.527	-0.492	-0.927*	-0.481	-0.760	-0.749	-1.045*
	(0.254)	(0.381)	(0.488)	(0.499)	(0.399)	(0.524)	(0.544)	(0.579)
Deposits	-0.003	-0.018	-0.029	-0.036	-0.043	-0.034	-0.027	-0.015
	(0.011)	(0.023)	(0.028)	(0.030)	(0.034)	(0.032)	(0.035)	(0.033)
ln(Assets)	0.803**	1.053*	1.151	1.273	1.871	1.309	0.937	1.509
	(0.345)	(0.594)	(0.909)	(1.105)	(1.378)	(1.686)	(1.930)	(2.328)
Market Cap.	0.008	0.062	0.065	0.070	0.048	0.003	-0.018	0.047
	(0.031)	(0.045)	(0.068)	(0.097)	(0.133)	(0.164)	(0.189)	(0.204)
Subord. Debt	-0.041	-0.095	-0.115	-0.066	-0.116	-0.099	0.036	0.056
	(0.061)	(0.065)	(0.088)	(0.108)	(0.143)	(0.172)	(0.185)	(0.182)
Short Debt	-0.020	-0.042**	-0.047**	-0.044	-0.064*	-0.046	-0.018	-0.005
	(0.012)	(0.020)	(0.022)	(0.029)	(0.037)	(0.039)	(0.045)	(0.042)
FE	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct	i-ct
Observations	785	761	723	695	657	630	597	574
Adjusted R-squared	0.408	0.480	0.519	0.558	0.600	0.598	0.627	0.681

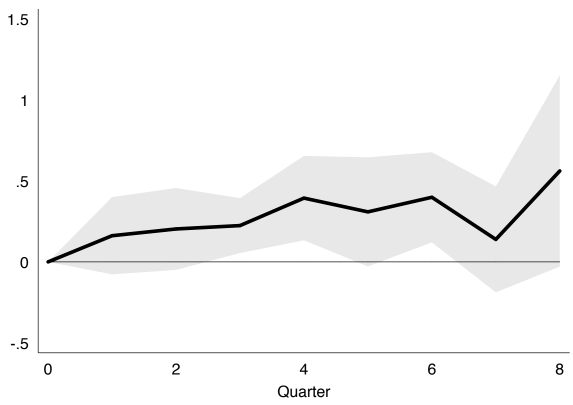
Notes: Table 3.B.4 reports coefficient estimates for a local projection model over 2013-2019 with controls. The dependent variable are cumulative changes in CET1 ratios, from 1 to 8 periods ahead. ΔREQ is the shock in CET1 requirements. Smoothing is a dummy which equals 1 for banks that never paid dividends with negative profits over 2000-2016, and 0 otherwise. Post2016 is a dummy which equals 0 for quarters before 2016 and 1 for quarters after 2016. Low Cap. Surplus is a dummy which equals 1 for banks with pre-2016 levels of regulatory CET1 capital below median, and 0 for banks above median. G-SIB is a dummy which equals 1 if a bank is defined as Global Systemically Important Bank, and 0 otherwise. CET1 Requirement is the requirement for CET1 capital, expressed as a percentage of (RWA). Profits and Deposits are, respectively, total profits and deposits (stocks) expressed as percentages of total assets. Assets is the natural logarithm of total assets. Market Cap. is the market capitalization, i.e. the aggregate valuation of the bank (share price times number of shares). Sub. Debt and Short Debt are, respectively, subordinated debt and short-term debt expressed as percentage of total assets. FE are the fixed effects included. Standard errors clustered by banks are reported in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix 3.C Local Projections - Further Results

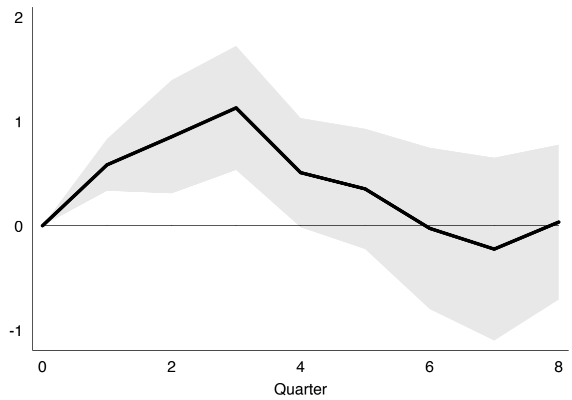
Figure 3.C.1: Shock in Requirements and CET1 Ratio - Simple Interactions



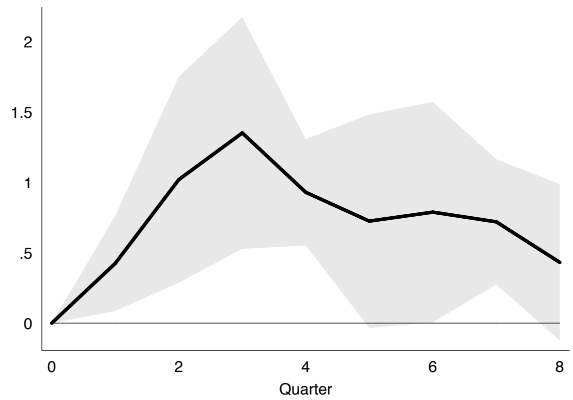
(a) After 2016



(b) After 2016 with Controls



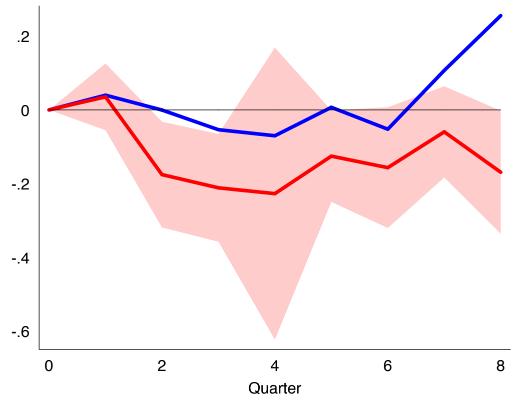
(c) Before and After 2016



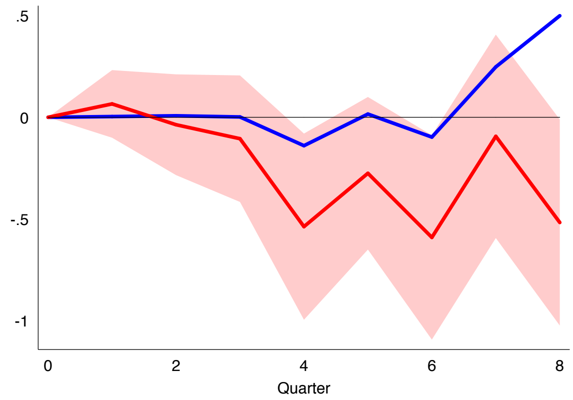
(d) Before and After 2016 with Controls

Notes: Figure 3.C.1 plots differential responses of CET1 ratios to a one percentage-point increase in CET1 requirements. Panel (a) and (b) plot coefficients for $\Delta REQ \times$ Smoothing in Table 3.B.1. They are interpreted as the difference in responses between banks that smooth dividends and banks that do not - namely concerned and not concerned about MDA restrictions. Panel (c) and (d) plot the coefficients for Post2016 $\times$ ΔREQ $\times$ Smoothing in Table 3.B.3. They are interpreted as changes in the coefficients of panel (a) and (b) before and after 2016. Grey-shaded areas are the 90-% confidence bands for these coefficients. The coefficients are statistically different from zero when the confidence bands do not cross the zero line.

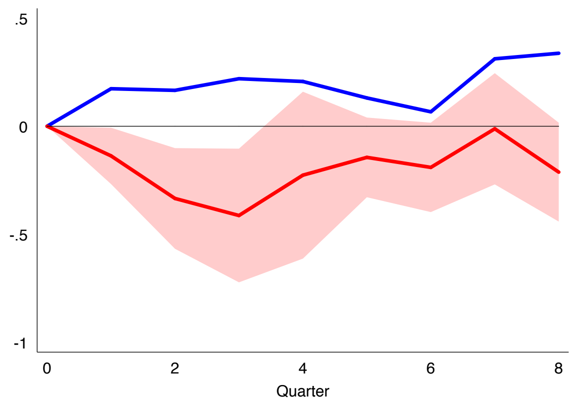
Figure 3.C.2: Shock in Requirements and CET1 Ratio - Low Volatility



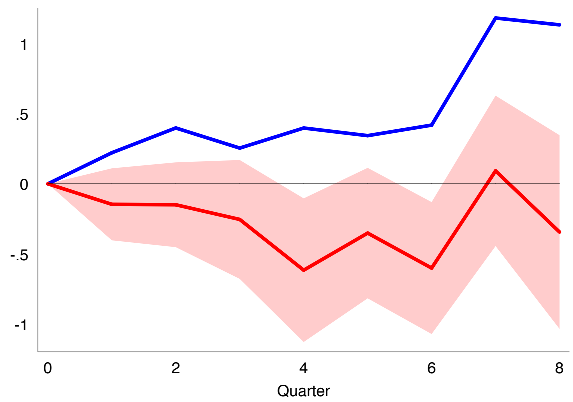
(a) After 2016



(b) After 2016 with Controls



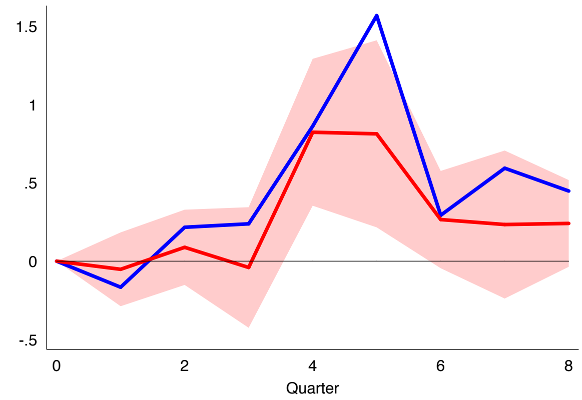
(c) Before and After 2016



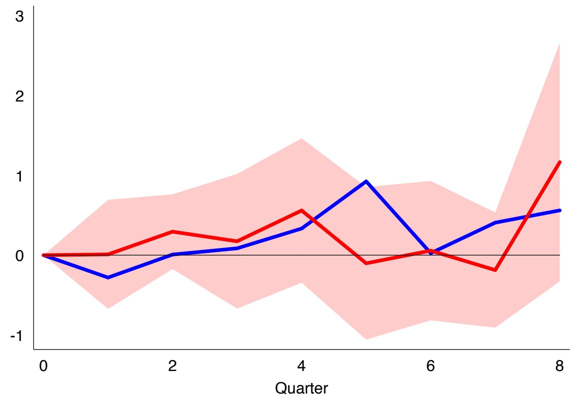
(d) Before and After 2016 with Controls

Notes: Figure 3.4 reports responses for CET1 ratios obtained with local projections and the dummy for low volatility of dividends. Panel (a) and (b) show results for the model with simple interactions over 2016-2019, respectively, without controls and with controls. The lines are responses of total new lending 8 quarters after a one percentage-point shock in CET1 requirements. Panel (c) and (d) show results for the model with triple interactions over 2013-2019, respectively, without controls and with controls. The lines are post-2016 differences in responses of total new lending 8 quarters ahead of a one percentage-point shock in CET1 requirements. Controls include both cross-sectional variables interacted with shocks in CET1 requirements and bi-dimensional balance-sheet variables. Red and blue lines are the responses for, respectively, banks with past average volatility of dividends below and above median - namely concerned and not concerned about MDA restrictions. The red shaded area is the 90-% confidence band for the responses of banks with low volatility of past dividends. The responses of the two groups of banks are statistically different from each other when the confidence band for banks with low dividend volatility does not cross the response of other banks.

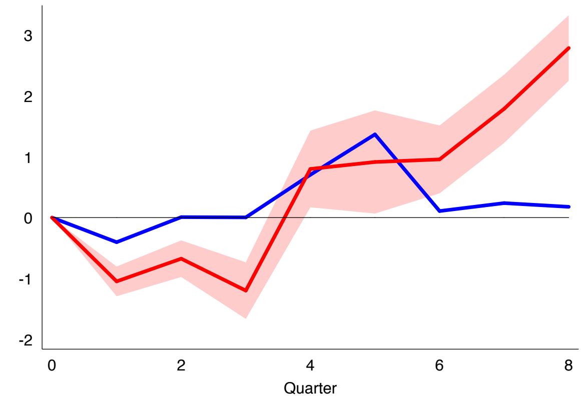
Figure 3.C.3: Shock in Requirements and CET1 Ratio - No Negative Surprise



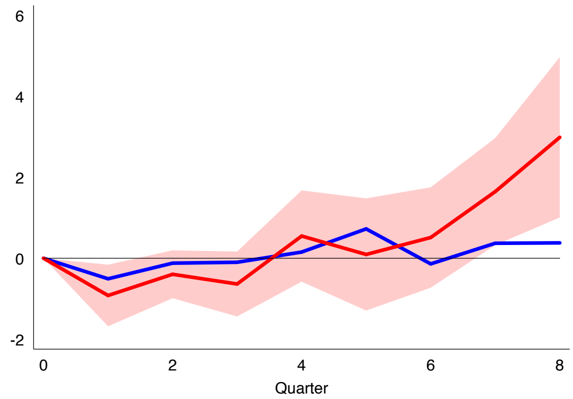
(a) After 2016



(b) After 2016 with Controls



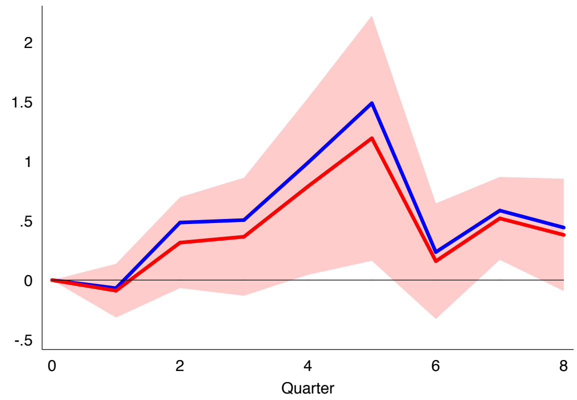
(c) Before and After 2016



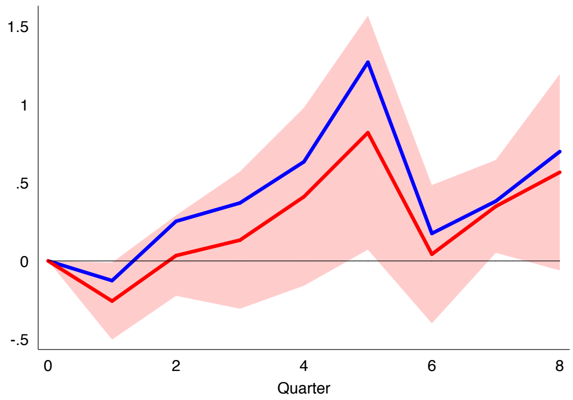
(d) Before and After 2016 with Controls

Notes: Figure 3.4 reports responses for CET1 ratios obtained with local projections and the dummy for no negative surprises on dividends. Panel (a) and (b) show results for the model with simple interactions over 2016-2019, respectively, without controls and with controls. The lines are responses of total new lending 8 quarters after a one percentage-point shock in CET1 requirements. Panel (c) and (d) show results for the model with triple interactions over 2013-2019, respectively, without controls and with controls. The lines are post-2016 differences in responses of total new lending 8 quarters ahead of a one percentage-point shock in CET1 requirements. Controls include both cross-sectional variables interacted with shocks in CET1 requirements and bi-dimensional balance-sheet variables. Red and blue lines are the responses for, respectively, banks that never paid dividends below market expectations over 2000-2016 and banks that did - namely concerned and not concerned about MDA restrictions. The red shaded area is the 90-% confidence band for the responses of banks with no negative surprise on past dividends. The responses of the two groups of banks are statistically different from each other when the confidence band for banks with no negative surprises on dividends does not cross the response of other banks.

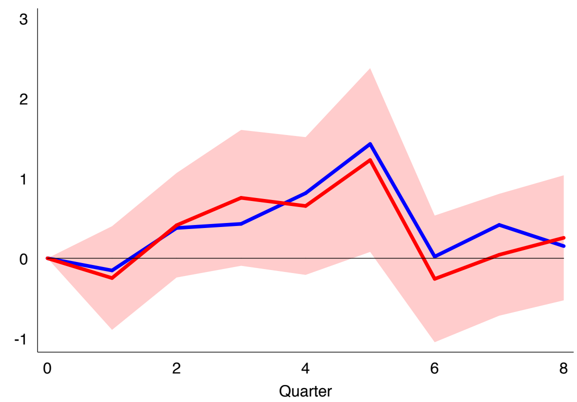
Figure 3.C.4: Shock in Requirements and CET1 Ratio - Target



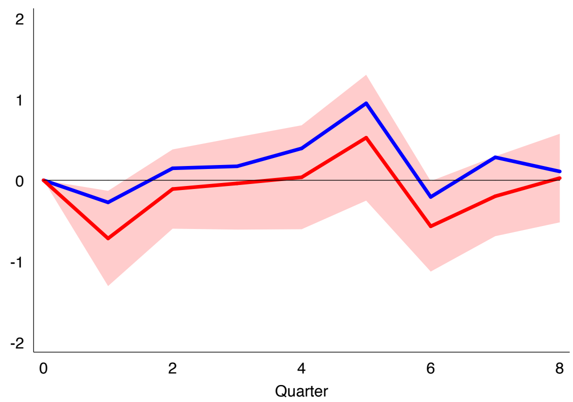
(a) After 2016



(b) After 2016 with Controls



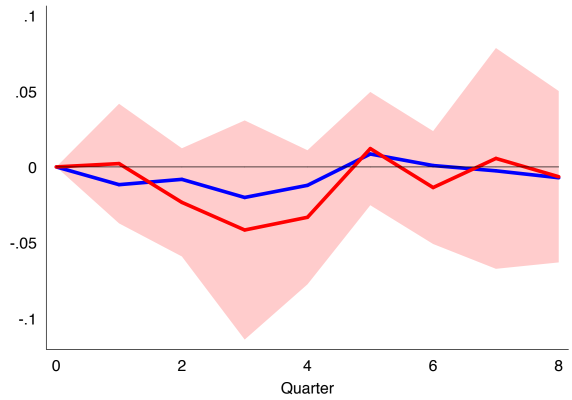
(c) Before and After 2016



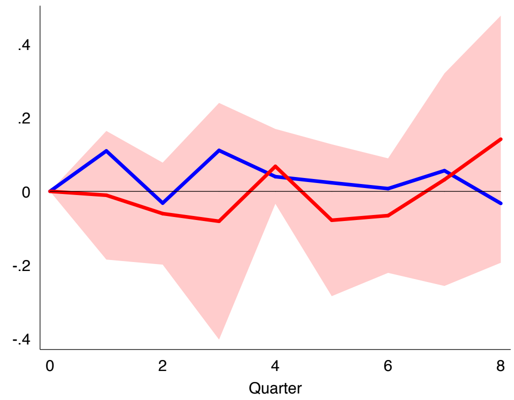
(d) Before and After 2016 with Controls

Notes: Figure 3.4 reports responses for CET1 ratios obtained with local projections and the dummy for dividend target. Panel (a) and (b) show results for the model with simple interactions over 2016-2019, respectively, without controls and with controls. The lines are responses of total new lending 8 quarters after a one percentage-point shock in CET1 requirements. Panel (c) and (d) show results for the model with triple interactions over 2013-2019, respectively, without controls and with controls. The lines are post-2016 differences in responses of total new lending 8 quarters ahead of a one percentage-point shock in CET1 requirements. Controls include both cross-sectional variables interacted with shocks in CET1 requirements and bi-dimensional balance-sheet variables. Red and blue lines are the responses for, respectively, banks which mentioned a dividend target in the annual reports over 2012-2016 and banks that did not - namely concerned and not concerned about MDA restrictions. The red shaded area is the 90-% confidence band for the responses of banks with dividend targets. The responses of the two groups of banks are statistically different from each other when the confidence band banks with dividend targets does not cross the response of other banks.

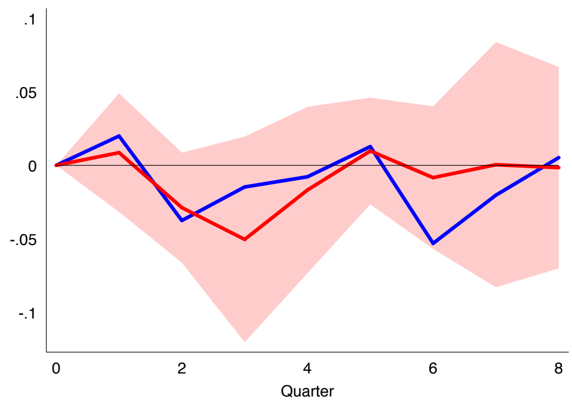
Figure 3.C.5: Shock in Requirements and Commercial Lending



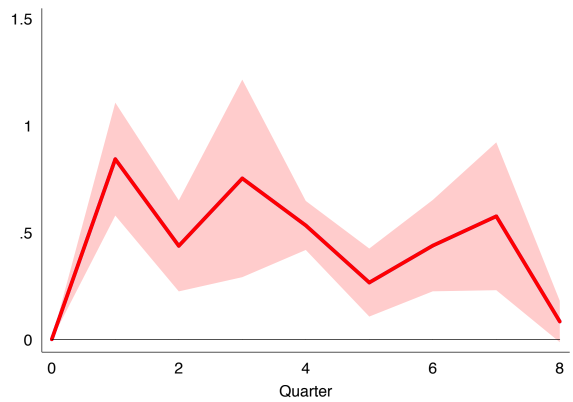
(a) After 2016



(b) After 2016 with Controls



(c) Before and After 2016



(d) Before and After 2016 with Controls

Notes: Figure 3.C.5 reports responses for commercial lending obtained with local projections. Panel (a) and (b) show results for the model with simple interactions over 2016-2019, respectively, without controls and with controls. The lines are responses of new commercial lending 8 quarters after a one percentage-point shock in CET1 requirements. Panel (c) and (d) show results for the model with triple interactions over 2013-2019, respectively, without controls and with controls. The lines are post-2016 differences in responses of new commercial lending 8 quarters ahead of a one percentage-point shock in CET1 requirements. Controls include both cross-sectional variables interacted with shocks in CET1 requirements and bi-dimensional balance-sheet variables. Red and blue lines are the responses for, respectively, banks that paid dividends with negative profits at least once over 2000-2016 and banks that did not - namely concerned and not concerned about MDA restrictions. The red shaded area is the 90-% confidence band for the responses of dividend-smoothing banks. The responses of the two groups of banks are statistically different from each other when the confidence band for dividend-smoothing banks does not cross the response of other banks.

Chapter 4

Hit them in the Wallet! An Analysis of the Indian Demonetization as a Counter-Insurgency Policy

Hit them in the Wallet!
An Analysis of the Indian Demonetization as a Counter-Insurgency Policy*

This paper is co-authored with Nathalie Monnet [⋆].

Abstract

This paper investigates the causal link between the cash nature of the finances of organized armed groups and their subsequent violent activities. We use the sharp cash shortage that followed the 2016 Indian Banknote Demonetization as a natural experiment. The severity of the shortage in different districts is measured using the spatial distribution of demonetized and newly introduced notes. We construct a unique and rich dataset on daily violent events, fatalities and surrenders of the Maoist insurgents in India between 2006 and 2018. Our results suggest that there is a general reduction in violence after the policy in districts experiencing more severe cash shortage, and a positive impact on surrenders of Maoist extremists. Second, we find that the increase in the trend of surrenders is mitigated where Maoists have higher abilities to raise funds, through three traditional sources of revenue, i.e. the extortion of public work contractors, mineral and forest resources. This paper provides the first study on the importance of cash in illegal activities and an ex-post evaluation of a policy countering illicit cash flows.

Keywords: Counterinsurgency, Conflict, Demonetization

JEL classification: C23, D74, Q34, E50

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4.1 Introduction

Conflict and violence cost the global economy $14 trillion a year, accounting for about 13% of the global GDP (United Nations Office for the Coordination of Humanitarian Affairs, 2018). Organized armed groups play a central role in conflicts and are a major obstacle to economic development.[1] Understanding the underlying functioning of armed organizations is a necessity in designing key policies to counter both human and economic losses.

Sustaining violent activities is costly and cash is a first-order element of these illegal activities due to its ease of access and difficulty of traceability.[2] While there exists a wide variety of funding sources for illegal enterprises, such as drugs smuggling, illegal mining and extortion, they are largely based on cash rather than easily traceable means involving banks (Rogoff, 2017). Although there is no reliable data available on the scale and use of cash, for both legal and illegal purposes, irregularities tend to give reason for a large underground economy. Cash transactions, mainly used for low value payments, are estimated to account for one-third of banknotes in circulation, whereas the demand of high denomination notes, such as the EUR 500 note, continues to rise (Europol Financial Intelligence Group, 2015). Despite a global effort to fight all forms of violence, economic research focusing on the financing of criminal and illegal activities remains scarce. This paper asks whether policies targeting the cash fundings of organized armed groups reduce their subsequent violence.

While identifying a causal impact of such policies is not a trivial task, due to the inability to quantify such funding flows, we overcome this challenge by exploiting a unique opportunity to observe the importance of cash, focusing on the 2016 Indian Demonetization as a natural experiment. On November 8, 2016, 86% of the existing circulating banknotes were suddenly and unexpectedly declared worthless by the authorities. This policy was followed by a sharp shortage of cash, affecting the entire population due to both printing press and withdrawal constraints of newly introduced notes. While the demonetization was not directly targeted at a specific armed organization, one of the core objectives stipulated by the Indian government is *to combat corruption and crime*, our focal point in this research paper.[3]

[1] The International Committee of the Red Cross (ICRC) defines armed organizations as "organizations that are party to an armed conflict, but do not answer to, and are not commanded by, one or more states" (International Committee of the Red Cross (ICRC), 2011). An estimation of the number of active armed organized non-state actors is difficult to obtain and depends on its definition. The Armed Conflict Location Events Data accounts for 2,618 groups involved in armed organized violence in 2018 (Raleigh et al., 2010).

[2] Wennmann (2009) gives an overview of the mobilization costs of organized armed groups, i.e. the logistical costs of starting and maintaining combat. The costs to start a conflict are estimated between USD 67,500 and USD 450,000 per thousand soldiers. The maintenance costs are more difficult to evaluate as they are dependent on external factors, such as the intensity and duration of the conflict. A rough estimate is between USD 2-35 million per thousand soldiers per year.

[3] In the press release announcing the demonetization, the Reserve Bank of India (RBI) stipulates that the policy "is necessitated to tackle counterfeiting Indian banknotes, to effectively nullify black money hoarded in cash and curb funding of terrorism with fake notes" (Reserve Bank of India, 2018b). However, later on, other rationale was mentioned such as increasing the tax base and accelerating the financial inclusion by reducing cash transactions and integrating formal and informal economies (Banerjee et al., 2018).

We focus on the Maoist insurgency, a widespread and ongoing conflict which aims to overthrow the Indian Government under a communist ideology. Since 2006, the conflict is responsible for the death of about 8,000 individuals and is located in low-development areas, called the Red Corridor, affecting one-sixth of the overall Indian territory. The Maoists, also called Naxalites, depend on a cash funding system to support their armed fight. They collect rupees through levies on the local economic activities, keeping cash holdings in secret locations in remote forest areas. Their main revenues come from the extortion of mineral and forest resources as well as public work contractors (Ramana, 2018). Following the demonetization, the organization was badly hit. The cash reserves of funding were instantaneously worthless, preventing the procurement of firearms, ammunition, commodities for daily use and the payment of cadres. Maoists have tried to deposit old currency through sympathizers, however, the police enhanced security at banks and other financial establishments, and large bank deposits were scrutinized.[4] Such negative income shock to the organization has led to a large increase in surrenders, as documented in local newspaper. Between the announcement of the policy and the end of November, 469 Maoists have surrendered to the authorities, the highest rate reported in less than a month.[5]

To study the impact of the demonetization on the Maoist conflict, we construct a novel dataset from a variety of sources. We first collect daily observations on the insurgency, including the location, amount and type of incidents, fatalities and surrenders from the *South Asia Terrorism Portal* (SATP), a source of data based on newspapers' clippings since 2006. We complement the violence data with observations from the *Armed Conflict Location and Event* (ACLED), which provides more detailed information on the types of violent incidents related to the conflict. Although the implementation of the demonetization was sudden and unexpected, the timing of the policy simultaneously impacted all districts across India, allowing no change across time. We identify spatial variations by exploiting the geographic distribution of demonetized and new notes, constructed by Chodorow-Reich et al. (2020b). This information allows us to pinpoint the intensity of the cash shortage in each district. We follow a Generalized Difference-in-Differences strategy, allowing us to compare pre- and post-policy trends in violent activities and surrenders, between districts severely and weakly affected by the shock.

We proceed with two sets of results. First, we "zoom out" by focusing on the impact of the adverse income shock on general violence. We find that after the policy, in districts relatively more affected by the cash shortage, the trend in general violent incidents decreased, especially for violent incidents involving the use of capital-intensive means such as explosives. We further uncover a substitution effect of violent activities toward looting, the act of coercively engaging in seizing goods or property other than weapons. This result is in line with an

[4] The Indian Express, 13/11/2016; Times of India, 12/11/2016.
[5] Times of India, 29/11/2016.

appropriation effect, the idea that a relative rise in a contestable good may increase violence by raising gains from expropriation (Dal Bó and Dal Bó, 2011; Dube and Vargas, 2013). Next, we find that, after the policy, the trend in overall fatalities increases in districts severely affected by the demonetization relative to others. However, this increase is entirely driven by an increase in the trend of insurgents' fatalities, as there is no significant impact on civilians and rather a decrease for police forces. We interpret this finding as a preliminary evidence of the *hearts-and-minds channel*, which predicts that a negative income shock to the illegal sector leads to a decrease in violence due to the increased police's ability to repress through local support (Berman et al., 2011). We argue that this is a relevant explanation in our context: there is a reduction of violent attacks toward appropriation and a rise in insurgents' deaths due to potentially improved police targeted operations.

Second, we "zoom in" the organization of the insurgency by concentrating on the act of surrendering. Our results suggest that there is a positive and significant impact of the demonetization on surrenders of Maoist extremists in districts where the currency shock was more severe. This finding sheds light on an *opportunity-cost channel* (Becker, 1968). This theory predicts that as conditions in the legitimate market improve relatively to the illegal occupations, the opportunity cost of engaging in rebellion increases, resulting in a lower number of rebels. In our context, the demonetization provides higher economic incentives for insurgents to surrender post-policy due to the drained cash holdings and the existence of rehabilitation programs in each state, offering various economic benefits such as an access to education, an income and/or an accommodation.[6]

In a subsequent part of the paper, we further investigate the mechanisms at stake, by focusing on the Maoists' traditional means of extortion. When the demonetization hits, rebels had to rebuild their cash reserves, through further extortion of their usual coercion system, namely mineral resources, public work contractors and forest products. We therefore use the spatial variation in the funding system as a potential mitigation effect for their decision to surrender. We find that the rise in the trend of surrenders is alleviated where insurgents have higher abilities to raise new cash through all sources of extortion. This result links the concurrent while contrary effects of the opportunity-cost and appropriation channels. While at the armed organization-level, the Maoist insurgency diverts its usual violent activities towards rent-seeking behavior in order to rebuild its lost finances, at the individual-level, part of the insurgents exits the illegal market for legal occupations, unless there are potentially appropriable resources. Overall, our results highlight the role of economic incentives driving the Maoist Insurgency in the effectiveness of the counter-insurgency policy. All our results

[6] In a recent report, Shapiro et al. (2017) summarize existing counter-insurgency policies against the Maoist insurgency at the State level. Only two States out of the ten Maoist-affected states do not dispose of such policies, namely Madhya Pradesh and Uttar Pradesh. However, they are also the less-affected districts in term of deaths since 2005 (with respectively, 5 and 15 deaths in total). In the other eight States, all programs where put into place before the demonetization. A summary of the implementation dates can be found in Table 4.2.

are stable across a series of robustness checks.

Related Literature. This paper contributes to the emerging literature on the importance of cash in illegal activities and an ex-post evaluation of a policy countering illicit cash flows. A related and contemporaneous research by Limodio (2019) explores the relation between terrorist attacks and their financing through charitable donations in Pakistan. Findings show that the attacks increase with accessible funds, identifying financial constraints. Relying on an analogous credit friction mechanism, we take a complementary approach by focusing the specificities of the cash.

In addition, this work contributes to four distinct strands of the literature. First, we contribute to the academic research evaluating counter-insurgency policies. Despite a global effort to fight all forms of violence, the literature has mainly focused on examining the causes and consequences of conflict (Blattman and Miguel, 2010), while evaluating policies to end it remains scarce. One rare example of such research includes the recent work of Armand et al. (2020), who study the effectiveness of FM radio defection messages on the Lord's Resistance Army insurgency in central Africa. Other studies have evaluated various development programs to improve economic conditions of the local population under the label of counter-insurgency policies, however with mixed evidence. Theoretical frameworks are rooted within the literature on the effect of income shock on conflict. Two prevalent theories predict a decrease in the violence. First, by improving local economic conditions, government programs increase the opportunity cost to fight against authorities, decreasing participation in the insurgency and therefore related violence (Grossman, 1991). Second, development programs might increase citizen support for the government, such that the population is more likely to help authorities to fight against insurgencies through information (Berman et al., 2011). This *hearts-and-minds* channel is tested with the implementation of US reconstruction programs in Iraq, showing a fall in violence against US forces and civilians. Similarly, Crost et al. (2016) finds a fall in conflict-related incidents in the Philippines following a conditional cash transfers program. However, development programs might as well increase violence through strategic retaliatory attacks by insurgents or by creating incentives for resources' appropriation. For instance, empirical evidence shows increased violence after the implementation of infrastructure programs in the Philippines (Crost et al., 2014), rural employment program in India (Khanna and Zimmermann, 2017),[7] US food aid in recipient countries (Nunn and Qian, 2014). Our paper differs from such literature by focusing on a counter-insurgency policy targeting directly the funding structure of conflict, rather than improving local population economic conditions. However, we base our results

[7]However, two similar papers find opposite results, i.e. a reduced violence, using the same rural employment program in India, the National Rural Employment Guarantee Scheme. Fetzer (2019) shows that the program mitigates adverse rainfall shocks by reducing maoist violence, whereas Dasgupta et al. (2017) uses a difference-in-differences approach with multiple local-language press data sources.

on similar mechanisms of transmission, such as detailed in Section 4.3.

In a similar vein, we contribute to the general literature investigating the effect of economic shocks on conflict. Since the pioneer work of Becker (1968), who developed a model where rational agents choose to engage in criminal activities if their expected return exceeds what they can earn from legal activities, a sizable literature has emerged on the relationship between economic resources and violence. If insurgents are not only driven by their ideology and preferences, but also by economic incentives, then there is tradeoff between legal and illegal activities. Research has widely focused on the resource curse, i.e. the role of commodity price shocks as a source of income shock (see for instance, Dube and Vargas, 2013; Berman et al., 2017; Bazzi and Blattman, 2014), however little research has focused on the cash nature of criminal finances.

Third, we contribute to the literature studying the impact of the Indian demonetization, a unique episode in the history of monetary economics. To the best of our knowledge, there exists only a handful of papers analyzing the impact of the demonetization. In a recently published paper, Chodorow-Reich et al. (2020b) provide evidence of the reduced economic activity, using nightlight data and employment surveys. Aggarwal and Narayanan (2017) focus on domestic trade in the agricultural sector, highlighting large drops in commodity prices in the three months following the demonetization. Our paper sheds light on the impact of such policy on the underground economy.

Finally, we contribute to the literature investigating the Maoist Insurgency. Political scientists and historians have mainly focused on the Maoists, including economic descriptive research.[8] However, there is an emerging focus on this conflict, as shown in Shapiro and Vanden Eynde (2020), who examines the relationship between mining activities and Maoists' targeted attacks, or in Vanden Eynde (2018), where the impact of income shocks, through lack of rainfall, depends on the type of targets and the revenue source of the rebels: violence increases against security forces, but only in district with mineral resources. On the other hand, attacks against civilians decreases regardless of the district's profile. Our research provides further evidence on one potential way to fight the Maoist insurgency: by targeting their finances.

The paper is organized as follows. Section 4.2 gives detailed information on the history of the Maoist insurgency, from its roots to its funding structure. Second, it focuses on our negative income shock, the demonetization, where we detail the rules and discuss its exogeneity. The next Section 4.3 draws a conceptual framework based on the theoretical literature linking income shocks and violence. Section 4.4 presents our data including some summary statistics, while section 4.5 discusses our identifying assumptions and exhibits our baseline results. Section 4.6 deepens the research question by considering mitigation effects of the policy. In

[8] See for instance: Miklian (2012) on weak institutions, Ramana (2018) on the details of the finances, Dubey (2013) on the history, Ghatak and Eynde (2017) and Gomes (2015) on the economic determinants.

Section 4.7, we compute various sensitivity analysis on our baseline results. Finally, Section 4.8 offers some concluding remarks.

4.2 Background

4.2.1 The Maoist Insurgency

Responsible for decades of violence throughout India, the Maoist insurgency, in reference of the communist ideology of the Chinese revolutionary leader Mao Zedong, is an ongoing long-term and low-intensity armed conflict between Maoist organizations (also known as Naxalites) and the Government of India.[9] It originated in 1967 in a remote village called Naxalbari, located in the eastern state of West Bengal, as a land dispute between local landlords and tribal farmers. The peasant uprising quickly gained support and spread across several states of India, so-called the Red Corridor, with the common ideology to fight against the Indian government, adopting violence and terror as their core instruments. For the first 30 years, the movement was characterized by a period of fracture and disorganization, with high level of internal conflict among various disparate sub-factions. However, in 2004, the two major organizations, the Maoist Communist Center (MCC) and the People's War Group (PWG), joined forces to form the largest operating faction, the Communist Party of India (Maoist).[10] The resulting exacerbation of violence alerted authorities, who regards the organization as a terrorist group referred as Left-Wing Extremism, and intensified direct confrontations between the insurgents and police forces.

Taking into account the features of the insurgency and the restricted amount of information disclosed by authorities, the intensity of the violence and the strength of the movement is difficult to quantify. Between 2006 and 2018, the conflict has caused the death of at least 8,000 individuals (see table 4.2 for the conflict-affected states) and the displacement of hundreds of thousands of people.[11] The armed wing of the insurgency, the People's Liberation Guerrilla Army (PLGA), is estimated to account for 20,000 fighters, constituting about twice the size of the FARC in Colombia (Gomes, 2015). The geographical spread of the conflict has greatly fluctuated over the past decade. In 2008, 223 districts across 20 states were under Maoist violence, whereas, in 2015, it decreased to 106 across 10 states. Following a newest expansion of the insurgency, it rose to 126 in 2017. In a recent report

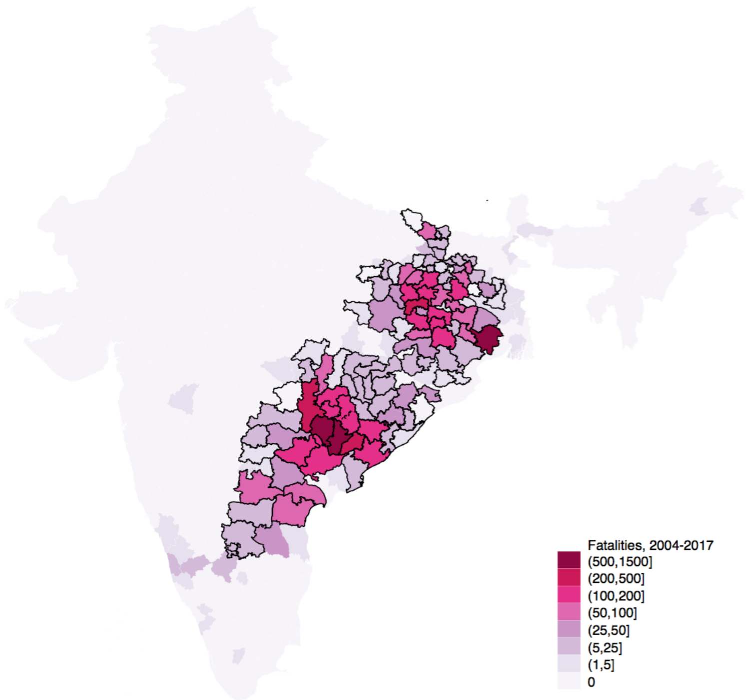
[9] The term *Naxalites* is derived from the place of origin of the insurgency, Naxalbari, while the term *Maoists* originates from the communist claims of the movement. We use both terms interchangeably.

[10] The MCC was operating in the eastern state of Bihar, while the PWG, created in 1976, was engaged in Andhra Pradesh. The newly-formed CPI (Maoist) is responsible for more than 80% of the violent incidents caused by left-wing extremists (Ministry of Home Affairs, 2015).

[11] There exists no legal framework in India to measure the extent of the affected population. Figures vary between 560,000 and 863,900 internally displaced people in the year 2015, for The Norwegian Refugee Council and the Internal Displacement Monitoring Center, respectively. The Guardian, 11/08/2016.

by the Ministry of Home Affairs, 44 districts of the 126 were removed from the list due to sparse violence. Eight new districts were added.[12] The lasting attractiveness and influence of the armed insurgency is rooted in lingering underdevelopment and poverty of the affected areas. Figure 4.1 gives information on the location of the Maoist-affected districts which are surrounded by a dark border. Moreover, it shows the extent and heterogeneous magnitudes of Maoist-related fatalities since 2004.

Figure 4.1: Fatalities in conflict-affected districts, 2004-2017



Note: This map shows the geographic distribution and magnitude of Maoist-related fatalities between 2004-2017. The dark borders display the 102 Maoist-affected districts in 2015, following the Ministry of Home Affairs list.

In order to study the impact of a policy countering the cash funding on the Maoist conflict, we are interested in knowing though which means the insurgency is funded. While this is a difficult task due to the illegal nature of such activities, the literature has found evidence of a close link between the Maoist movement and three main sources of income.

Maoists dispose of a centralized finance system, which follows their hierarchical organizational structure and allows them to reallocate their funds across conflict areas. The governing body at the country level, called Central Committee, draws the main guidelines for the collection and expenditure of funds. The lower-level committees, which are - from top to bottom - State, Regional, Zonal, Area and Village Committees - implement these guidelines (Dubey, 2013). In principle, collection and expenditures of funds are managed at the level of Zonal

[12] Note that the boundaries of districts and states have greatly varied over the past twenty years in India: from 593 districts in 2001 to 712 districts in 2018, with the creation of a new state in 2014, Telangana, carved out of Andhra Pradesh. We restrict our analysis to the all districts in the 10 affected states in 2015. See table 4.2 for a complete list.

Committees, and then the Central and State Committees take care of reallocating excess funds where needed.

Maoists' principal source of income comes from money extortion in three main economic sectors: mining, public works and tendu-leaf production. Maoists have a strong presence in mineral-rich states like Jharkhand and Chhattisgarh, where they extort mining money mainly by imposing levies on both private companies and public contractors (Miklian, 2012). They are capable of coercion on both from legal and illegal mining, particularly coal, iron and bauxite (Shapiro and Vanden Eynde, 2020). Maoists also extort money from public work contractors. In this case, levies are lower when public funds are used for schools and drinking-water supplies, while they are higher for works on exploration of minerals and transportation infrastructure. Finally, the oldest source of funding for Maoists is the extortion of forest resources and subsistence agriculture, more specifically on the tendu leaf industry. In India, tendu leaves are used to wrap beedi, the most common Indian cigarette (Lal, 2009). While money extorted from either mineral resources or public work contractors can be used by Zonal Committees to cover their budget needs, cash extorted from tendu leaves supposedly goes directly to the Central Committee. By the present guidelines, each year Zonal Committees are required to collect around three times their annual budget and store as reserves what they do not use. These reserves can be used either by platoon commanders for immediate war needs or by the Central Committee for reallocation. Committees then allocate the extorted money to finance all Maoists' activities. First, committees allocate funds to meet war needs, thus buying weapons and military supplies, such as uniforms, communications equipment, medicines and meals for the army. Second, Maoists use their funds to disseminate their ideology by financing meetings, classes and propaganda. Third, committees allocate their budget money for indirect support to these activities, for example by providing financial assistance to the families of the cadres or legal aid to functionaries arrested by security forces (Ramana, 2018; Mahadevan, 2012).

4.2.2 The 2016 Indian Demonetization

The Indian economy relies heavily on cash. Up to 2016, around 68% of transactions were made in cash and 86% of the currency in circulation was in form of Rs. 500 and Rs. 1000 banknotes, the two largest denomination in the economy (Ghosh, 2017; Chodorow-Reich et al., 2020b). On November 8, 2016, after the closure of commercial banks, the Prime Minister of India announced that, from midnight onwards, these banknotes were no longer legal tender. The rationale behind this sudden and unexpected policy announcement was threefold: (1) repress the shadow economy; (2) curb corruption and (3) suppress illicit and counterfeit notes. The main guidelines for transition toward this new-denomination system were briefly explained. New Rs. 500 and Rs. 2000 banknotes were introduced in replacement.

Individuals could deposit old banknotes into bank accounts until December 30. However, they could neither withdraw newly-issued notes or exchange all their demonetized notes back at once, due to heavy constraints on withdrawals. These early limits were necessary since, in order to maintain secrecy, the government started to print and distribute the new notes only just after the announcement of the policy. All these limits on cash withdrawals were progressively relaxed and eventually abolished on January 30.[13] As a result of the policy, the vast majority of the old notes (around 97%) were deposited back into the banking system by the end of the year (Karthikeyan and Thomas, 2017). However, the withdrawal and printing press constraints led to a huge cash shortage. On the day of the announcement, total currency in circulation dropped by 75% overnight and recovered only slowly over the following months (Chodorow-Reich et al., 2020b). Concurrently, the economic sectors that rely heavily on cash registered significant economic losses. For example, daily trade in domestic agricultural markets declined by over 15% after demonetization and recovered only partially in the following ninety days (Aggarwal and Narayanan, 2017). Economic losses occurred also in the sectors of construction, local transport, community services, e-commerce, steel, refinery products, telecom and automobile (Ghosh, 2017; Karthikeyan and Thomas, 2017; Singh and Singh, 2016).[14]

Following the demonetization, the Maoist insurgency was badly hit, as their activity relies heavily on the availability of their funds. Maoists' Zonal Committees aim to collect funding of at least three times their annual budget, which is estimated to be around Rs. 4.2 billion ($60 million) per year, while their cash reserves around a few dozen (if not hundreds) million dollars (Ramana, 2018). However, when the policy hit, their reserves in old-denomination currency became instantaneously worthless. As a result, Maoists experienced a significant fund shortage, i.e. a negative income shock relative to the legal sector. This is due the fact that insurgents were prevented to use the legal banking system to exchange their finances. Commercial banks were under the scrutiny of police forces who strengthened security. For instance, on November 21, Press Trust of India (PTI) reports

> Three Communist Party of India - Maoist (CPI-Maoist) cadres, [...], were arrested [...] on the way to a bank for exchanging/depositing extortion money in Latehar District.

If, on the other hand, insurgents succeeded in deposing their money at the local banks, their

[13] Initially, over-the-counter exchanges of old notes was restricted to Rs. 4,000 per person and per day. Cash withdrawals from bank accounts were restrained to Rs. 10,000 per day and Rs. 20,000 per week. Finally, withdrawals from ATMs were limited to Rs. 2,000 per day and per card. These limitations on withdrawals greatly varied over the first few weeks of the demonetization, with over 50 notifications from the Reserve Bank of India. Banerjee et al. (2018) provide a complete timeline of the various rule changes. In our paper, we are interested in the impact of the relative negative income shock to illegal activities with respect to the legal market. The various changes in the limits of withdrawal should not have had an impact on the illegal sector, which cannot directly exchange their cash reserves through the banking system.

[14] This paragraph draws heavily on the recent work of Beyes and Bhattacharya, 2016; Banerjee et al., 2018; Chodorow-Reich et al., 2020b.

bank account were quickly frozen, as reports the Asian Age,[15]

> The Chhattisgarh Government has ordered to block at least 40 bank accounts in Naxal-hit regions of Bastar District and Rajnandgaon District following suspicious transactions in these accounts in the aftermath of demonetization of two high value currency notes on November 22.

In the next section, we detail, using the existing theoretical literature, the mechanisms through which the negative income shock of the demonetization to the insurgents impacted their violent activities and their organization.

4.3 Conceptual Framework

In this section, we draw on a specific strand of the theoretical literature to provide a conceptual framework for the empirical analyses we undertake in this paper. We focus on the literature that links income shocks and conflict to explain why the demonetization might be effective in influencing insurgents to decrease their level of violence or to surrender.

Following this literature, the policy can be considered as negative income shock on Maoist's cash reserves, i.e. the main nature of their financing. The demonetization is a general adverse income shock, since all individuals in India were affected. However, we argue that the shock is relatively more disruptive for Maoists since they do not have legal means to exchange their old notes.[16] Three major theoretical arguments link income and violence at the local level. In our context, we believe that all these mechanisms are relevant.

The first key economic theoretical framework is the *opportunity-cost channel* (Becker, 1968; Grossman, 1991; Esteban and Ray, 2008; Dal Bó and Dal Bó, 2011). This model considers insurgents as rational agents, who choose between work with a certain wage or insurgency. A positive income shock (in the legitimate economy) improves rebels' outside options, by raising the opportunity cost of insurgency, which in turn renders participation less likely.[17] The direct impact is a reduction in violence through less recruits or less funds. In our context, this is a plausible mechanism. When the policy hit, monetary resources have been slashed, while in the legitimate economy, surrender and rehabilitation programs continue to exist, allowing the insurgents to cease supplying labor to criminal activities and to receive economic allowances. All else equal, if a sufficiently large number of Maoists surrender, we expect violence to decrease under the assumption that conflict is proportional to the amount of time devoted to the insurgency.

[15] Both of these newspaper clippings where collected by the South Asian Terrorism Portal (SATP), our main source of data, which is detailed in Section 4.4.

[16] Note that the policy is not only more harmful for Maoists, but generally for all illegal activities.

[17] Specifically, individuals prefer to work if the legitimate market wage is above their reservation wage, which is dependent on their individual preferences such as commitment to the cause, risk aversion and attitude toward violence.

A second potential impact of such mechanism is that violence is reduced indirectly through a higher willingness of the population to share information with local authorities. This alternative explanation unraveling the link between an income shock and violence is the *hearts-and-minds approach* (Berman et al., 2011).[18] Under the assumption that the main constraint to armed activities is the extent to which the local population reveal information to the authorities, a positive income shock to the legitimate economy triggers a reduction of violence due to the increased state's ability to repress through a higher supply informants. In our case, not only the Maoists are affected by the demonetization, but the entire population. Thus, it is difficult to argue that this type of policy would incentivize the civil population to collaborate with police forces. However, one possibility is that insurgents who surrender become police informants. While we cannot directly test this theory in our empirical exercise, we show preliminary evidence toward such mechanism.

Finally, the *appropriation* channel refers to the idea that violence is directed toward the expropriation of economic rents (Dube and Vargas, 2013). A rise in a disputed income may increase conflict by raising the return to rapacity and promoting appropriation over these resources.[19] The demonetization can be considered as a shock which raises the return to appropriation, since the policy completely depleted the monetary reserves of the insurgency. This implies a rise in violence by increasing targeted operations toward rent-seeking behavior to rebuilt lost finances.

In this paper, we focus on the link of Maoists' finances and their violent activities relying on these existing theories. We shed light on the various implications brought by lost finances for the Maoist insurgency and their underlying theoretical explanations. This paper is a contribution to the nascent literature that tries to tackle the question of illicit cash flows.

4.4 Data & Descriptive Statistics

In this section, we provide a detailed picture of the data exploited in this paper. We construct a daily-district dataset from distinct sources such as administrative information, reports and newspaper articles.

4.4.1 Conflict data

Our main source of conflict data comes from the *South Asia Terrorism Portal* (SATP), which reports Maoists-related incidents from both local and national English-speaking newspapers in India since 2005. Available data include both the location and date of the incident at

[18] This channel is also referred under the name "grievances" in the literature.

[19] This mechanism, also called "rapacity channel", is considered in several models, such as in the "state as a prize" and the rent-seeking literatures (Hirshleifer, 1991; Grossman, 1999; Chassang and Padro-i Miquel, 2009; Garfinkel and Skaperdas, 2007).

various aggregation levels, as well as the number of fatalities (by type: insurgents, civilians and security forces), injuries and surrenders. Using this information, we manually code these details in a daily-district level dataset. In the case of missing information, we further search for the primary source for verification. We use data from March 24, 2006 at the earliest to April 15, 2018, but the timeframe varies depending on the outcome variable.[20] This allows for data before and after the implementation of the policy on November 8, 2016. Using yearly information, Figure 4.2 shows, on the left panel, the descending trend in Maoists' violent activities since 2008, whether it is in terms of violent incidents or subsequent fatalities. It is noticeable that the decreasing trend sharpens around the demonetization (red vertical line). The right panel put into perspective the yearly number of surrenders with cash recovered by police forces between 2012 and 2018. While this figure does not inform on the effectiveness of the demonetization in deterring violence and forcing Maoists to surrender, it shows the importance of cash for the insurgency. Between 2012 and 2018, around Rs. 71 million of cash was recovered by the police forces. For instance, on May 2, 2016, as reported by the Times of India,

> The Special Task Force (STF) [...] arrested four Communist Party of India Maoist (CPI-Maoist) cadres [...]. The Police also seized two rifles, a double barrel gun, a countrymade rifle, 205 pieces of cartridges of 7.62 bore SLR with charge clip, two pressure cooker bombs with explosive materials, six pieces of magazine of SLR rifle, 22 pieces of charger, two detonators, handbills asking people not to participate in panchayat election and INR 250000 in cash.[21]

In our analysis, we base ourself on daily information, in order to tackle the effectiveness of the demonetization policy.

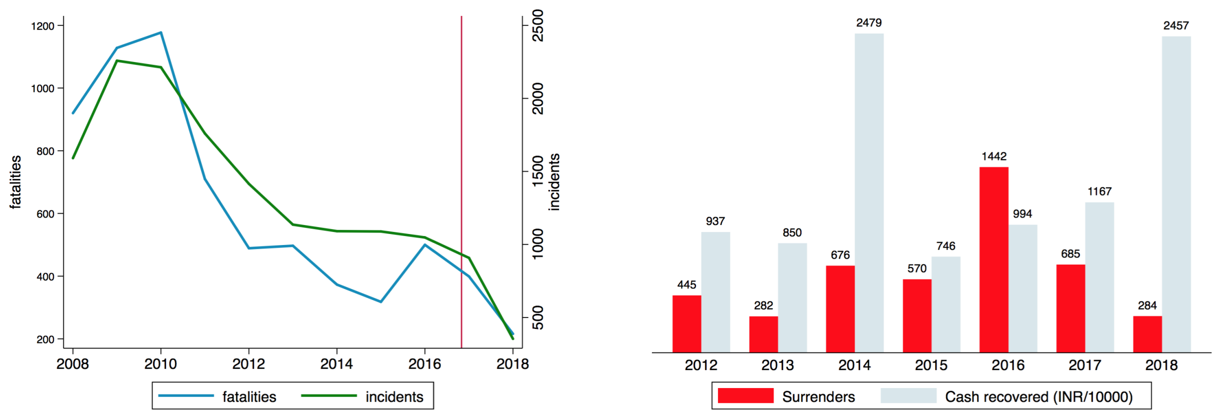
The literature on the Maoist insurgency is mostly based on data from SATP, which represents the most extensive and complete reporting of conflict-related events in South Asia (Fetzer, 2019). However, the accessible information does not disentangle different categories of violent incidents. Thus, we complement our conflict information with two widely used databases. First, the *Armed Conflict Location and Event* (ACLED) provides daily geo-referenced conflict-related events from various sources such as press accounts, humanitarian agencies and research publications (Raleigh et al., 2010).[22] The main advantage of this database is the inclusion of all political violence, disaggregated in six types of events, and 25 sub-event

[20] Our main outcome variables, violent incidents, fatalities and surrenders, differ in terms of initial date of availability. At the daily-district level, major incidents and fatalities are available since 2010, incidents related explosives since 2013, and surrenders are available for the entire period. In our baseline results, we keep a one-year pre/post-policy window to ensure that we have a corresponding timeframe throughout the analysis.

[21] This newspaper clipping is an example of our primary source of information, from the South Asian Terrorism Portal (SATP).

[22] The ACLED database is widely used in the research on conflict. See for instance (Berman et al., 2017).

Figure 4.2: The Maoist Insurgency, 2008-2018



Note: These figures plot the time series of conflict intensity from SATP database. The left panel depicts the descending trend in Maoists' violent activities and subsequent fatalities between 2008 and 2018. The right panel put into perspective the yearly number of surrenders with cash recovered by police forces between 2012 and 2018.

types.[23] However, Maoists' related incidents are only covered between 2016-18. This data allows us to provide further information on the mechanisms of transmission of the cash shortage to the violence.

Second, we make use of the *UCDP Georeferenced Event Dataset*, which records daily events of lethal violence since 1989 (Sundberg and Melander, 2013).[24] The UCDP data, contrary to ACLED data, allows to distinguish the victims between the insurgents, the civilians and the government/police forces. However, there is no details on the types of events. The data is only used for the sensitivity analysis.

4.4.2 Demonetization Shock

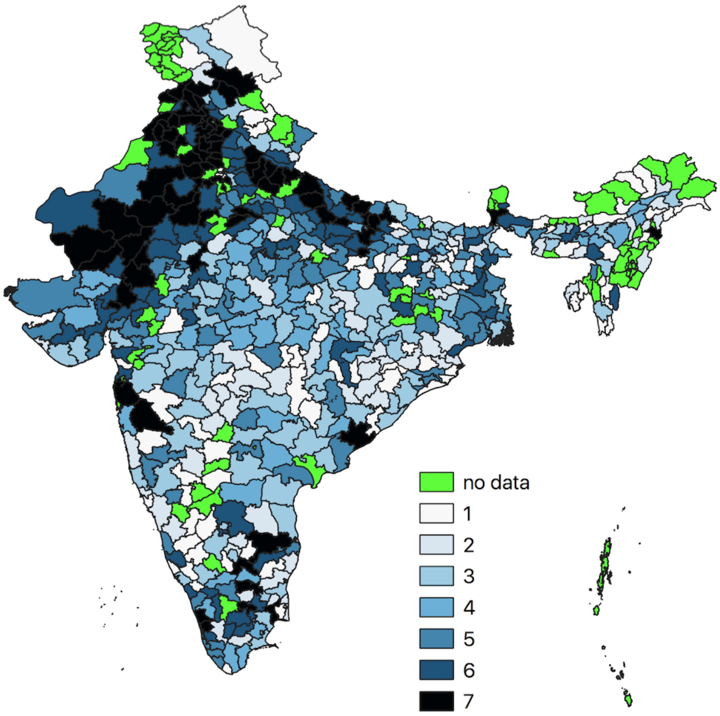
Our analysis is based on the timing of the demonetization, which was implemented for the entire Indian population on November 8, 2016. However, in order to identify the impact of this counter-insurgency policy, we rely on the geographical variation in the intensity of the cash shortage experienced in each different district. As such, we use the *demonetization shock* constructed by Chodorow-Reich et al. (2020b). Using data from the Reserve Bank of India (RBI), they are able to quantify the ratio between the arrival of new notes to the quantity of demonetized notes in each district of India. Their geographic demonetization shock is depicted in Figure 4.3, in which we categorize the intensity of the demonetization on a scale of one to seven, the darkest shade representing a severe cash shortage. Due to the confidentiality of the RBI data, our usage of the shock is limited to a categorical variable,

[23]Event types include battles, explosions/remote violence, violence against civilians, protests, riots, strategic developments. In the sub-event types, we are specifically interested in violence related to Maoists' activities such as armed clash, remote explosive, attack against civilians and looting/property destruction.

[24]UCDP GED Global version 19.1.

representing the net value of new notes received by commercial banks in December 2016 to the total value of demonetized notes until the end of January 2017.

Figure 4.3: Demonetization Shock (Chodorow-Reich et al., 2020b)



Note: This map shows the geographic distribution of the demonetization shock in December 2016 at the district level, constructed by Chodorow-Reich et al. (2020b). Districts with larger shock are shaded darker.

One important aspect to point out is the exogeneity of the distribution of new notes. As thoroughly discussed in their paper, this shock can be seen "as good as random" with respect to local economic conditions. Following two strategies, they show that first, the narrative of the RBI does not support a link between the availability of new notes and local economic conditions. In the first few months of the aftermath of the policy, new notes were send out to commercial banks following a logistical criteria. Moreover, it was impossible for the RBI to precisely know the distribution of old notes before the implementation of such policy. Second, simple correlations between the geographical spread of the demonetization with pre-policy trends in the local economic conditions validate the randomness of the geographical variation in the shock.

Following their intuition, we test whether the demonetization shock varies across districts due to variation in the pre-trend of Maoists' violent activities. For instance, it could be argue that the RBI did not allocate sufficient new notes in the conflict-affected district to additionally jeopardize the insurgents' daily activities. Table 4.1 shows that there is no evidence of pre-trend in the data. We find no correlation between the outcomes and the demonetization shock before November 8, 2016, with one exception. The total number of violent incidents is negatively correlated with the intensity of the policy shock if we look at the overall distribution of the currency shock in India (column 3). There are two reasons why this negative correlation does not drastically affects our results. First, the negative correlation means that conflict-affected districts were on average less affected by the cash shortage, which indicates that our baseline coefficients of interest are a lower bound of the impact of the demonetization. If districts were on average more affected by the policy, they would show a higher degree of influence. Second, in our baseline results, we only focus on conflict-affected districts, which corresponds to column 4. In that case, the geographical distribution of the shock does not correlate with the intensity of the conflict, which spares us from potential endogeneity concerns.

Table 4.1: Geographical distribution in the demonetization shock

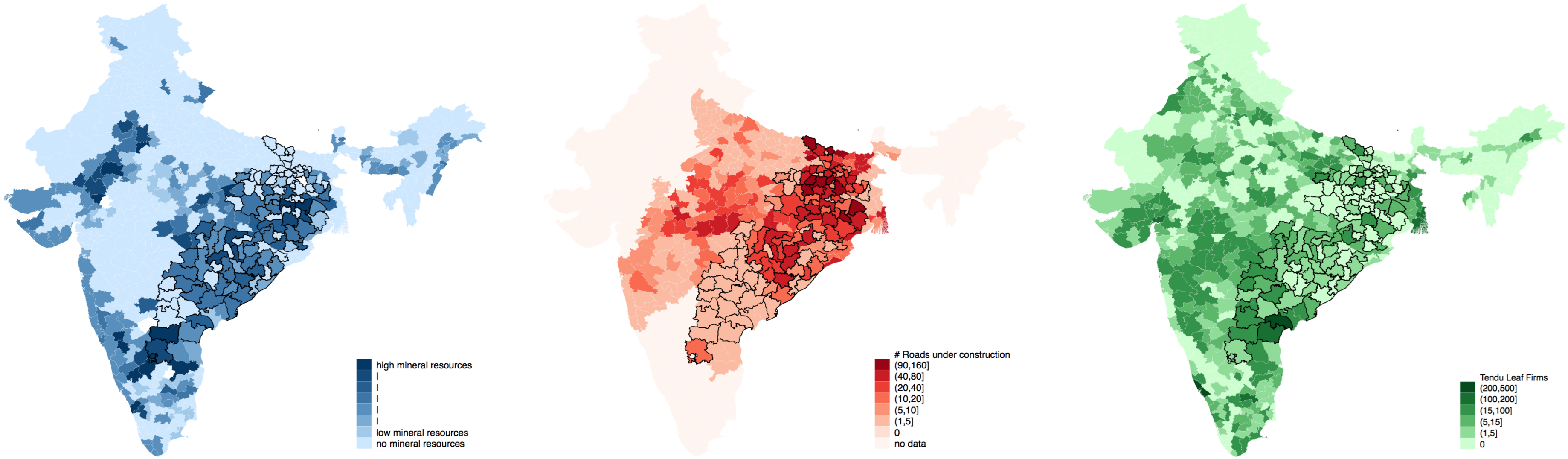
	Demonetization Shock					
	(1)	(2)	(3)	(4)	(5)	(6)
Surrenders	-0.000	0.001				
	(0.001)	(0.001)				
Violent Incidents			-0.076**	-0.020		
			(0.036)	(0.030)		
Fatalities					-0.016	-0.001
					(0.012)	(0.009)
Constant	3.999***	3.047***	4.025***	3.152***	4.010***	3.114***
	(0.090)	(0.173)	(0.090)	(0.180)	(0.090)	(0.175)
Districts	all	conflict-affected	all	conflict-affected	all	conflict-affected
Observations	534	94	534	94	534	94
R-squared	0.000	0.014	0.008	0.005	0.004	0.000

Notes: The table reports the correlation coefficients between the geographical distribution of the demonetization shock as depicted in Figure 4.2.2 and our pre-policy outcome variables, from SATP database. The independent variables represent the total number of surrenders, violent incidents and fatalities from the initial date of observation to the announcement of the policy. Odd columns (1, 3, 5) include all districts in India, while even columns (2, 4, 6) restrict the sample to Maoist-affected districts. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

4.4.3 Maoists' Finances

In a second part of the paper, we focus on the Maoists' abilities to raise new revenues through their usual extortion system on local economic activities. Their main sources of finances are threefold: mineral resources, such as iron, bauxite and coal; public work contractors; and forest products, with a focus on tendu leaves. We collect data from different sources to test whether the impact of the demonetization on insurgents' finances is alleviated when there are fairly accessible resources to extract.

Figure 4.4: Maoists' sources of finances in conflict-affected areas



Note: This figure displays the three main sources of finances by the Maoist insurgency. The left panel maps the distribution of the production value of mineral resources in 2015. The middle panel identifies the number of roads under public work constructions in 2015. The right panel shows the number of firms working in the forest industry between 1998 and 2008. The darker the shade, the higher is the reliance on extortion means. The dark borders display the 102 Maoist-affected districts in 2015, following the Ministry of Home Affairs list.

Mineral Resources.

Data on mineral resources come from three distinct sources. In our main specifications, we make use of data collected from the National Mineral Inventory from the Indian Bureau of Mines, which covers 71 minerals with over 5,500 freehold and 7,500 leasehold deposits. The data include the location (district), reserves and resources for the year 2015. We collect data on the main 24 minerals to identify the value of production of mineral resources in each district.[25] Following the methodology in Berman et al. (2017), we restrict our analysis to the 15 minerals for which we have world price data.[26] Real world prices of the minerals, measured in 2010 USD, come from the World Bank Commodities prices dataset, or from the U.S. Geological Survey if not included in the former. In our specification, we normalize the value of production in each district on a scale from 0 to 1, in order to facilitate the interpretation. Since we are interested in the relative wealth in mineral resources between districts, rather than the exact production value of each single district, this transformation does not affect our estimation. The distribution per district is visible in the left-hand side panel of the maps in Figure 4.4. The dark borders display Maoist-affected districts and the darker the shade means a high production value of mineral resources. While the map shows that there is a large variation in the value of production of diverse mineral resources over the entire country, districts affected by the insurgency tend to by highly correlated with mining activities. In total, 414 districts out of 627 do not produce any minerals, while 59% of the 102 districts affected by the Maoist insurgency rely on mining activities.

Next, we complement our analysis with two other sources of mineral resources data that are employed in the sensitivity analysis. First, we exploit mining leases information from the Indian Bureau of mines, which collects basic data relating to major minerals except coal, petroleum and natural gas.[27] The State Governments are the owners of minerals located within their respective boundaries, and are empowered to grant individuals or companies the rights to extract minerals, in exchange of predetermined compensation, called royalties and set by the Central government.[28] Mining leases, also called mining concessions, are defined as a lease granted for the purpose of undertaking mining operations, such as winning any mineral. As of 2015, 7,664 leases were in force in 23 States. Andhra Pradesh is leading with more than a thousand mining leases, followed by Madhya Pradesh and Telangana. However,

[25] We collect data from the following categories: metallic minerals (ferrous and non-ferrous groups), precious and semi-precious minerals, strategic minerals and coal. We omit fertilizers, refractory minerals, ceramic and glass minerals, other industrial minerals and minor minerals.

[26] Our minerals of interest are: coal, bauxite, iron, copper, lead and zinc, nickel, tin, gold, platinum silver, chromite, manganese, garnet, cobalt, molybdenum. We removed diamond as it is difficult to estimate the price which depends on the quality of the alloy.

[27] Data was provided thanks to Shapiro and Vanden Eynde (2020).

[28] Under the Mines and Minerals Development and Regulation (MMDR) Act 1957, the State Governments may grant reconnaissance permits, prospecting and composite licenses, and mining leases by discretion. The existing MMDR Act was recently amended by the Central Government. Since January 2015, the State Governments grant the mineral concession through auctions, in order to improve transparency.

we do not use this data in our main specifications as they do not account for freehold deposits, neither for coal, which is highly related to Maoists' extortion activities.

Second, we use data on large-scale mines from the Raw Material Data (IntierraRMG, 2013).[29]. The RMD data include worldwide information on the location, production and specific minerals produced by mining companies since 1980 and are focused on large-scale mines, operated by the governments or multinational companies. Small-scale and illegal mines are not covered. However, mines extracting coal are included in the sample, in contrast to the mining leases dataset. We create a measure of large-scale mines, by identifying the number of active mines per district for the year 2012, the latest observed date in the dataset. From Table 4.3, we can see that the number of active large-scale mines in conflict-affected states is much lower than the number of active leases. The distribution is also different: Jharkhand and Orissa show the highest number of large-scale mining companies with 69 each. A striking difference is Andhra Pradesh that displays only 3 large-scale mines, but the highest number of leases with 1293.

Public Works.

To measure the relative dependence on public works at district level, we consider the *Pradhan Mantri Gram Sarak Yojana* (PMGSY) program, which is a centrally-sponsored scheme for the construction of roads and other infrastructure projects, such as bridges, in rural areas.[30] Within this plan, the Ministry of Rural Development allocates funds to state-level agencies, called Executing Agencies, which manage the tendering process to identify contractors. Awarded contractors go through a thorough monitoring procedure and have up to 15 months to achieve their mandate. We concentrate on PMGSY publicly available datasets, specifically on the report *Physical Financial Monitoring*. Among other things, it includes, for each district and year, the project's award date, completion date, status, name of the contractor, company, and working road as well as total expenditure. We use this information to build three district-level measures of public works, from which Maoists could go extort new cash after demonetization: the 2015 average number of daily (1) public-work contractors, (2) roads and (3) expenditure. To mitigate potential endogeneity concerns, such as the fact that the implementation of the policy could have had a direct impact on the allocation of public works, we focus on mandates awarded before the implementation of the policy. The difference between the three measures of public contracts is in terms of magnitude: there is more than one road under construction for each contractor (see table 4.2). Due to the logistical difficulty to collect such data, we restrict our sample to the 10 conflict-affected states. However this does not have any impact on our results, as we only focus on Maoist-affected districts in the baseline estimates. The central panel of Figure 4.4 maps the number of roads under

[29]Data was provided thanks to Berman et al. (2017).

[30]In English, the program means "Prime Minister's Rural Roads Scheme".

construction in 2015, our main variable of interest in Section 4.6. We collect data on 308 districts, out of which 29% do not have any public work in 2015. Similarly to mineral resources, the map shows a high correlation between Maoist-affected regions and the number of roads under construction. This is particularly true for the Northern districts.

Forest Industry.

We collect data for firms working in the tendu leaf industry, one of the main source of extortion for the Maoists, from the *Annual Survey of Industries* (ASI) supplied by the Ministry of Statistics and Programme Implementation, for the years 1998-2010. The ASI data is a representative sample of all registered manufacturing establishments in India. Keeping only firms working in the forest industry, and specifically linked to tendu leaf, bidi or cigarette, we obtain a dataset of 6,413 firms all over India, with 58% located in conflict-affected states (see table 4.2).[31] The right-hand side panel of Figure 4.4 displays the distribution of the number of firms per district. Conflict-affected districts, with their boundaries surrounded in dark lines, tend to be highly represented in the manufacturing industry of forest products, with a large variation between districts.

Table 4.2: Conflict-affected States

	Affected districts	Total surrenders	Total deaths	Deaths Incidence	Mineral resources	Public works roads	Public works contractors	Forest industry firms	Rehabilitation programs
Andhra Pradesh	8/13	781	742	0.88	49,712	32	22	802	1997
Bihar	22/38	193	684	0.66	849,845	1,692	614	102	2001, 2009
Chhattisgarh	13/18	2,248	2,880	11.28	18,898	303	85	60	2004
Jharkhand	21/24	165	1,566	4.75	40,605	874	270	46	2001
Madhya Pradesh	1/50	0	5	0.01	30,217	784	251	431	-
Maharashtra	3/35	172	541	0.48	5,907	12	7	613	2005
Orissa	19/30	3,155	782	1.86	1,235	672	383	93	2006, 2012
Telangana	8/10	522	24	0.07	129	0	0	283	1997
Uttar Pradesh	3/71	4	15	0.01	10	47	21	811	-
West Bengal	4/19	57	699	0.77	49'122	283	178	472	2010
Total	102/308	7,297	7'938	0.99		4,700	1,830	3,713	-

Note: This table summarizes our main variables of interest for each state affected by the conflict. *Total surrenders* and *total deaths* sum the number of fatalities related to the Maoist Insurgency since 2005. *Deaths incidence* gives the ratio between total deaths and 100,000 inhabitants in percent. *Affected districts* are the number of districts affected by the Maoist insurgency over total districts in each state. *Mineral resources* are the value of the 2015 mineral production in billion USD. *Public works - roads* is the daily average of roads under public work construction in 2015, while *public works - contractors* is the daily average of contractors awarded for public works in 2015. *Forest industry firms* are the number of firms working in the industry between 1998-2008. *Rehabilitation program* is the year in which each state implemented their own policy.

[31] We restrict the firms related to forest resources using the following 4-digit National Industrial Classification (2008): post-harvest crop activities (0163), gathering of non-wood forest product (0230), manufacture of tobacco products (1200), manufacture of machinery for food, beverage and tobacco processing (2825), wholesale of agricultural raw materials and live animals (4620).

4.4.4 Additional data

We supplement our dataset with additional district-specific information. We collect monthly rainfall data by districts from the Indian Meteorological Department (IMD) for the years 2013-2017, which allow us to control for potential confounding factors. More details are provided in the identification strategy. Second, we add forest resources information, which was collected from the Ministry of Environment and Forest, for the year 2015. Conflict-affected districts tend to be highly covered in forest resources, with a large variation between districts. We use these data as a robustness check for our measure of firms working in the forest industry.

4.4.5 Descriptive Statistics

The sample used in the baseline analysis includes all districts in the 10 States affected by the conflict following the Ministry of Home Affairs list, limiting the sample to 102 districts out of 627.[32] Table 4.2 provides a general picture of conflict-affected States. The Maoist insurgency is a very heterogeneous conflict: the 10 States are not affected in the same magnitude. For instance, Jharkhand can be considered as a severely affected State with more than 87% of its territory under the Maoist insurgency. Since 2005, the Maoist insurgency has killed about 8,000 individuals, while more than 7,000 insurgents have surrendered to the local authorities. One drawback from our analysis is that we do not have information on how many insurgents are involved in the conflict, neither on the recruitments.

Table 4.3 provides summary statistics of the main variables, which all vary at the district level. Our final sample contains 94 districts as there are missing information for 8 districts in the case of the currency shock. A detailed table is given in Appendix 4.D. On average, districts lie at the third category of cash shortage, which vary between 1 and 7. Concerning the conflict data, we provide descriptive information on the cumulative summation of all events at the end of the period, i.e. one-year post-police. The initial date vary depending on the source of data and the type of event, as discussed earlier. For instance, using SATP information, the average total number of violent incidents recorded in a district since 2010 is 3,029. On the other hand, if we concentrate on ACLED data, the average is 3,431 however, since 2016. We account for the under-reported number of incidents in SATP data in our robustness analysis.

[32] We follow the 2015 list of 106 districts in 10 Left Wing Extremism affected States from the Ministry of Home Affairs, which is based on their violence profile and other parameters. These States are covered under the Security Related Expenditure Scheme, which allow them to receive reimbursement for counter-insurgency measures. As rainfall information is not recorded in certain districts and some of them were either split or merged between 2013 and 2017, we merged them to create a balanced panel dataset of 102 districts.

Table 4.3: Descriptive Statistics - District-level

	Obs.	Mean	S.D.	Min	Max
Demonetization Shock	94	3.106	1.610	1	7
SATP (#)					
Violent Incidents	102	3.029	7.780	0	56
Explosives	102	1.833	4.893	0	32
Fatalities - Total	102	6.843	22.04	0	190
Fatalities - Insurgents	102	3.304	9.576	0	54
Fatalities - Police Forces	102	2.343	11.69	0	113
Fatalities - Civilians	102	1.196	3.380	0	23
Surrenders	102	68.73	229.9	0	1,529
ACLED (#)					
Violent Incidents	102	3.431	7.269	0	50
Battles	102	1.863	4.326	0	29
Explosives/RemoteViolence	102	0.578	1.901	0	15
Violence against Civilians	102	0.637	1.225	0	5
Disrupted Weapons Use	102	0.0196	0.139	0	1
Looting	102	0.304	0.768	0	4
Mineral Resource					
Mineral production value (N)	102	0.0115	0.0992	0	1
Large-scale mines (#)	102	2.059	4.188	0	21
Mining leases (#)	102	14.33	36.55	0	284
Public Works					
Roads (#/day)	102	28.32	34.12	0	156
Companies (#/day)	102	11	12.67	0	68.28
Expenditure (INR/day)	102	8.303	9.937	0	44.63
Forest Industry					
Firms (#)	102	12.82	49.77	0	462
Forest Cover (%)	102	26.26	18.32	0	81.71

Source: authors' computation from various sources of data cited in the main text.

4.5 The Impact of the Demonetization on the Maoist Insurgency

The first step of the empirical strategy consists in analyzing the impact of the demonetization on the Maoist insurgency. We use daily observations on Maoists' activities at the district level and propose two sets of results revealing coexistant mechanisms of transmission. First, we "zoom out" by focusing on the impact of the adverse income shock on general violence. We label this first set of results the *armed insurgency* channel, as we argue that the decisions to commit further violence post-policy are adopted at the level of the organization. Second, we "zoom in" the organization of the insurgency by concentrating on the act of surrendering. This is the *individual choice* channel, under which we make the assumption that the will to surrender is an individual mechanism.

4.5.1 Identification Strategy

The aim of our analysis is to assess whether policies that disrupt insurgents' finances are effective in reducing violence. When terrorists face a fund shortage, they cannot provide war supplies to their troops, who can either suspend the attacks until the shortage is over or surrender to security forces. We argue that the context of the Maoist conflict at the time of demonetization serves well to test this channel for two main reasons. First, demonetization was followed by a sudden and large cash shortage, which may trigger the channel. Second, the Indian government was successful in maintaining secrecy around the policy until the date of the announcement, so that Maoists (and anyone else) could not adjust in advance. As this policy shock was unexpected, the impact on Maoist finances is expected to be large and the magnitude of the channel significant.

This identification strategy is based on two main identifying assumptions. First, we assume that, within Maoists' organizational structure, each district is responsible for collecting the funds it needs and it does so by relying on its local resources only. We argue that the guidelines set out by the Communist Party of India on January 2007 to organize its finances bring arguments in favor of this assumption. By these guidelines, committees at all geographical level must be financially self-sustainable (Ramana, 2018). All committees are thus responsible to collect their funds, allocate them to cover their needs and save what they do not use as reserves. In addition, the basic units for collection and allocation of funds are the Zonal Committees. In order to grasp this zone-driven logic, we base our analysis at the district level. In case of fund shortages, Maoists in districts that are rich in extortion resources are more capable of rising new cash than districts with little resources.

Second, we assume that, in the very aftermath of demonetization, low-resources districts did not receive funds from other districts. In principle, Maoists' centralized finance system allows the Central and State Committees to reallocate the excess funds where needed. However, we argue that this reallocation system could not work properly following the demonetization, specifically in the short term. Notably, since the policy was sudden and unexpected, Maoists could not take precautionary measures and reallocate resources in advance to low-resources districts. Furthermore, in the emergency of the fund shortage, it is likely that Maoists in high-resources districts used the new cash they could extort primarily to cover their emergency needs. It then took some time for committees of these districts to store new excess funds as reserves for reallocation purposes. Thus, low-resources districts were left with little funds after the policy and were very exposed to the consequences of the fund shortage. In the long term, the impact of demonetization on Maoists' violence may level out, as local committees would rebuild their reserves and the Central Committee would reallocate them to Maoists in districts more affected by the shock. However, Vanden Eynde (2018) suggests that Maoists capacity to share resources across local units is limited in general and therefore the impact of demonetization may also last in the long term.

We use a Generalized OLS difference-in-differences (DID) specification to grasp differences in the trends of violent activities between districts after the demonetization. Specifically, we test whether violence decreases after the demonetization (first difference) are larger in districts where the cash shortage was more severe (second difference). We estimate the following specification:

$$Y_{dt} = \beta \, PostDM_t \times DMshock_d + \gamma \, PostDM_t \times X_d + \lambda_d + \lambda_{st} + \varepsilon_{dt} \qquad (4.1)$$

The level of analysis is the district d × day t. The dependent variables, Y_{dt}, represent our two sets of results. First we focus on the impact of the demonetization on violence, by looking at the daily cumulative summation of either violent incidents or fatalities. We argue that these results is an *armed organization channel*, since, in most cases, armed violence is a group-wise decision. Second, we analyze the impact of the policy on the cumulative summation of surrenders, as an *individual-choice channel*.

$PostDM_t$ is a binary variable taking the value 1 after the demonetization, 0 otherwise. $DMshock_d$ is the district-level measure of intensity of the policy, a categorical variable from weakly (1) to severely (7) affected by the cash shortage. X_d accounts for other potential shocks that could have simultaneously affected the outcome variables, such as rainfall shock or policy operations, that are discussed hereafter. λ_d are a set of district fixed-effects that filter out all time-invariant characteristics affecting the outcome variables and the demonetization shock, e.g. local characteristics. Similarly, λ_{st} corresponds to a set of state × day fixed-effects that account for time-variant unobservables such as state-level policies that might affect the outcome variables.[33] Given that our variables of interest are geographically clustered and

[33] Economic policy and counter-insurgency strategies are decided at the state level and vary greatly between

that we rely on a high temporal resolution, we adjust our standard errors for both spatial and serial correlations in all specifications. We apply a spatial HAC correction to the standard errors following Colella et al. (2019). The serial correlation is assumed to vanish after 30 days, while the spatial correlation is within a radius of 500 km. We provide robustness analysis by varying the cutoffs of both spatial and temporal corrections. Baseline results are based on a one-year pre- and post-policy, i.e. from 08/11/2015 to 08/11/2017, and we restrict our analysis to the 102 districts affected by the conflict in 2015.[34]

Our coefficient of interest is β which explains the interaction term between the dummy for the demonetization policy and our geographical variation in the intensity of the cash shortage, $PostDM_t \times DMshock_d$. Given the fact that we include district and state $\times$ day fixed-effects in all specifications, our identification strategy relies on the exogeneity of the interaction term, which we account for in Section 4.7 for the timing of the policy, as well as in Section 4.4 for the geographical distribution of the shock.

A first threat to our identification strategy is the possibility that the relationship between the outcome variables and district-level characteristics changes in the post-policy period. For instance, it could be argued that a confounding contemporaneous income shock could explain a change in violence or insurgents' surrender. For instance, Vanden Eynde (2018) finds that lack of rainfall increases Maoists' violence against the security forces but only in districts where mining activity is sufficiently important, whereas it increases violence against civilians regardless of the location of mining activities. To address this concern, we control for district-level monthly rainfall shocks, which proxy for labor-income shocks, in our baseline specification. Our rainfall shock, X_{dm}, is built similarly to Miguel et al. (2004) as the proportional change in rainfall from the same month in the previous year, $(R_{dm} - R_{d,m-12})/R_{d,m-12}$, where d stands for district and m for month.

A second threat to our identification strategy is the possibility that targeted police operations, unrelated to the implementation of the demonetization, could directly have an impact on both violence and surrenders. In order to account for this potential confounding factor, we control for post-policy police operations in our baseline regressions, using the number of deaths of security forces as a proxy. However, we only use this measure in our specification related to surrenders for two reasons. First, our measure of police operations is subject to a bad controls issue, as it could itself be an outcome variable (Angrist and Pischke, 2008). Second, when we focus on violence, we believe that the *heats-and-minds* channel could be a

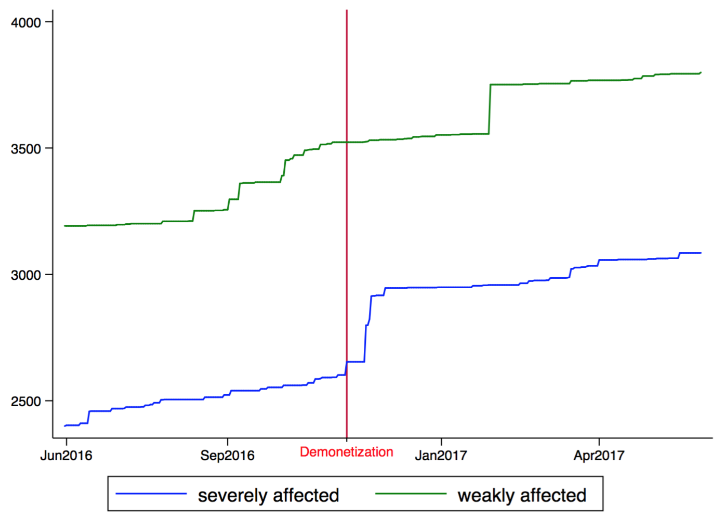
states. For instance, different Indian states have implemented a surrender and rehabilitation policy for Maoists, which includes protection and a stipend for the insurgents who surrender before the police. State $\times$ day fixed-effects allow to control for such unobserved heterogeneity (Vanden Eynde, 2018).

[34]As discussed, we follow the 2015 list of 106 districts in 10 Left Wing Extremism affected States from the Ministry of Home Affairs, which is based on their violence profile and other parameters. However, due to missing information and the variation of district boundaries over time, we create a balanced panel dataset of 102 districts. The list of districts used can be found in Appendix 4.D. Note that due to missing information on the demonetization shock, only 94 districts are considered in our final sample.

driver of a reduction in violence, through enhanced police operations post-policy. Therefore, we do not want to partial out such mechanism. On the other hand, when we concentrate on surrenders, we suppose that the *opportunity cost* mechanism, through financial resources, is more relevant. In a nutshell, we expect that Maoists surrender because they have higher financial incentives to turn in to the rehabilitation program, and not because there a more police operations due to the demonetization.

A last note on the construction of our dependent variables. We exploit the cumulative summation of our daily outcome variables for two reasons. First, the parallel trends assumption required in difference-in-differences estimation would be violated if we were using daily incidence of violence. Using the cumulative summation allows an analysis of the differences in trends of our treated and untreated observations. In our context, all districts are treated but at different intensities, i.e. some areas are highly affected by the cash shortage, while others are weakly affected by the cash shortage. Figure 4.5 plots the cumulative summation of surrenders between highly affected districts vs. weakly affected districts, which gives insights into our empirical exercise.[35] Second, our use of the cumulative summation allows us to work with daily observations containing rare events allowing for the identification of a causal interpretation. It enables us to proceed with a large set of fixed effects, partialling out potential cofounding factors. Moreover, the demonetization was implemented within a night, granting an exact pinpointing of a time variation.

Figure 4.5: Differential impact of the demonetization on surrenders



Note: This figure plots the daily cumulative summation of surrenders since March 24, 2006. The blue line represents districts where the cash shortage was severe (categories 4 to 7 of the demonetization shock), while the green line represents districts weakly affected by the demonetization (categories 1 to 3). This figure gives insights to the parallel trend assumption.

[35]Figure 4.5 takes an arbitrary cutoff between the categorical demonetization shock for the weakly affected (1 to 3) vs. severely affected districts (4 to 7). However, this does not have any effect in our baseline results, since we do not use any cutoff, but rather use the entire variation in the intensities.

4.5.2 Baseline Results

Our first set of baseline results are displayed in Table 4.4, in which we focus on violence through the number of violent incidents and fatalities. Following the demonetization, the Maoist insurgency was badly hit, as their activity relies heavily on the availability of their funds. In this section, we uncover the reaction of the armed group to their fund shortage.
In the SATP panel, our main source of data, we find that, in the aftermath of the demonetization and in districts that were severely affected by the cash shortage, there is a decrease in the trend of overall violent incidents (column 1). Column 2 presents a sub-type event of violents incidents, incidents related to the use of any type of explosives. Results suggest a similar decreasing path. This is in line with our expectations: capital-intensive violence should decrease post-policy. The limitation of the SATP data does not allow use to investigate further the types of violent activities that were relatively more or less affected by the cash shortage. Turning to ACLED, which allows us a more precise look at sub-event categories, we find a similar decreasing trend in overall violence, despite the loss of significance (ACLED Panel, column 1). Disaggregating the types of violence, we focus on four types of sub-events: explosions/remote violence, battles, violence against civilians and strategic developments.[36] The latter, which is defined as "important information regarding the activities of violent groups that is not itself recorded as political violence", is further separated in two types of events: disrupted weapons use and looting/property destruction.[37] Results show that two types of violent activities decrease, in line with our expectation: explosions/remote violence and violence against civilians (columns 2 and 4). There is no significant change for battles and disrupted weapons use (columns 3 and 5). However there is an increase in the trend of looting cases. This last result uncovers a substitution effect, which is in line with the *appropriation* channel: Maoist lost their cash reserves, which prevented them to sustain their fight, however, they diverted their actions to the seizure of goods and property through an increase in looting.
In terms of magnitude, we compute the growth differential between districts at the 75^{th} percentile of the observed distribution of the cash shortage intensity (category 4 out of 7), i.e. highly affected districts, and districts at the 25^{th} percentile (category 2), i.e. weakly affected districts. After the policy, the difference in cumulative violent incidents between districts at the 75^{th} and 25^{th} percentiles (of the observed distribution of the cash shortage intensity) decreases by -0.34 incidents. This magnitude remains small, however, we have to keep in mind that the direction of the violence varies depending on the type of events.

[36]We do not take into account protests and riots, two other sub-categories in the ACLED dataset, since Maoists do not rely on such violent interaction.

[37]Disrupted weapons "capture all instances in which an event of *Explosions/Remote violence* is prevented from occurring, or whenever armed actors seize significant caches of weapons", while looting cases are recorded when armed groups seize goods or property other than weapons without reported violence.

We complement our results by looking at fatalities, a well-used proxy of violence in the conflict literature. When we examine the overall trend in fatalities (SATP Panel, column 3), we notice that there is a positive impact of the policy in districts severely affected by the policy relative to others. However, the rise in the trend of fatalities is mainly driven by insurgents' fatalities (column 4), while there is a decrease in the trend of police forces' deaths (column 5) and no significant impact on civilians (column 6). We interpret these results as a preliminary evidence of the *hearts-and-minds channel*. When a negative income shock hits the illegal sector, one possible reaction is a decrease in violence due to the improved police's ability to oppose insurgents or pay off rebels through civilians support.[38] This channel is not directly testable, as we do not have access to data on the number of police informants.

Table 4.4: Baseline Results - violence

SATP Panel	Violent Incidents	Explosives	Cumulative Fatalities	Fatalities Insurgents	Fatalities Police Forces	Fatalities Civilians
	(1)	(2)	(3)	(4)	(5)	(6)
Post DM $_t$ × DM Shock $_d$	-0.160***	-0.159***	0.199**	0.287***	-0.072**	-0.016
	(0.032)	(0.024)	(0.080)	(0.066)	(0.031)	(0.010)
Post DM $_t$ × Rainfall Shock $_{dm}$	0.003*	0.002*	0.010**	0.003*	0.007**	0.000
	(0.002)	(0.001)	(0.004)	(0.001)	(0.003)	(0.000)
Growth Differential	-0.32	-0.32	0.40	0.57	-0.14	-
Fixed Effects	d,st	d,st	d,st	d,st	d,st	d,st
Observations	68,777	68,777	68,777	68,777	68,777	68,777
R-squared	0.950	0.932	0.960	0.926	0.951	0.989
ACLED Panel	Violent Incidents	Explosions/ Remote violence	Cumulative Battles	Violence a. civilians	Disrupted weapons use	Looting
	(1)	(2)	(3)	(4)	(5)	(6)
Post DM $_t$ × DM Shock $_d$	-0.005	-0.078***	0.047	-0.031*	-0.000	0.056***
	(0.070)	(0.016)	(0.044)	(0.017)	(0.000)	(0.014)
Post DM $_t$ × Rainfall Shock $_{dm}$	0.006***	0.001**	0.003***	0.001***	-0.000	0.000***
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Growth Differential	-	-0.15	-	-0.06	-	0.11
Fixed Effects	d,st	d,st	d,st	d,st	d,st	d,st
Observations	63,221	63,221	63,221	63,221	63,221	63,221
R-squared	0.789	0.780	0.769	0.777	0.081	0.780

Notes: SATP Panel: The dependent variable is the cumulative of daily violent events starting on January 1, 2010. ACLED Panel: The dependent variable is the cumulative of daily violent events starting on January 1, 2016. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). Growth differential figures calculate the difference between the 75th percentile and the 25th percentile distribution of the DM shock variable (4-2). *** p<0.01, ** p<0.05, * p<0.1.

Our second set of results are linked to the *individual choice channel*. In an armed conflict, we argue that the decision to enter or exit an insurgency is a individual choice, while the subsequent violent actions of such organization is a group-level decision. In this specification,

[38] Details are explained in Section 4.3.

we analyze the impact of the cash shortage on surrenders of Maoist insurgents to the local authorities. In our context, the demonetization provides higher economics incentives for insurgents to surrender post-policy due to the existence of rehabilitation programs in each state, offering various economic benefits such as an access to education, an income and/or an accommodation.[39] This specification also offers an understanding of the reduction in violent events. All else equal, a sufficiently large number of surrenders should lead to a decrease in violence.

Table 4.5: Baseline Results - surrenders

	Cumulative Surrenders		
	(1)	(2)	(3)
Post DM $_t$ × DM Shock $_d$	9.460***	9.120***	9.119***
	(1.847)	(1.823)	(1.823)
Post DM $_t$ × Rainfall Shock $_{dm}$		0.082***	0.082***
		(0.023)	(0.023)
Post DM $_t$ × Police Operations $_d$			7.476***
			(2.104)
Average Impact	29.39	28.33	28.33
Growth Differential	18.92	18.24	18.24
Fixed Effects	d,st	d,st	d,st
Observations	68,808	68,103	68,103
R-squared	0.946	0.948	0.948

Note: The dependent variable is the cumulative of daily surrenders starting on March 24, 2006, from SATP data. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). Growth differential figures calculate the difference between the 75^{th} percentile and the 25^{th} percentile distribution of the DM shock variable (4-2). *** p<0.01, ** p<0.05, * p<0.1.

Table 4.5 displays the results with respect the cumulative summation of surrenders as our dependent variable. In column 1, we only control for district and state × fixed-effects. In column 2, we additionally control for rainfall shocks that might have occurred post-policy. Finally, in column 3, we include a second covariate, police operations, which is proxied by the number of police forces deaths related to the Maoist insurgency. Column 2 represents our preferred specification, as it account for a possible confounding income shock, without the potential bias of bad controls.[40] Results are robust to the inclusion of covariates and show that the trend in surrenders increases post-policy in districts highly affected by the cash shortage relative to others. We estimate that growth differential between districts at the 75^{th} percentile and the 25^{th} percentile distribution of the demonetization shock is about

[39]Table 4.2 provides an overview of the implementation dates of such programs in each conflict-affected states summarized in Shapiro et al. (2017).

[40]Details are discussed in the identification strategy.

18 surrenders. In terms of average impact, a district at the average intensity of cash shortage (around the category 3) observes an increase of 28 surrenders post-policy.

4.5.3 Duration

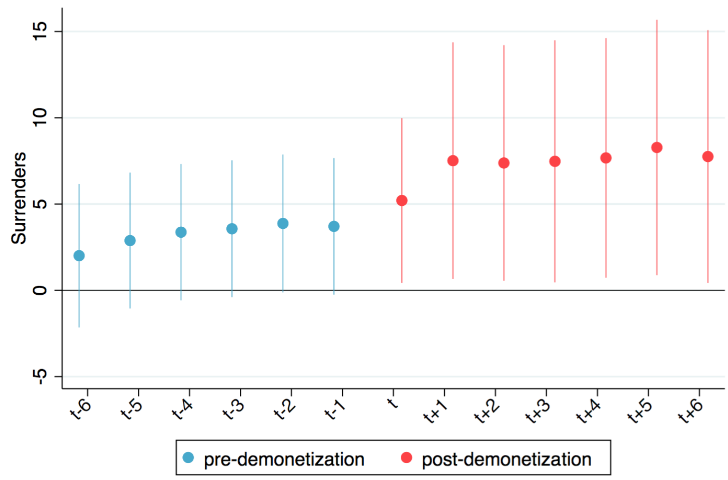
Studying the impact of the demonetization puts into question the duration of the effect. The immediate magnitude of the cash shortage was immense, however, in the long term, the economy fully recovered with the replacement of demonetized banknotes. Chodorow-Reich et al. (2020b) show that within a night, money in circulation declined by 75%, whereas it only slowly recovered over the next year. By the end of March 2017, the currency in circulation was only 74% the pre-demonetization value (Aggarwal and Narayanan, 2017), while the Reserve Bank of India (RBI) accounts that 99.3% of the old notes were deposited back into the banking system by March 2018 (Reserve Bank of India, 2018a).[41] The slow readjustment of the cash in circulation does not mean that the demonetization had no impact on the Maoist insurgency. First, part of the recovered banknotes could have been seized by the authorities, which we cannot directly observe. Second, even if Maoists have found a way to counter the various security measures enforced by the government, the demonetization was costly, whether it is in terms of the loss reserves or the diverted time to rebuilt finances. However, the short term effect of the cash shortage could lead to a temporary effect on the insurgency. Maoists have been hit, but for how long? Figure 4.6 gives insights to the duration the effect on the trend in surrenders. In this caterpillar plot, we proceed with a lead-and-lags analysis by replicating our baseline results on surrenders, i.e. Table 4.5 column 2, by including monthly dummies instead of our policy indicator. The figure plots the 6-months pre- and post-policy coefficients with their respective 95% confidence intervals. Blue dots represent pre-demonetization monthly dummies and red dots post-policy dummies. It shows how the effect of the shortage evolves over time. The largest rise in surrenders happens between t and $t+1$ (highest slope), i.e. within the following month of the demonetization. However, while the effect does seem to fade away, the magnitude is pretty constant, showing that the effect is probable short term.

This figure also serves as a sensitivity analysis. If the policy is indeed causing the effect in the trend in surrenders, then the leads should not be significant. In our context, we find that there is no anticipatory effects or pre-existing trends, as none of the pre-policy coefficients are statistically different from zero as depicted in blue.

To further evaluate the lasting impact of the demonetization shock requires more data. In our analysis, we rely on the intensity of the shock which only accounts for the arrival of

[41] The RBI reports that "[...] eventually, the pre-demonetisation level of currency in circulation was exceeded in March 2018.", however it also stipulates that it is yet under its projection: "While the currency in circulation as on March 31, 2018 accounted for 101.8 per cent of its pre-demonetisation level, it works out to around 88 per cent of its underlying 3-year trend had there been no demonetisation" (Reserve Bank of India, 2018a).

Figure 4.6: Caterpillar Plot



Note: This figure plots the coefficients of an interaction term between the cross-sectional demonetization shock and monthly dummies. The dependent variable is the daily cumulative summation of surrenders since March 24, 2006. The blue dots represent pre-demonetization coefficients, while the red dots represent post-policy coefficients. This figure gives insights to duration of the policy effect.

new notes in December 2016. However, this cash shortage being slowly relaxed over several months, the spatial differences across districts dissipate over time.

4.6 Mitigation Effects

In our baseline results, we show that, in districts severely affected by the cash shortage, the demonetization led to a decrease in violence, through two mechanisms: as a decision at the *armed organization* level and as an *individual choice*. In the former case, we find a reduction in the trend of violent incidents and deaths of security forces. In the opposite, we find a rise in the trend of insurgents' fatalities. One potential channel are enhanced police operations. In the latter, we find that there is an increase in the trend of surrenders, which furthers explain the decrease in violence. When it comes to the insurgents' personal decision to exit the conflict, incentives play a significant role. The *opportunity cost* theory tells us that as opportunities in the legitimate market improve relatively to the illegal occupations, rebels exit the conflict. However, a shock affecting the opportunity cost of fighting such as the demonetization also tends to alter the returns to appropriation of existing wealth. In this section, we investigate the interaction between the opportunity cost channel uncovered in our baseline results, and the appropriation mechanism, by looking at Maoists' abilities to raise new cash through their usual extortion means: mineral resources, public work contractors and forest resources, as described in Section 4.2. We focus on the *individual choice* channel, examining whether the increase in surrenders found in our baseline results is mitigated when insurgents have the possibility to refinance themselves and their organization through renewed extortion. Specifically, we proceed with a difference-in-difference-in-difference specification,

$$\begin{aligned} Surrenders_{dt} = {} & \beta \, PostDM_t \times DMshock_d \\ & + \alpha^i \, PostDM_t \times Fundings_d^i \\ & + \gamma^i \, PostDM_t \times DMshock_d \times Fundings_d^i \\ & + \lambda_d + \lambda_{st} + \varepsilon_{dt} \end{aligned} \tag{4.2}$$

in which the cumulative summation of surrenders, $Surrenders_{dt}$ is regressed on: (1) the interaction between the post-policy dummy $PostDM_t$ and our demonetization shock $DMshock_d$ such as in our baseline specification; (2) an interaction between the three cross-sectional sources of finances i, $Fundings_d^i$ and $PostDM_t$; and (3) a triple interaction between these terms. All coefficients measure the post-policy differential effect on the trend in surrenders. β estimates the differential effect of the demonetization shock, i.e. the intensity of the cash shortage, α^i identifies the differential effect of the availability and quantity of income sources and γ^i shows whether the differential effect of the demonetization shock, i.e. β, is alleviated when there are available means of revenues for Maoists. Our three measures of $Fundings_d^i$ are (a) the production value of mineral resources in 2015, using international prices to mitigate potential endogeneity concerns; (b) the number of roads under public work constructions in 2015; (c) the number of manufacturing firms working in the forest industry between 1998-2008. Specification (2) follows the same structure as our baseline specification. include two sets of fixed effects: λ_d are the district fixed-effects and λ_{st} corresponds to state $\times$ day fixed-effects. Standard errors are adjusted for spatial (500 km) and serial correlation (30 days) (Colella et al., 2019). Results are based on a one-year pre- and post-policy, i.e. from 08/11/2015 to 08/11/2017, and we restrict our analysis to the 102 districts affected by the conflict in 2015.

Before turning to the results, we discuss identifying assumptions and potential threats to identification for the measures of funding sources. A first potential concern could arise from unobservables correlated with both district's mining activities and its trend in surrenders. For instance, there could be a peak in the intensity of the conflict, on a specific day and in a specific district, that could trigger both an increase/decrease in surrenders and the closing of mines. Similarly, a second concern could result from reverse causality from trends in surrenders to the mining activity. For instance, it can be argued that a large increase in surrenders could trigger a change in the location of mines. To account for these issues, including state $\times$ day fixed-effects is crucial, as they partial out a common shocks at the state-level. Second, the analysis is restricted to a sample of districts that were affected by the Maoist Insurgency both before and after the implementation of the demonetization policy. Furthermore, our variable of interest is the value of mineral production before the implementation of the policy. Last, we perform a variety of robustness checks using different measure of mining activity. Results can be found in Appendix Table 4.B.1 and display similar patterns.

Turning to public works, it could be similarly argued that low economic development might codetermine the awarding of public works by the state authorities and the location of the conflict, hence of the surrenders. However, our district fixed-effects account for this potential unobserved confounding factor, as well as other initial local conditions (such as GDP). On the other hand, a reverse causality issue seem unlikely. While public works are an instrument to foster development by the construction of roads for instance, it is doubtful that the trends in Maoist surrenders are a determinant of the number of public work contractors. From Figure 4.4, we can see that the distribution of public works is quite heterogeneous across districts, and that Maoist-affected districts are not specific targets for the spending in public funds. Note that the fact that the public works are clustered is taken into account in our spatial correction of the standard errors. However, to account for the potential bias in the estimate, we use the number of public work awarded to contractors before the implementation of the policy. We also perform robustness checks using alternative variables (see Table 4.B.2 in Appendix).

Concerning the localization of firms working in the forest industry, the underlying determinants are rooted in the ecosystem of the districts, such as the availability of such resources (which are depend on climate conditions and topography). While forest products play an important role in the source of income for Maoist, we believe that the trends in surrenders do not impact directly impact the presence of firms. However, to account for a potential bias, we compute sensitivity tests by using the district-level coverage in forests as well as an indicator variable for the presence of such industry in the district rather than a count variable which is more subject to bias. Results do not display similar patterns. We detailed the potential reasons in Appendix, Table 4.B.3.

Table 4.6 reports the results for the coefficients β and γ^i. In all four columns, results on the post-policy differential impact of the demonetization are positive and statistically significant. The magnitude is very similar to our baseline results (Table 4.5 column 2), and stable across the sources of revenue. The triple interaction term highlights that the positive and significant increase in the trend of surrenders post-policy in districts highly affected by the cash shortage is mitigated in districts where there is a relatively high reliance on either mineral resources (column 1), public work (column 2) and forest products (column 3). When we include all main sources of finances for the Maoist in one specification, the results are unchanged (column 4).

This result further reveals the complementary while contrary effects of the opportunity-cost and appropriation channels. While at the armed organization-level, the Maoist insurgency diverts its usual violent activities towards rent-seeking in order to rebuild its lost finances, at the individual-level, part of the insurgents exits the illegal market for legal occupations, unless there are potentially appropriable resources.

Table 4.6: Mitigation Effects

	Cumulative Surrenders			
	(1)	(2)	(3)	(4)
Post DM $_t$ × DM Shock $_d$	9.32***	12.59***	9.47***	13.25***
	(1.86)	(2.50)	(1.90)	(2.63)
Post DM $_t$ × DM Shock $_d$ × Mineral Resources	-48.58**			-57.29**
	(22.46)			(22.20)
Post DM $_t$ × DM Shock $_d$ × Public Works		-0.07***		-0.07***
		(0.02)		(0.02)
Post DM $_t$ × DM Shock $_d$ × Forest Resources			-0.04***	-0.05***
			(0.01)	(0.01)
Post DM $_t$ × Fundings $_d^i$	✓	✓	✓	✓
Post DM $_t$ × Rainfall shock $_{dm}$	✓	✓	✓	✓
Fixed Effects	d,st	d,st	d,st	d,st
Observations	68,777	68,777	68,777	68,777
R-squared	0.95	0.95	0.95	0.95

Note: The table presents ordinary least squares (OLS) estimates for the specification (2), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily surrenders starting on March 24, 2006, from SATP data. State × day and District fixed-effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

4.7 Robustness Checks

Studying the impact of counter-insurgency policies, our identification strategy relies on the timing of the policy and on the measure of our demonetization shock, i.e. the varying intensity of the cash shortage in each district. The difference-in-difference specification provides evidence that the policy has had an unexpected and welcomed negative impact on insurgents who decreased their violent activities and surrender to the local authorities. However, it could be argued that there are other characteristics at the district-level that correlate with the demonetization shock and affect rebel activity.

In this section, we show that the baseline results are robust to various sensitivity checks, by extending the previous findings and exploring a number of potential alternative factors. Table 4.7 Panel A replicates the baseline result of the trend in violent incidents (Table 4.4 Panel SATP column 1). Panel B replicates the results for the trend in surrenders (Table 4.5 column 2). The following columns are detailed hereafter.

4.7.1 Alternative Specifications

District-specific time trends. We start by replicating our baseline results by including district-specific time trends. While state × day fixed effects control for aggregate time effects, the inclusion of district-specific time trends help to rule out the possibility that the districts severely and weakly affected by the cash shortage were already on differential growth trajectories in their outcome variables. The idea is to absorb pre-existing trends. Results are displayed in Table 4.7 column 2 and are stable across both outcome variables.

Weighted Regression. We replicate our baseline results weighted by each districts' total outcome variables the day before our regression's timeframe, i.e. on November 7, 2015.[42] Our weighted regressions allow for a higher emphasis on districts with larger variance in the distribution of the outcome variables. Results are displayed in Table 4.7 column 3. Both coefficients are magnified in absolute terms. This could be driven by the fact that we loose all districts where there are no violent incident or surrender.

Logarithm. Next, we check whether results are sensitive to the definition of our dependent variables. Using the logarithm of the daily cumulative of violent incidents and surrenders as our explained variables allows to smooth the trends and further taking into account issues of common support across highly and less exposed districts. However, due to the inclusion of all districts in conflict-affected states, we lose our counterfactual, i.e. the districts in which they were no violence or surrenders over the entire period. Table 4.7 column 3 displays the results.

Fixed Effects. In our baseline regressions, we always include both district fixed-effects and state × day fixed-effects to account for either geographical or time invariant unobservable characteristics. However, the inclusion of such large a set of fixed-effects is computationally demanding and might saturate the model. Thus, we relax our specification by including only district fixed-effects and time fixed-effects. Results are shown is Table 4.7 column 5 and are consistent with our baseline evidence.

Outliers. We further check whether results are driven by outliers. From Figure 4.C.1 in Appendix 4.C, it is noticeable that there is a peak of surrenders on November 8, date of the implementation of the policy. However, as the policy was announced in the evening, it is unlikely that this peak drives the results. From the local newspaper, we know that

> [...] 52 milita members have surrendered before Malkangiri police. [...] the surrenders have taken place close on the heels of the killing of at least 28 rebels

[42] Our baseline specification is based on a one-year pre- and post-policy, i.e. from 08/11/2015 to 08/11/2017. Thus our weights are the total number of violent incidents or surrenders per district between the beginning of our sample, i.e. 01/01/13 and 26/03/2006 respectively, to 07/11/2015.

> in a fierce encounter with police on October 24.[43]

Second, a large peak is apparent post-policy, on January 29, 2017. The Indian Express reveals that

> 195 Maoist cadres surrendered before senior police officials during a programme at Narayanpur district headquarters.[44]

While the implementation of a post-policy programme in Narayanapur district could be driven by the demonetization, we check wether this large outlier drives our results. As we cannot control for day $\times$ district specific events that would have a direct impact on surrenders, we removed both peaks from our baseline regressions. Results are displayed in Table 4.7 column 6, showing similar significant coefficients, although lower in magnitude due to lower dependent variable.

Geographical Coverage. Next, we test an alternative geographical scope, by including all districts in India. Results, as shown in Table 4.7 column 7, retain the same sign, but exhibit lower magnitude. This is expected since this specification includes districts that are not affected by the Maoist insurgency, and thus, where there are no violence or surrenders.

Timeframe. We further test alternative timeframe for our baseline regressions. We first expand the timeframe to a longer period of study, from January 2010 to April 2018. Results are larger in magnitude, as shown in Table 4.7 column 8, which might be due to the inclusion of more zeros. Second, we restrain the timeframe of our sample to a 3-month pre- and post-policy. Results, presented in Table 4.7 column 9, remain stable in term of sign, but the magnitude of the impact is slightly lower.

[43] The Indian Express, 09/11/2016.
[44] The Indian Express, 29/01/2017.

Table 4.7: Alternative Specifications

Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Cumulative Violent Incidents								
Post DM $_t$ × DM Shock $_d$	-0.160***	-0.160***	-0.604**	-0.025***	-0.110***		-0.025***	-0.246***	-0.074***
	(0.032)	(0.032)	(0.272)	(0.006)	(0.024)		(0.006)	(0.057)	(0.027)
Observations	68,103	68,103	15,274	27,361	68,103		351,055	133,679	17,190
R-squared	0.953	0.998	0.966	0.983	0.945		0.954	0.863	0.992
Panel B	Cumulative Surrenders								
Post DM $_t$ × DM Shock $_d$	9.120***	9.099***	42.785***	0.013**	4.276***	3.930***	1.748***	15.915***	3.594***
	(1.907)	(1.895)	(9.823)	(0.006)	(0.837)	(0.781)	(0.343)	(2.452)	(1.389)
Observations	68,103	68,103	45,510	47,640	68,808	68,103	351,055	133,679	17,190
R-squared	0.948	0.989	0.979	0.993	0.942	0.949	0.948	0.886	0.993
Post DM $_t$ × Rainfall shock $_{dm}$	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed Effects	d,st	d,st	d,st	d,st	d,t	d,st	d,st	d,st	d,st
Districts	94	94	66/24	74/45	94	94	534	94	94
Months pre/post policy	12	12	12	12	12	12	12	2013-18	3
Time Trends		✓							
Weighted			✓						
Log-linear				✓					
Outliers						✓			

Note: The table presents ordinary least squares (OLS) estimates for the specification (1), where the unit of observation is a day t in a district d. In Panel A, the dependent variable is the cumulative of daily surrenders starting on March 24, 2006, from SATP data, unless stated otherwise. In Panel B, the dependent variable is the cumulative of daily violent incidents starting on January 1, 2010, from SATP data, unless stated otherwise. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

4.7.2 Sensitivity Analysis - Violence

Table 4.8: Comparison of two sources of conflict data

		Violent Incidents	Fatalities	Surrenders
SATP	district-daily	326	1,668	6,485
SATP	district-year	-	3,768	-
SATP	state-year	-	3,823	6,847
SATP	country-year	10,660	4,463	4,759
UCDP	district-daily	1,865	3,645	-

Note: This table compare the total number of violent incidents, fatalities and surrenders between 2010 and 2017 across SATP and UCDP datasets. Note that we do not make the comparison with ACLED data since the timeframe is limited to 2016-2018.

In our main specification, we base our results on both SATP and ACLED conflict datasets. However, while SATP is a widely used dataset in the academic literature on the Maoist Insurgency, at the daily level, the number of observations concerning violence are greatly underestimated. Between 2010 and 2017, SATP records a total of 326 violent incidents, while at the aggregated country-year level there are supposedly 10'660 violent incidents. Data on fatalities show a similar pattern, although on a different scale: 1,668 fatalities in the district-daily dataset from SATP, compared to 3,645 fatalities for UCDP data. Surrenders are less affected by the underestimation. In fact, total surrenders are slightly under-reported if we compare the SATP district-daily dataset with the state-year. However, it is over-estimated compared to country-year level of aggregation. The underestimation of violence data would be an issue in our estimation if, and only if, there are reasons to believe that the under-reporting is linked to the demonetization. We argue that it is not plausible, as the under-reported show similar pattern before and after the implementation of the policy. Therefore, our baseline estimation still represents the population. However, to avoid a potential bias in our results, we replicate our baseline results using the UCDP data. Table 4.9 displays the results. The direction of the impact is stable, with the exception of police forces' fatalities. There is a decrease in the trend of violent incidents, and an increase in total and insurgents' fatalities in line with our baseline results.

Table 4.9: Sensitivity Analysis - violence

UCDP Panel	Violent Incidents	Fatalities	Cumulative Fatalities Insurgents	Fatalities Police Forces	Fatalities Civilians
	(1)	(2)	(3)	(4)	(5)
Post DM $_t$ × DM Shock $_d$	-0.070	0.337**	0.134	0.300***	-0.190***
	(0.082)	(0.139)	(0.087)	(0.053)	(0.042)
Post DM $_t$ × Rainfall shock $_{dm}$	✓	✓	✓	✓	✓
Fixed Effects	d,st	d,st	d,st	d,st	d,st
Observations	68,103	68,103	68,103	68,103	68,103
R-squared	0.997	0.997	0.992	0.997	0.998

Note: The table presents ordinary least squares (OLS) estimates for the specification (1), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily violent incidents starting on March 24, 2006, from UCDP data. State × day and District fixed effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

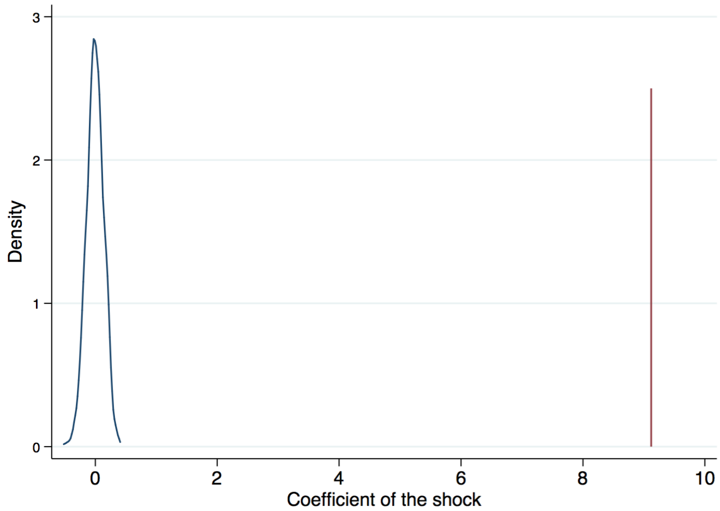
4.7.3 Sensitivity Analysis - Surrenders

In the following subsection, we draw various robustness checks on the validity of our results on both violence and surrenders. However, we only present our sensitivity analysis for surrenders. Results on violence are consistent and can be found in Appendix 4.A.

4.7.3.1 Placebo

Our identification strategy relies both on the timing and spatial allocation of the demonetization policy. The empirical exercises provide evidence that, the demonetization shock, has an immediate effect on both violence and surrenders. However, it could be argue that our results are an artifact of the construction of this shock. Following existing literature (see for instance Berman et al., 2019), we answer this potential concern by performing a placebo test. We randomly permute the spatial allocation of the demonetization shock in each district and estimate specification (2) of Table 4.5 with our newly-assigned demonetization shock. In Figure 4.7, we plot the sampling distribution of our coefficient of interest, for which we repeat the estimation 1,000 times. The red line displays our baseline results which is far from the Monte Carlo coefficients, that are insignificant. This exercise confirms the validity of our approach.

Figure 4.7: Placebo Test



Note: This figure depicts the Monte Carlo sampling distribution of Post DM $_t$ × DM Shock $_d$. We randomly permute the 7 categories of DM Shock $_d$ and run specification (1) (i.e. Table 4.5 column 2) $1,000$ times.

4.7.4 Measurement Errors

Another potential threat to the identification strategy is the construction of the demonetization shock. While we carefully digitized the map in Chodorow-Reich et al. (2020b), it is plausible that we made some reporting errors. To account for potential measurement errors, we enquire the robustness of our results to three alternative definitions of the policy shock, by rescaling our categorical variable. Results are displayed in Table 4.10. Column 1 replicates our baseline results from Table 4.5 column 2, where our demonetization shock is a categorical variable from 1 to 7. In column 2, we rescale the demonetization shock as an indicator variable where categories 1 to 3 correspond to untreated, and categories 4 to 7 to treated. The sign and significance of the result are unchanged, but the magnitude drastically increases, as expected. In column 3, as we slightly change the cutoff categories of the rescaled binary indicator, with categories 1 to 4 corresponding to untreated, and categories 5 to 7 to treated. The magnitude of the result drops to the baseline coefficient. This is due to the fact that there is a high propension of districts affected by the conflict at the category 4 of cash shortage severity. Finally, we rescale our demonetization shock on a scale from 1 to 5, where we merged the middle categories ($3-4=3$, $5-6=4$) as they were the most difficult to disentangle. Results, displayed in column 4, show stable results.

Table 4.10: Measurement Errors in the Demonetization Shock

	Cumulative Surrenders			
	(1)	(2)	(3)	(4)
Post DM $_t$ × DM Shock $_d$ Baseline $[1, 7]$	9.120***			
	(1.907)			
Post DM $_t$ × DM Shock $_d$ Binary $0 = [1; 3], 1 = [4; 7]$		41.853***		
		(10.526)		
Post DM $_t$ × DM Shock $_d$ Binary $0 = [1; 4], 1 = [5; 7]$			10.764***	
			(3.462)	
Post DM $_t$ × DM Shock $_d$ Categorical $[1, 5]$				12.616***
				(2.198)
Post DM $_t$ × Rainfall shock $_{dm}$	✓	✓	✓	✓
Fixed Effects	d,st	d,st	d,st	d,st
Observations	68,103	68,103	68,103	68,103
R-squared	0.948	0.948	0.947	0.948

Note: The table presents ordinary least squares (OLS) estimates for the specification (1), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily surrenders starting on March 24, 2006, from SATP data. State × day and District fixed effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

4.7.5 Spatial Spillovers

All our results do not take into account potential lagged effects in the temporal or spatial dimensions. In this subsection, we allow for the inclusion of spatial spillovers. We construct four types of spatially lagged demonetization shock, depending on a kilometer bandwidth representing the distance from the districts' centroid in a radius. The newly constructed variables takes into account the mean demonetization shocks of the districts within an arbitrary radius on the district in question. Table 4.11 show the results, with our baseline results in column 1. Columns 2 to 5 use the four different kilometers threshold, from 1,000 km to 100 km respectively. We find that the inclusion of spatial spillovers does not affect the magnitude of the impact.

Table 4.11: Spatial Lags

	Cumulative Surrenders				
	(1)	(2)	(3)	(4)	(5)
Post DM $_t$ × DM Shock $_d$	9.120***	10.560***	8.500***	10.369***	10.560***
	(1.907)	(2.151)	(1.817)	(2.152)	(2.151)
Post DM $_t$ × DM Shock $_d$ within 1,000 km		31.496***			
		(8.713)			
Post DM $_t$ × DM Shock $_d$ within 500 km			-131.541***		
			(14.437)		
Post DM $_t$ × DM Shock $_d$ within 250 km				26.858***	
				(6.405)	
Post DM $_t$ × DM Shock $_d$ within 100 km					31.496***
					(8.713)
Post DM $_t$ × Rainfall shock $_{dm}$	✓	✓	✓	✓	✓
Fixed Effects	d,st	d,st	d,st	d,st	d,st
Observations	68,103	65,294	68,103	68,103	65,294
R-squared	0.948	0.950	0.950	0.948	0.950

Note: The table presents ordinary least squares (OLS) estimates for the specification (1), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily surrenders starting on March 24, 2006, from SATP data. State × day and District fixed effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

4.8 Conclusion

This paper provides empirical evidence on the causal link between the financing of armed groups and their violent activities, focusing on the cash nature of such illegal flows. India offers the ideal setting of study with the implementation of the 2016 Indian Banknote Demonetization as a natural experiment. Using a measure of the intensity of the cash shortage caused by the policy, the demonetization has had an unexpected and welcomed negative impact on the Maoist Insurgency. Our findings highlight the simultaneous existence of three existing theoretical frameworks that links income shock to conflict.

At the individual level, we find that there is an increase in the trend of surrenders in areas experiencing a more severe cash shortage. In line with an opportunity-cost channel, the demonetization raises the opportunity cost of insurgency by incentivizing the Maoists to surrender and get into rehabilitation programs offering economic benefits. However, the increase in the trend of surrenders is mitigated in districts where the Maoists have higher abilities to refund themselves through extortion of local economic resources. This uncovers the simultaneous interaction of the appropriation channel, the idea that the demonetization likewise increases the return of fighting toward the appropriation of economic rents.

At the armed organization level, there is a general decrease in the trend of violence, however, with an opposite effect on cases of looting. Alongside we find an increase in the trend of fatalities, however, entirely driven by the deaths of insurgents rather than police forces and civilians. This general picture of the impact on violence does not allow us to illustrate the mechanisms underlying such effect. One could argue that violence decreases because there is an increase in surrenders. While this is a plausible explanation, we do not have any information on the size of the insurgency, neither on potential new recruits. Another argument, from the hearts-and-minds model, is that the demonetization has given the local authorities an increase ability to repress through local support. We argue that the decrease in the trend of fatalities of police forces is a preliminary evidence of this second hypothesis, however not directly testable with our data.

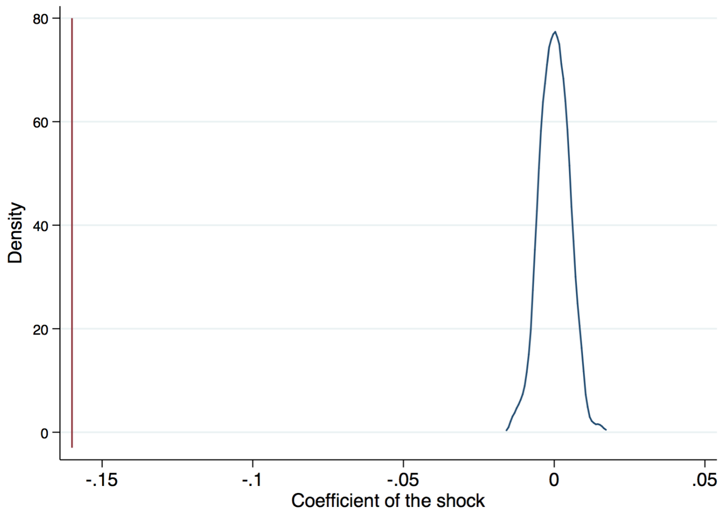
Maoists have been hit, but whether the policy weakens the insurgency in the long term is not clear. Preliminary evidence shows that the effect is short term and dissipates over time. However, more data are necessary to fully acknowledge the impact of such policy.

Overall, our results suggest that the effectiveness of the policy is closely linked to economic incentives driving the Maoist Insurgency. The analysis of the demonetization as a counter-insurgency policy shows that policies which target the cash finances of armed groups can be effective, in the presence of rehabilitation programs.

Appendix 4.A Sensitivity Analysis - Violence

4.A.1 Placebo

Figure 4.A.1: Placebo Test



Note: This figure depicts the Monte Carlo sampling distribution of Post DM $_t$ $\times$ DM Shock $_d$. We randomly permute the 7 categories of DM Shock $_d$ and run specification (1) Panel SATP in Table 4.4 $1,000$ times.

4.A.2 Measurement Errors

Table 4.A.1: Measurement Errors in the Demonetization Shock

	Cumulative Violent Incidents			
	(1)	(2)	(3)	(4)
Post DM $_t$ × DM Shock $_d$	-0.160***			
Baseline $[1, 7]$	(0.032)			
Post DM $_t$ × DM Shock $_d$		-0.875***		
Binary $0 = [1; 3], 1 = [4; 7]$		(0.125)		
Post DM $_t$ × DM Shock $_d$			-0.566***	
Binary $0 = [1; 4], 1 = [5; 7]$			(0.113)	
Post DM $_t$ × DM Shock $_d$				-0.130**
Categorical $[1, 5]$				(0.052)
Post DM $_t$ × Rainfall shock $_{dm}$	✓	✓	✓	✓
Fixed Effects	d,st	d,st	d,st	d,st
Observations	68,103	68,103	68,103	68,103
R-squared	0.953	0.954	0.953	0.953

Note: The table presents ordinary least squares (OLS) estimates for the specification (1), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily violent incidents starting on January 1, 2010, from SATP data. State × day and District fixed effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

4.A.3 Spatial Spillovers

Table 4.A.2: Spatial Lags

	Cumulative Violent Incidents				
	(1)	(2)	(3)	(4)	(5)
Post DM $_t$ $\times$ DM Shock $_d$	-0.160***	-0.176***	-0.182***	-0.147***	-0.176***
	(0.032)	(0.033)	(0.033)	(0.030)	(0.033)
Post DM $_t$ $\times$ DM Shock $_d$ within 1,000 km		-0.397***			
		(0.058)			
Post DM $_t$ $\times$ DM Shock $_d$ within 500 km			-4.648***		
			(0.476)		
Post DM $_t$ $\times$ DM Shock $_d$ within 250 km				0.283**	
				(0.121)	
Post DM $_t$ $\times$ DM Shock $_d$ within 100 km					-0.397***
					(0.058)
Post DM $_t$ $\times$ Rainfall shock $_{dm}$	✓	✓	✓	✓	✓
Fixed Effects	d,st	d,st	d,st	d,st	d,st
Observations	68,103	65,294	68,103	68,103	65,294
R-squared	0.953	0.954	0.957	0.953	0.954

Note: The table presents ordinary least squares (OLS) estimates for the specification (1), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily violent incidents starting on January 1, 2010, from SATP data. State $\times$ day and District fixed effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

Appendix 4.B Additional Robustness Analysis

4.B.1 Mineral Resources - Alternative Specifications

In Table 4.B.1, we perform various robustness checks of our results in Table 4.6 column 1 by varying the construction or source of data of our variable of interest, the mineral resources. In column 1, we replicate our baseline results, which uses the district-level normalized international value of production of minerals in 2015. In column 2, we use the same measure but with a different transformation, i.e. the natural logarithm of the international value of production in 2015. In this case, the signs of both our coefficients of interest remains similar, however the post-policy general impact is magnified. We believe that this is due to the exclusion of districts without any mineral production. In column 3, we follow the approach in Berman et al. (2017) by interacting an indicator for the presence of a mineral with its international price, rather than using the exact production level. Our post-policy shock coefficient remain stable, however, the triple interaction is statistically insignificant. In column 4, we only use the mineral for which we have World Bank data on their international price, relying on 10 minerals instead of 15. Results are unchanged from our baseline. Column 5 uses a different data sources, the number of leasehold deposits (a subset of the total mineral production). The difference is that in this case, we have the exact number mines, however, this is a subset since it does not include freehold deposits, neither the production of coal, petroleum and natural gas. Results remain stable. Finally, column 6 exploits the number of large-scale mines per district for the year 2012 from the Raw Material Data. Our post-policy shock coefficient is stable, however, there is not mitigation impact from large-scale mines, which might be more difficult to extort due to higher security means. Another possibility is that the results might be noisy due to the dated observations.

Table 4.B.1: Alternative Specifications - Mineral Resources

	Cumulative Surrenders					
	(1)	(2)	(3)	(4)	(5)	(6)
Post DM $_t$ × DM Shock $_d$	9.32***	36.51***	9.09***	9.32***	10.21***	8.75***
	(1.94)	(8.94)	(1.90)	(1.94)	(2.06)	(1.76)
Post DM $_t$ × DM Shock $_d$ × Mineral Resources normalized production value	-48.58**					
	(23.61)					
Post DM $_t$ × DM Shock $_d$ × Mineral Resources ln(production value)		-0.72***				
		(0.24)				
Post DM $_t$ × DM Shock $_d$ × Mineral Resources Berman et al. (2017)			-0.00			
			(0.00)			
Post DM $_t$ × DM Shock $_d$ × Mineral Resources World Bank Data				-48.51**		
				(23.60)		
Post DM $_t$ × DM Shock $_d$ × Mineral Resources Public Leases					-0.09***	
					(0.02)	
Post DM $_t$ × DM Shock $_d$ × Mineral Resources Large-scale mines (#)						-0.06
						(0.15)
Post DM $_t$ × Fundings ^i_d	✓	✓	✓	✓	✓	✓
Post DM $_t$ × Rainfall shock $_{dm}$	✓	✓	✓	✓	✓	✓
Fixed Effects	d,st	d,st	d,st	d,st	d,st	d,st
Observations	68,103	41,923	68,103	68,103	68,103	68,103
R-squared	0.95	0.95	0.95	0.95	0.95	0.95

Note: The table presents ordinary least squares (OLS) estimates for the specification (2), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily surrenders starting on March 24, 2006, from SATP data. State × day and District fixed effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

4.B.2 Public Works - Alternative Specifications

Table 4.B.2 offers two alternative specifications of our results in Table 4.6 column 2 by varying the variable of interest, public works. Our coefficient of interest, Post DM $_t$ $\times$ DM Shock $_d$, remains stable throughout the checks. In column 1, we replicate our baseline results, which uses the daily average number of roads in construction in 2015. In column 2, we concentrate on the daily average number of companies in 2015. However, our triple interaction coefficient looses significance. This could also show the channel through which public works are extorted. As explained by Ramana (2018), "The amount extorted is determined by the nature of the work and its cost", rather than the number of companies. The last column uses the daily average expenditure in 2015, and display similar results, in line with our expectations.

Table 4.B.2: Alternative Specifications - Public Works

	Cumulative Surrenders		
	(1)	(2)	(3)
Post DM $_t$ $\times$ DM Shock $_d$	12.59***	10.91***	14.57***
	(2.61)	(2.42)	(2.93)
Post DM $_t$ $\times$ DM Shock $_d$ $\times$ Public Works Roads	-0.07***		
	(0.02)		
Post DM $_t$ $\times$ DM Shock $_d$ $\times$ Public Works Companies		-0.05	
		(0.04)	
Post DM $_t$ $\times$ DM Shock $_d$ $\times$ Public Works Expenditure			-0.39***
			(0.09)
Post DM $_t$ $\times$ Fundings ^i_d	✓	✓	✓
Post DM $_t$ $\times$ Rainfall shock $_{dm}$	✓	✓	✓
Fixed Effects	d,st	d,st	d,st
Observations	68,103	68,103	68,103
R-squared	0.95	0.95	0.95

Note: The table presents ordinary least squares (OLS) estimates for the specification (2), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily surrenders starting on March 24, 2006, from SATP data. State $\times$ day and District fixed effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

4.B.3 Forest Industry - Alternative Specifications

Table 4.B.3 offers one alternative specification of our results in Table 4.6 column 3 by varying the variable of interest, forest industry. In column 1, we replicate our baseline results, which uses the number of manufacturing firms related to the forest industry between 1998 and 2008. In column 2, we use the percentage of forest cover in 2015 as a proxy of the production of forest resources. However, results are not stable across columns. One possibility is that the forest cover measure is not narrow enough and includes confounding factors. For instance, Maoists tend to hide in forest areas and it could be argue that they are differentially affected by the demonetization due to better secrecy around their location. Second, the forest industry measure is negatively correlated with forest cover, which further explains the different results.

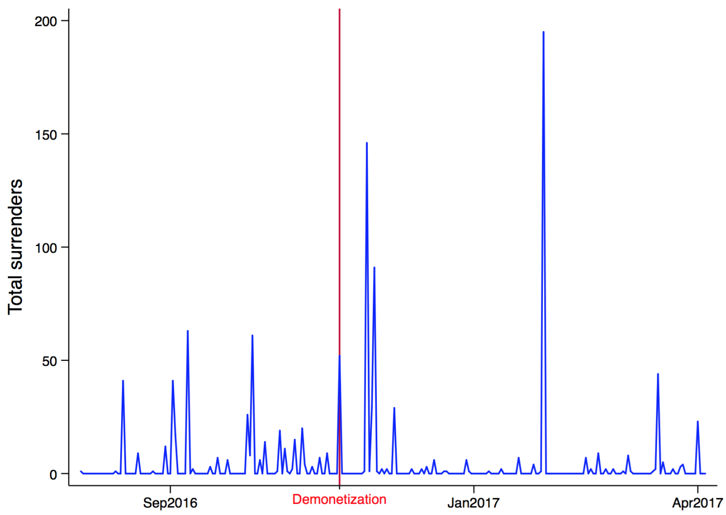
Table 4.B.3: Alternative Specifications - Forest Industry

	Cumulative Surrenders	
	(1)	(2)
Post DM $_t$ × DM Shock $_d$	9.47***	-2.30***
	(1.99)	(0.71)
Post DM $_t$ × DM Shock $_d$ × Forest Industry	-0.04***	
	(0.01)	
Post DM $_t$ × DM Shock $_d$ × Forest Cover		0.59***
		(0.10)
Post DM $_t$ × Fundings ^i_d	✓	✓
Post DM $_t$ × Rainfall shock $_{dm}$	✓	✓
Fixed Effects	d,st	d,st
Observations	68,103	68,103
R-squared	0.95	0.95

Note: The table presents ordinary least squares (OLS) estimates for the specification (2), where the unit of observation is a day t in a district d. The dependent variable is the cumulative of daily surrenders starting on March 24, 2006, from SATP data. State × day and District fixed effects are present in all columns, as well as the interaction between the post-policy indicator and rainfall shock. Standard errors adjusted for spatial (500 km) and serial correlation (30 days) in parentheses following Colella et al. (2019). *** p<0.01, ** p<0.05, * p<0.1.

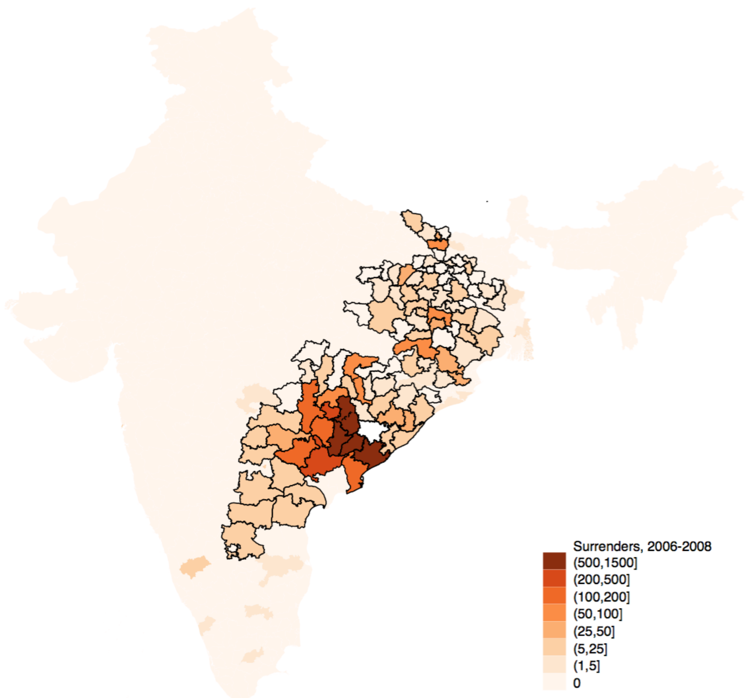
Appendix 4.C Additional Figures

Figure 4.C.1: Daily Surrenders



Note: This figure plots the number of daily surrenders around the implementation of the demonetization, which is depicted with the red line. Data is from SATP.

Figure 4.C.2: Surrenders in conflict-affected districts, 2006-2018



Note: This map shows the geographic distribution and magnitude of Maoist-related surrenders between 2006-2018. The dark borders display the 102 Maoist-affected districts in 2015, following the Ministry of Home Affairs list.

Appendix 4.D List of Districts affected by the Conflict

In our analysis, we follow the 2015 list of 106 districts in 10 Left Wing Extremism affected States from the Ministry of Home Affairs, which is based on their violence profile and other parameters. These States are covered under the Security Related Expenditure Scheme, which allow them to receive reimbursement for counter-insurgency measures. Table 4.D.1 gives the exhaustive list. Furthermore, districts colored in blue are considered as severely affected districts. As rainfall information is not recorded in certain districts and some of them were either split or merged between 2013 and 2017, we merged them to create a balanced panel dataset of 102 districts. Districts in *italic* are districts for which the currency shock information is missing.

Table 4.D.1: Conflict-affected Districts

States	Districts
Andhra Pradesh	Anantapur,East Godavari, Guntur, Kurnool, Prakasam, Srikakulam, Visakhapatnam, Vizianagaram
Bihar	*Arwal*, Aurangabad, Banka, Begusarai, Bhojpur, Gaya, Jamui, Jehanabad, Kaimur Bhabua, Khagaria, Lakhisarai, Munger, Muzaffarpur, Nalanda, Nawada, Pashchim Champaran, Patna, Purba Champaran, Rohtas, *Sheohar*, Sitamarhi, Vaishali
Chhattisgarh	Bastar, Bijapur, Dakshin Bastar Dantewada, Dhamtari, Durg, Jashpur, Koriya, Mahasamund, Narayanpur, Raipur, Rajnandgaon, Surguja, Uttar Bastar Kanker
Jharkhand	Bokaro, Chatra, Deoghar, Dhanbad, Dumka, Garhwa, Giridih, Gumla, Hazaribagh, *Khunti*, Kodarma, *Latehar*, *Lohardaga*, Pakur, Palamu, Pashchimi Singhbhum, Purbi Singhbhum, *Ramgarh*, Ranchi, *Saraikela-Kharsawan*, *Simdega*
Madhya Pradesh	Balaghat
Maharashtra	Chandrapur, Gadchiroli, Gondiya
Orissa	Balangir, Bargarh, Debagarh, Dhenkanal, Gajapati, Ganjam, Jajapur, Kalahandi, Kandhamal, Kendujhar, Koraput, Malkangiri, Mayurbhanj, Nabarangapur, Nayagarh, Nuapada, Rayagada, Sambalpur, Sundargarh
Telangana	Adilabad, Karimnagar, Khammam, Mahabubnagar, Medak, Nalgonda, Nizamabad, Warangal
Uttar Pradesh	Chandauli, Mirzapur, Sonbhadra
West Bengal	Bankura, Birbhum, Paschim Medinipur, Puruliya

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